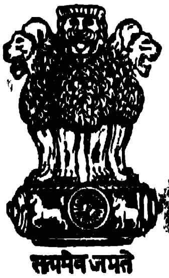

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THE
SCIENTIFIC MONTHLY

JANUARY 1948

EDMUND WARE SINNOTT

PRESIDENT OF THE A.A.A.S. FOR 1948

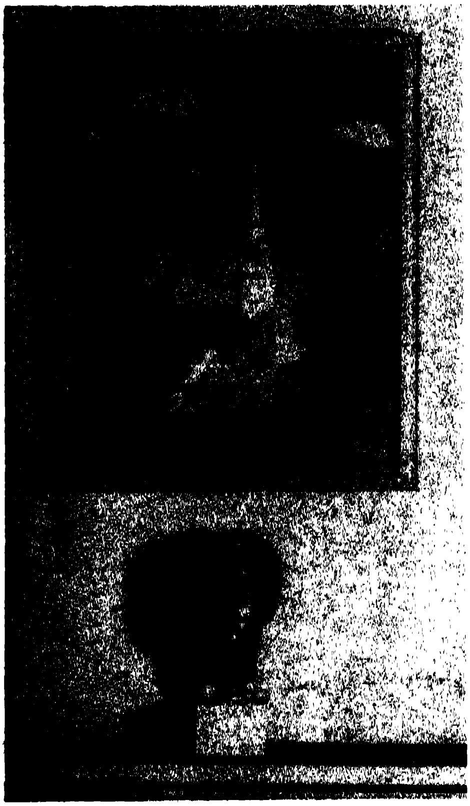
ALBERT F. BLAKESLEE
Genetics Experiment Station, Smith College; President, A.A.A.S., 1940.

SOMEWHAT over thirty-two years ago I was asked to recommend my successor for the position of professor of botany and genetics in what was then the Connecticut Agricultural College at Storrs. I recommended Edmund Sinnott. In so doing I told President Beach that I could appropriately use the words of John the Baptist in prophesying that "He it is, who coming after me is preferred before me, whose shoe's latchet I am not worthy to unloose." I trust John had an equal satisfaction in seeing his prophecy come true. In my own case the prophecy was not a wild gamble since, as an instructor in one of Professor Thaxter's classes in cryptogamic botany in 1907, I had known Sinnott as a brilliant young student and had followed his subsequent activities with a teacher's interest.

A biographer often looks to the ancestry of his subject and the surroundings to which he has been exposed for an explanation of achievement. Any close correlation is usually difficult to discover, but in the present case it seems safe to conclude that fate dealt a favorable hand of genetic cards and that early environmental influences, which are sometimes difficult to separate from heredity, also had a beneficent influence on the resultant product.

The father of Edmund Ware Sinnott was Charles P. Sinnott, whose ancestors came from Maine; Charles Sinnott's paternal grandfather was Irish, his grandmother French. The rest of the ancestry appears to be pure Yankee. Sinnott's mother's people (the Smiths) came from the Connecticut Valley and were descended from the Reverend Henry Smith, first minister of Wethersfield, Connecticut. His father graduated from the Bridgewater (Massachusetts) State Normal School and from Harvard. He taught various sciences, especially geology and geography, in a number of normal schools, but the longest period of teaching (1897–1936) was at Bridgewater.

Edmund Sinnott was born at Cambridge, Massachusetts, February 5, 1888. The family soon moved to Milwaukee, Wisconsin, and lived there until 1897, when they returned to Massachusetts. Here he was educated in the public schools of Bridgewater and then at Harvard, taking an A.B. there in 1908 and a Ph.D. in 1913. He was always interested in natural history even in his younger days. After he came under the influence at Harvard of Jeffrey, Fernald, Thaxter, Parker, and Castle, he definitely determined to be a biologist and was headed at first for zoology. Later, however, he became particularly interested in plant morphology under Jeffrey, and went on to graduate



ART BY E. W. SINNOTT

THE CONGREGATIONAL CHURCH AT FARMINGTON, CONNECTICUT, AND THE HEAD OF CLARA SINNOTT.

work with him. In 1910–11, in company with A. J. Eames (now of Cornell), he spent a year in Australasia as a Sheldon Fellow, collecting plant material, particularly of the conifers. This was in the good old days before World War I, when travel was cheap and uncomplicated. This experience in a region where the plant life was so entirely different from our own was very stimulating to him.

Under Jeffrey's guidance he was brought up in the strict tradition of comparative anatomy, the major goal of which was a reconstruction of the phylogenetic tree of the plant kingdom. His dissertation was on the reproductive structures of the Podocarpineae. He had published a number of papers before this, however, all on the comparative anatomy of the higher plants, for one of which he received the Bowdoin Medal from Harvard in 1910. For two years after the doctorate he worked at the Bussey Institution with I. W. Bailey. The two had set themselves the grandiose goal of going through the angiosperms and working out the phylogeny of all their families on the basis of structure; they published a number of papers together in this field. There was no opportunity to complete this plan, and in 1915 Sinnott left to become professor of botany and genetics at Storrs. Here his interests turned from the purely descriptive and comparative aspects of plant form to the developmental factors—genetic and environmental—that determine it. He believed that the cucurbits provide in their fruits excellent material for such a study and while at Storrs he investigated the inheritance of shape and size, the cellular basis of these characters, and the effects upon them of various external factors.

He has continued to use the cucurbits as the main object of his personal investigations, a recent phase of which has been a study of how cellular behavior determines the size and form of fruits of normal and polyploid plants. He has, however, employed other plants, especially in cooperative studies of the broader problems of differentiation and growth. Thus, he and I collaborated in studies of the genetics of *Datura* and worked out the anatomical correlates of the various chromosomal types. Greatly to our surprise, Sinnott was able to recognize these types by differences in anatomical structure seen in freehand sections of flower stalks with apparently the same ease with which we could recognize them by external appearance.

In 1928 he went to Columbia as professor of botany and chairman of the department at Barnard College; he was also on the graduate faculty of the University. He remained for twelve years, the last year leaving Barnard and being transferred entirely to Columbia. Dr. L. C. Dunn and Sinnott went to the University from Storrs the same year and there continued their earlier association and cooperation. While at Columbia Dr. Sinnott served for three years as president of the Torrey Botanical Club and still serves on the Boards of the New York Botanical Garden and the Boyce Thompson Institute for Plant Research.

EDMUND WARE SINNOTT

PRESIDENT, AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, 1948.

In 1940 he went to Yale as Sterling professor of botany, chairman of the department, and director of the Marsh Botanical Garden. This was when some in Yale held the old-time belief that botany consists mainly in identifying and naming wild flowers, and doubted the propriety of developing the subject as a major university department. Sinnott, however, put new life and meaning into the Department of Botany and in 1945 was chosen director of the reorganized Sheffield Scientific School. The Sheffield centennial, celebrated this fall in New Haven, was a significant scientific event. Yale University has strongly supported Sinnott's work, and he has been able to gather about him an active and enthusiastic group of staff members and students, some of whom have come to his department in preference to financially better offers elsewhere. One cannot visit the Yale Department of

Botany at the present time without sensing the fine spirit of joyous endeavor and happy comradeship under Sinnott's leadership.

In 1916 Sinnott married Mabel Haskell Shaw. They have three children: Edmund Ware, Jr., Harvard, 1938; Mildred Shaw, Swarthmore, 1941; and Clara Richardson, Connecticut College, 1945. During the summer the Sinnotts live in a delightful old farmhouse at Woodbury, Connecticut, with adequate land for the growth of squashes, where he carries on most of his experimental work.

A thing that surprises one regarding the subject of our sketch is his versatility—his excellence in so many characteristics. That he is a good teacher is attested by those who have had the privilege of being students in his classes. It is also shown by the successful textbooks he has written: *Botany: Principles and Problems,* now in its fourth edition; and *Principles of Genetics* (with L. C. Dunn), now in its third edition. His standing and contributions as a plant scientist have brought about his election to the leading honorary societies in this country to which he has been eligible: Phi Beta Kappa; Sigma Xi; National Academy of Sciences; American Philosophical Society; American Academy of Arts and Sciences. For the same reasons he has been elected president of the following societies: Botanical Society of America; Torrey Botanical Club; American Society of Naturalists; Society for Study of Development and Growth; chairman of Section G (Botany) of the American Association for the Advancement of Science; and now president of our Association. He is a facile writer and a forceful and fluent speaker.

Good judgment in problems of scientific organization and tact in human relations are responsible for his being much sought after on committees; for administrative positions, such as trustee of *Biological Abstracts,* New York Botanical Garden, Boyce Thompson Institute for Plant Research, Associated Universities, and membership on the National Research Council and its Committee on Growth, which helps in the research problems of the American Cancer Society; and for advice in other capacities. He has not shunned civic responsibility in scientific affairs when the labor much exceeded the honor involved. Thus, he served several years as editor of the *American Journal of Botany* and was an effective member of the committee that reorganized the *Journal.*

Many of the illustrations in his textbooks were made by Sinnott himself, but I did not realize the extent of his artistic ability until I saw over the mantel in his home a painting he had made of the Congregational Church at Farmington, Connecticut. Early New England meeting-house and church architecture is his hobby, and he has visited and photographed most of these buildings erected in the eighteenth and early nineteenth centuries. His art expression is not confined to oil painting. He has also done some creditable pieces of sculpture. One of these, of his younger daughter, Clara, is shown in the accompanying photograph, below his painting of the Farmington church.

That the reader may realize there are some limitations to the abilities of the subject of this sketch, it must be stated there is good evidence that he never aquired proficiency in music either as a performer or as a composer, that he never was interested in writing poetry, and that he never excelled in athletics.

Sinnott has been an inspiring leader among the students and associates in his group, with a contagious enthusiasm which, in some of his recommendations of students, may perhaps tend to be somewhat unduly weighted with kindness. In research he has shown judgment as to what problems are important, and skill in organizing methods for their attack. He has shown rare ability in cooperating with others in research. His friends know him for his loyalty and for his sparkling personality, bubbling over with kindly humor.

PHOTOGRAPHY IN SCIENCE

ALEXANDER J. WEDDERBURN
Associate Curator, Division of Graphic Arts, Smithsonian Institution.

RESPONSE of scientists and scientific organizations to the First International Photography in Science competition was most gratifying to its sponsors, the Smithsonian Institution and THE SCIENTIFIC MONTHLY. A total of 361 entries were received from the United States, Canada, Mexico, and Ireland. In the black-and-white division, 243 prints were submitted. A total of 118 prints and transparencies were received for consideration in the color division. It is interesting to note that monochrome remains the more popular of the two media of expression in the field of scientific photography.

Photography is becoming more and more a tool of research. For various fields of investigation, ranging from astronomy to microscopic studies in biology and problems in physics involving almost infinitesimal time intervals, it has been necessary to develop different types of photographic apparatus and methods. In most sciences, where the validity of findings depends on extreme precision and accuracy, these methods are quite ingenious and have advanced far toward perfection.

The specific aim of the competition was to develop and extend the use of photography as a basic research tool as well as to aid in the dissemination of information about the various photo-science techniques.

The first, second, and third place winners in the color division were, respectively: Dr. Thomas C. Poulter, associate director, Armour Research Foundation, Chicago, Illinois; Mr. Albert M. Stover, head, Development Section, Plastics and Chemicals Division, The Glenn L. Martin Company, Baltimore, Maryland; and Mr. Ralph O. Marts, Forest Products Laboratory, Forest Service, U.S.D.A., Madison, Wisconsin.

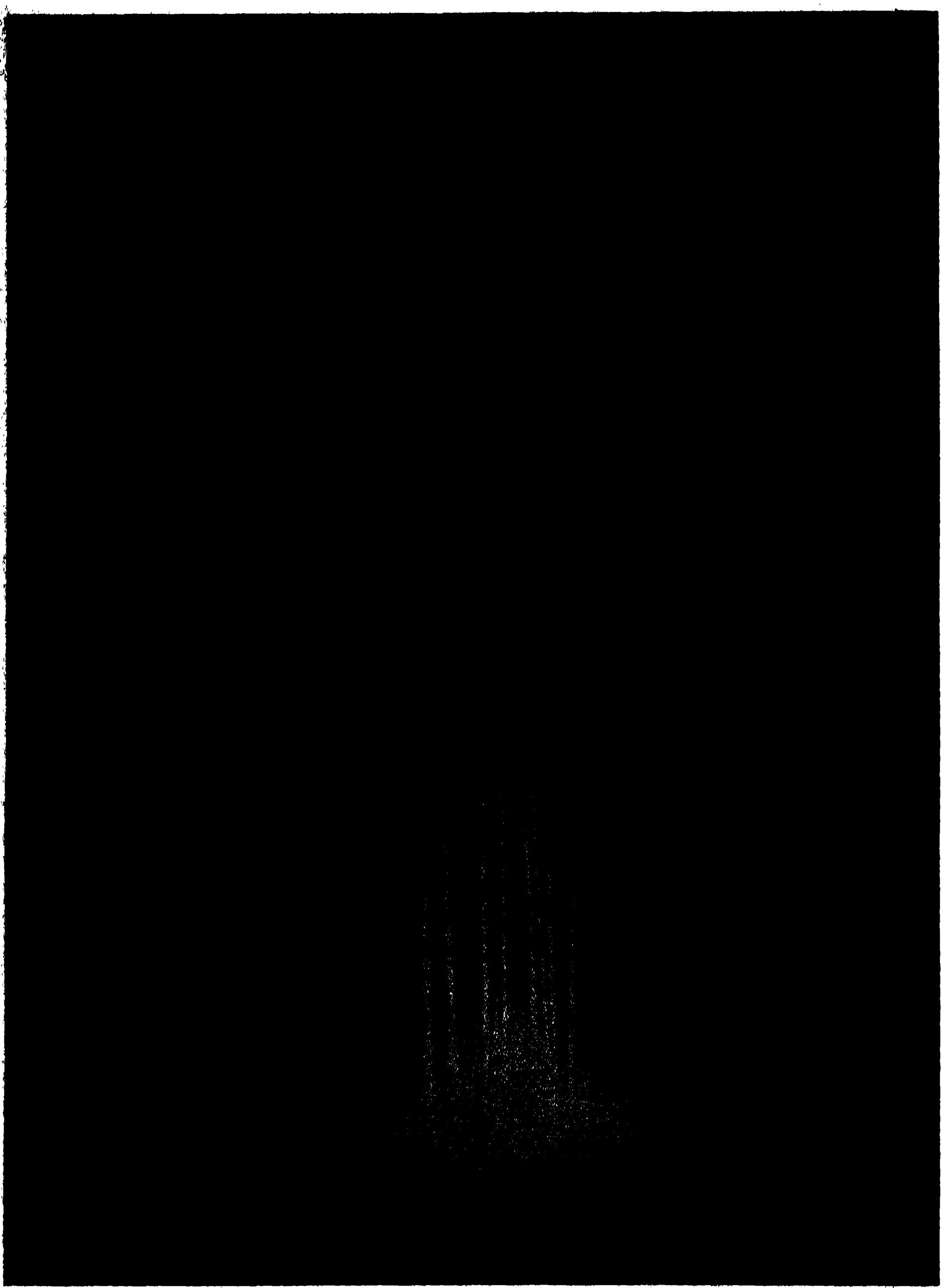
Winners in the black-and-white division were: Dr. Edwin R. Willis, Biological Laboratories, Philadelphia Quartermaster Depot, first place; Mr. V. P. Hollis, University of Minnesota, St. Paul, second place; and Mr. Harry U. Rhoads, Lambert Pharmacal Company, St. Louis, Missouri.

Members of the Judging Committee, who met October 27, 1947, were Dr. Ralph D. Bennett, technical director, Naval Ordinance Laboratory, White Oak, Maryland; Dr. K. M. Endicott, U.S. Public Health Service, Bethesda, Maryland; Mr. A. Aubrey Bodine, associate editor, *Camera Magazine;* and A. J. Wedderburn.

Entries were judged on the basis of initiative, originality, and results obtained through the use of photography. A further consideration was proficiency in the use of various photographic techniques. In view of the great number of entries that could not be removed from consideration on other grounds, final elimination was based on a critical analysis of technical excellence of presentation from the photographic standpoint. Conversely, certain of the best pictorial representations were rejected for failure to meet the requirement of originality of technique or that of unique or unusual results obtained in the particular field of science represented.

Among the techniques exemplified, many photomicrographs, electron micrographs, macrophotographs, and astrophotographs were outstanding for their technical excellence and their scientific, industrial, and military value as research tools. Sound photographic methods employed were evidenced by the excellent rendering of the correct tonal qualities in the resultant prints and color transparencies.

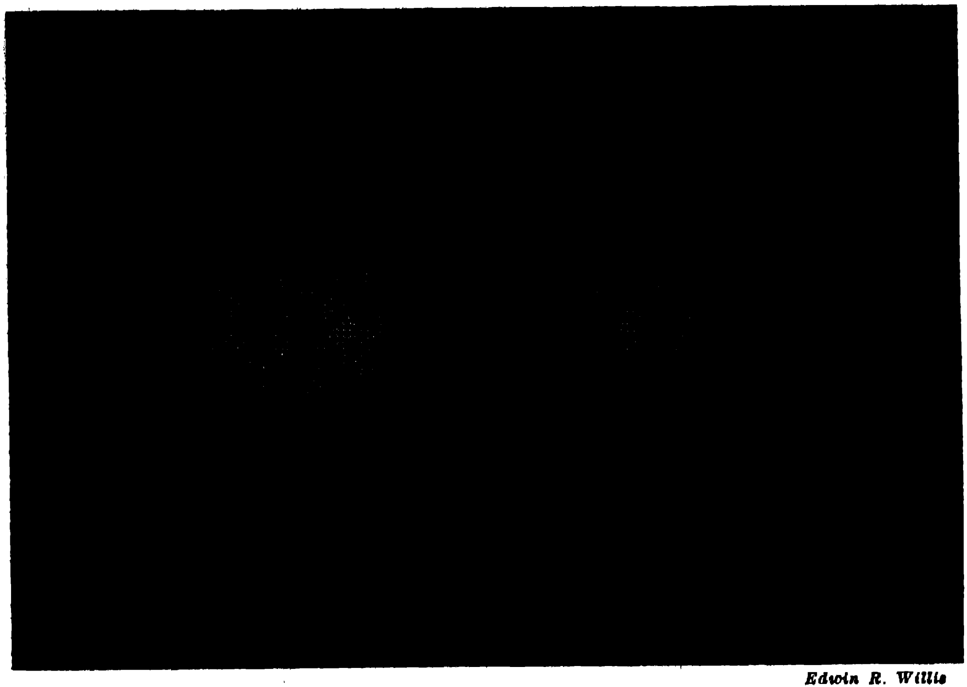
Perhaps the most significant aspect of the



Thomas C. Poulter

FIRST PLACE, COLOR DIVISION

REPRODUCED HERE IN BLACK AND WHITE, THIS PHOTOGRAPH BY DR. POULTER, ASSOCIATE DIRECTOR, ARMOUR RESEARCH FOUNDATION, SHOWS THE RESULTS OF A FIELD TEST TO DETERMINE THE SPACING NECESSARY TO PREVENT COUNTERMINING IN MONROE-EFFECT, ANTI-SUBMARINE BOMBS. THE JETS AVERAGE 250 FEET.



Edwin R. Willis

FIRST PLACE, BLACK-AND-WHITE DIVISION

DR. WILLIS, FORMERLY RESEARCH ASSOCIATE OF THE OHIO STATE UNIVERSITY RESEARCH FOUNDATION, UTILIZED PHOTOGRAPHY TO MAKE ACCURATE COUNTS OF MOSQUITOES RESPONDING TO ODORS IN HIS INSECT OLFACTOMETER. THIS PICTURE, ONE OF MANY TAKEN DURING THE COURSE OF HIS WORK IN THE DEPARTMENT OF ZOOLOGY AND ENTOMOLOGY, SHOWS THE MAJORITY OF THE INSECTS RESTING ON THE RIGHTHAND PORT OF THE OLFACTOMETER TO WHICH THEY WERE ATTRACTED BY ODOR FROM A MAN'S ARM. HALF SIZE.

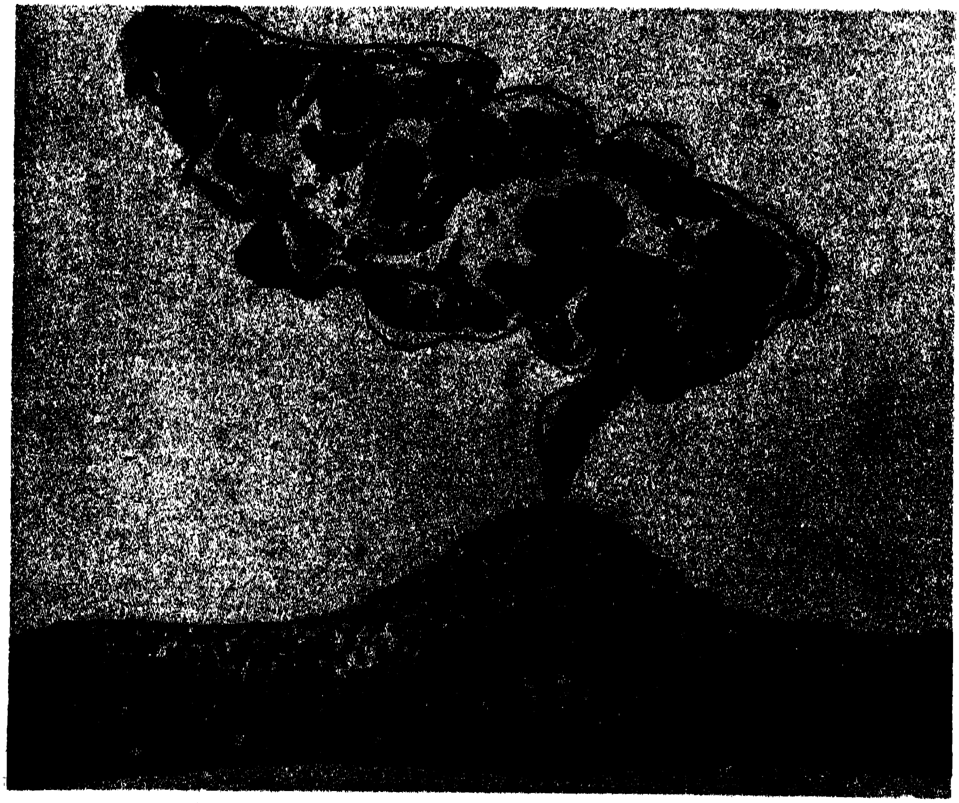
competition was the almost universal use of conventional means of photographic recording. With few exceptions, which will be mentioned later, the great bulk of the photographs were produced by means that have become recognized and commonly accepted as standard photographic procedure. The electron micrograph, although comparatively new, must be classed with the above group.

Dr. Poulter's winning print in the color division is an appropriate example of the application of photography to military usage. The picture graphically illustrates the results of a test to determine the spacing necessary to prevent countermining in Monroe-effect, antisubmarine bombs. Placed too closely together, these bombs will not produce jets. Tests were conducted to determine how closely bombs could be spaced and still produce jets (visible in picture). The jets illustrated are approximately 250 feet in height and capable of penetrating 23 inches of solid armor plate. The color photograph was made at a distance of about a quarter of a mile from the detonation point.

Mr. Stover's second-place entry in the color division, which unfortunately was not suitable for reproduction in these pages, is a series of photographs taken by means of polarized light to show the stresses developed in a plastic film being subjected to a "trapezoid tear test." The increasing area under stress clearly shows in the pictures that tensile strength rather than tear resistance is being measured, thus ruling the test out for extensible plastic film. According to Mr. Stover, the test, which is standard for measuring the strength of fabric, had been proposed as a measure of the tear strength of unsupported plastic film.

The third-place-winning 35-mm. color transparency of Mr. Marts, which is also unsuitable for monochrome reproduction, is a fluorescence photomicrograph entitled "Vascular Bundle in Fern."

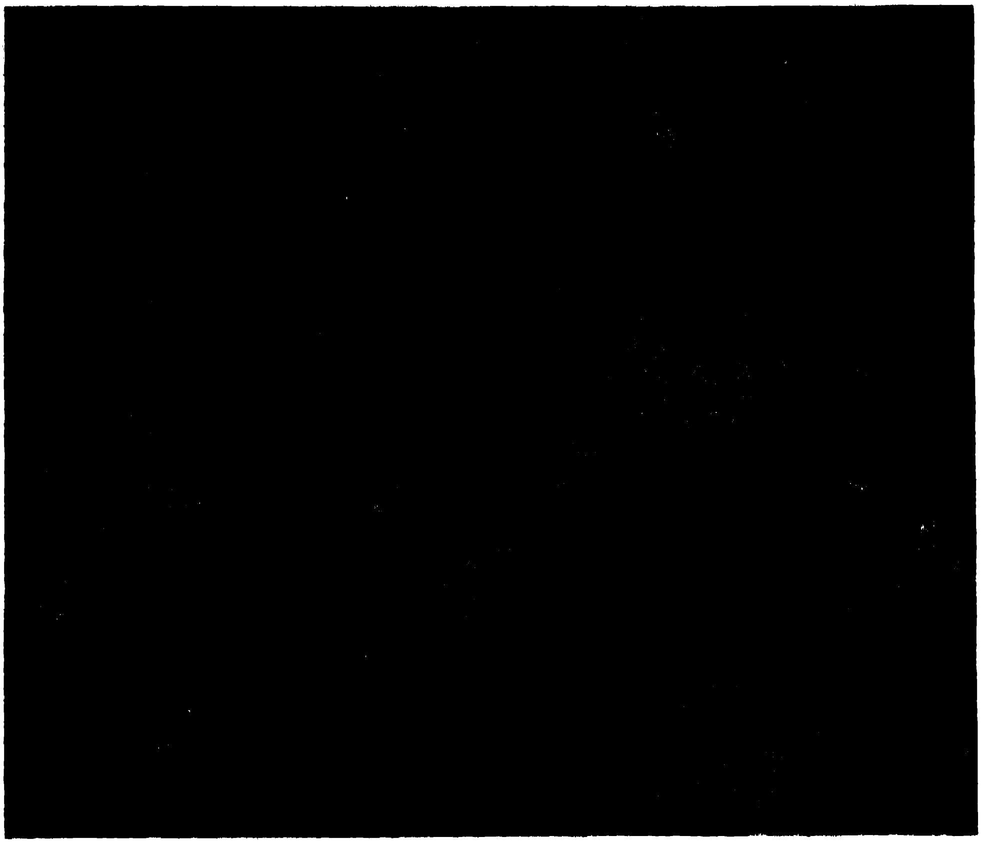
Dr. Willis' application of photography to medical research is an interesting example of how accurate data were obtained that were not otherwise available by direct visual observation because of the rapid movement of the specimens under observation. Females of the yellow-fever mosquito (*Aëdes aegypti*) are shown responding to the odor of a man's arm at the righthand port of an insect olfactometer. With the exception of the test odor in the righthand air stream, there was a close correspondence of other stimuli (temperature, relative humidity, rate of air flow, and extraneous odors) in both air streams. This photograph was the first of ten, exposed at one-minute intervals, that comprise one test in an investigation of the olfactory responses of female mosquitoes. It is of interest to note that Dr. Willis used three 25-watt Mazda lamps that, symmetrically arranged behind the two translucent funnels that formed the olfactometer ports, silhouetted the mosquitoes on the side of the wire-screen cage at the instant the exposure



V. P. Hollis

SECOND PLACE, BLACK-AND-WHITE DIVISION

THIS PHOTOMICROGRAPH IS REMARKABLE BECAUSE IT SHOWS A COMPLETE LONGITUDINAL SECTION OF A PLANT LOUSE, FEEDING UPON A LEAF. MR. HOLLIS, UNIVERSITY OF MINNESOTA, NOT ONLY PICTURES THE APHID ABOVE AND THE LEAF BELOW, BUT SHOWS THE APHID'S BEAK INSERTED INTO LEAF TISSUE.



H. U. Rhoads

THIRD PLACE, BLACK-AND-WHITE DIVISION

MR. RHOADS, LAMBERT PHARMACAL CO., ST. LOUIS, PRESENTS AN ELECTRON MICROGRAPH OF HUMAN TOOTH ENAMEL SHOWING THE STRUCTURAL CONFIGURATION OF THE ENAMEL RODS AND INTERROD SUBSTANCE.

was made. In the intervals between the successive exposures, the mosquitoes were in complete darkness. This method of lighting was chosen to avoid establishing a counterattractant of heat in any part of the olfactometer other than at the ports.

The second-place photomicrograph of Mr. Hollis in the black-and-white division is illustrative of the combination of fine techniques. The photographic excellence of the print is apparent. The preparation of the longitudinal section of the plant louse is skillfully done and is unusual in that it portrays the whole aphid with its beak penetrating the leaf structure.

The electron micrograph (magnification, 23,400 ×) by Mr. Rhoads of human tooth enamel shows the structural configuration of the enamel rods and interrod substance. The view illustrated was made at an angle to the axis of the enamel rod, the tooth being dulled with Sturge chalk. Working from the print, the replica is prepared by the polystyrene silica technique of Heidenreich.

Aside from the conventional photographic processes used by contributors, there were several interesting adaptations of known techniques to specific needs in given situations, calling for originality of method to obtain hitherto unobtainable results. Additionally, processes were advanced by which the considerable expense of negative use and preparation was eliminated. In the field of photographic research, an entry was submitted in the color division as possessing basic potentialities of a third dimensional effect through a subtractive method and means of optical image modulation.

In the category of adapted photographic techniques was a print from Dr. Harold W. Manter, Zoology Department, University of Nebraska. This photograph of the scolex and anterior segments of a tapeworm, *Ptychobothrium* species, from the intestine of a flying fish, *Cypselurus callopterus,* was made without the aid of a camera. The stained and mounted specimen on a microscope slide was projected through a Spencer microfilm reader directly on the printing paper. Although the resultant image was actually a negative one, the proper reversal was obtained in the final print by means of staining the specimen carmine red, resulting in a white subject on a black background. In this instance, white being the color of the living animal, the negative print was more natural than the specimen on the slide.

A method for the rapid recording of plating results was illustrated by Mr. Kenneth A. Wagner, instructor in botany, the University of Tennessee. The permanent records of Petri plates may be made cheaply and quickly by direct contact printing. The photogram is accomplished by placing the plates on a sheet of photographic printing paper, the exposure being made by an overhead light. The expense and time of making the usual film are thus eliminated. It should be noted that although the resulting print is actually a paper negative, positive results are obtained, the clear areas of the plate allowing passage of a greater percentage of illumination than those areas covered by the specimens. Thus the colonies that appear white on the Petri plate reproduce white on the less-exposed portions of the paper.

Of interest to entomologists, Mr. H. Lou Gibson (Honorable Mention) provided a four-picture illustration of his method of photographing insects having a true metallic silver coloration. The specimen used was *Plusiotis gloriosa,* a rare scarab native to Arizona. The bands on the elytra are rough-surfaced "mirrors," and the elytra are pale-green, enamel-like surfaces. Methods satisfactory for photographing other insects are unsuccessful with this beetle. The difficulty experienced in photographing this insect results from the recording of specular high lights and reflections of surrounding objects in the elytra. The reflection characteristics are shown in one picture where a dot-dot-dash symbol on a small card was picked up strongly in one of the silver bands and noticeably in the elytrum. The problem was solved by making a cage of lead foil around the insect, throwing light into the case, and thereby photographing the foil reflections in the bands. A "set-up" of pencils and tape were used for casting shadows on the far legs and on the near side of a supporting piece of cactus to minimize the more intense high lights. Small mirrors were used for fill-in light, and back lighting was accomplished by means of a photoflood lamp.

A measure of progress in the pursuit of three-dimensional photography without the use of viewing devices, lenticulated film, laminated screens, or other impedimenta was claimed by Mr. Ivan M. Terwilliger, of Pictures Inspacian, Santa Monica, California. His color-slide entries show a photographic sequence of four pictures (frames) exposed consecutively at standard sound motion-picture speed of 24 frames per second. The sequential frames demonstrate a modulation cycle within the picture, by which each frame is individually different, photographically and pictorially, from any other of the adjacent three. Each frame of the sequential cycle has its own individual depth of focus that is slightly different from that of any one of the other three of the same sequence. Each picture of the sequential cycle has a double focus, in which a major portion of the picture's density is sharp on the foreground images, and a minor portion of the picture's density is sharp on the background images. The ratio between the foreground-sharp-density and the background-sharp-density is different for each picture of the sequence, the total density of each picture being the same as that of the others. Observation of the slides reveals that in each frame of the sequence the pictured background has a degree of sharpness dissimilar to the sharpness of the corresponding backgrounds in the other frames.

In connection with three-dimensional photography, it is of interest to note that another system, designated as the "Trivision" process, will be demonstrated soon at the Naval Photographic Center, Anacostia, Washington, D. C., by its inventor, Mr. Douglas F. Winnek, of Mount Vernon, New York. Mr. Winnek is now putting the finishing touches on his equipment at the Navy's Aeronautical Photographic Experimental Laboratory at the Philadelphia Navy Yard. No special seeing devices are used to obtain the illusion of depth in the Trivision process. Tiny, almost invisible ridges on the film are the basis of the invention. In the new system, ridges, or lenticulations on a transparent picture surface are applied to the film. These ridges serve as thousands of tiny recording surfaces accepting many pictures when the film is exposed in the camera. The final print is a composite of many pictures.

Navy Trivision equipment developed by Mr. Winnek includes a press for putting the ridges on film surfaces, single-lens camera, and an enlarger printer for performing a critical movement of the film during enlargement or reduction of a picture.

By use of the Winnek process, it has been predicted that three-dimension X-rays will be used to enable a surgeon to predetermine the necessary depth of incision before an operation.

The study of radioactive substances in relation to their reaction on forms of animal life was represented by a series of color prints by Miss Jane K. Glaser (Honorable Mention). Reaction of a rat to plutonium was indicated in the picture by a typical localized graying of the hair at the site of the deposition. In another print tumors of the intestine were visible following ingestion of radioactive yttrium. A third in the color series represented rat lung tumors following the inhalation of radioactive cerium.

An informative photomicrograph by Mr. Albert J. Oliver, of the Radiation Laboratory, University of California, Berkeley, depicted a nuclear event recorded directly in a photographic emulsion. The emulsion had been bombarded in the 184-inch Berkeley cyclotron with a beam of alpha particles having an energy of 80 MeV. The plane of the emulsion was tangent to the beam. The photographic plate, a portion of which was the subject of the entry, was not exposed to light. The silver grains were deposited by ionization caused by high-energy charged particles. The tracks of the charged particles were formed directly in the emulsion. In the photographed area, one of the alpha particles passed into the edge of the emulsion, and its path is easily traceable by the track of developed grains.

In the field of medical research, Mrs. Muriel C. MacDowell, of the Department of Pathology, Long Island College of Medicine, Brooklyn, entered four composite photomicrographs from which detailed study of entire kidney units is possible. The MacDowell entry is one of the best examples of the close relationship of science and photography to the rapidly expanding visual-education field.

One of the fine composites was a spodogram showing distribution of mineral ash in the rat nephron following the administration of calciferol. A nephron (the structural and functional unit of the kidney) from a rat that was excreting large amounts of Ca in the urine following administration of the calciferol (Vitamin D) was isolated by microdissection under water from kidney tissue macerated in acid. After mounting on a slide, the specimen was incinerated at a temperature of 500° C. for one half hour, thus destroying all organic substances and leaving in its original structural pattern the nonvolatile oxides of the protoplasmic minerals Mg, Na, K, Li, and, in great excess as a result of the experimental procedure, Ca. Successive overlapping photomicrographs of 100× magnification were taken under constant conditions of illumination (dark field by means of cardioid condensor), exposure, and development. From the overlapping prints, the proper contiguous portions were cut and matched to form, when mounted, an exact composite photomicrograph of the entire kidney unit. In the final print, the varying content of mineral, mostly Ca, is clearly visible in the various portions of the nephron. Heavy deposits are seen in the proximal convolution (recorded by 15 exposures); in the ascending limb, which required 7 exposures, only traces are seen, which increase in the distal convolution to disappear in the branching collecting tubule.

Of the many entries submitted, a very small number were rejected because of inappropriate subject matter. The accepted photographs, which were shown at the Smithsonian Institution during November, will be part of the International Science Exhibition of the A.A.A.S. at Chicago, December 26–31.

From there the exhibit will move to the Buhl Planetarium in Pittsburgh; the next stop will be at the Cranbrook Institute of Science, Bloomfield Hills, Michigan. Requests for displaying these pictures at other institutions should be addressed to the editor of THE SCIENTIFIC MONTHLY.

The Second International Photography in Science competition is scheduled for exhibition in September 1948 in the U. S. National Museum of the Smithsonian Institution. Further details will be announced later.

HONORABLE MENTION WINNERS

Black-and-White Division:

DR. F. A. HAMM, Central Research Laboratory, General Aniline & Film Corporation, Easton, Pa.: electron micrograph of crystals of heliogen blue pigment.

DR. CORNELIUS B. PHILIP AND N. J. KRAMIS, Rocky Mountain Laboratory, U.S.P.H.S., Hamilton, Mont.: photomicrograph of the chigger vector of tsutsugamushi, a larval mite mounted and cleared in polyvenal alcohol.

MR. THOMAS CARVER, Physics Department, Harvard University, and DR. FRANK H. J. FIGGE, Department of Anatomy, University of Maryland School of Medicine, Baltimore: a series of photographs of a hypospray jet in various stages of ejection.

MR. H. LOU GIBSON, Eastman Kodak Company, Rochester, N. Y.: series of photographs of *Plusiotis gloriosa* beetle to show technique of eliminating reflections from the elytra.

DR. ROBERT SCHREK, Veterans Administration Hospital, Hines, Ill.: photomicrograph series of cells under dark-field illumination.

Color Division:

MISS ALICE ELIZABETH SUTHERLAND, Research Division, Callaway Mills, La Grange, Ga.: photomicrograph of cross section of yarn from azlon cotton fiber.

MISS JANE K. GLASER, Argonne National Laboratory, Chicago, Ill.: tumor masses in soft tissue of rabbit from primary bone tumor.

MR. BRADFORD B. UNDERHILL, Ordnance Research Laboratory, Pennsylvania State College: multichannel oscillograph record.

MISS INES V. DEGRUY, Southern Regional Research Laboratory, U.S.D.A., New Orleans, La.: photomicrograph of cotton fiber swelled to show ballooning; photomicrograph of synthetic resin polymerized in wood.

NATURE'S GREATEST EXPLOSIONS: SUPERNOVAE*

N. U. MAYALL

Associate Astronomer at Lick Observatory in California, Dr. Mayall (Ph.D., California, 1933) is an editor of the Astrophysical Journal. *He has specialised in radial velocities of extragalactic nebulae and globular star clusters.*

TO UNREPENTANT sinners an atomic-bomb explosion may seem like the height of the Lord's wrath, but to unhallowed astronomers the real hell-fire of the universe is a *supernova*. In this bit of astral argot the Hollywoodian prefix, "super," needs no explanation; and "nova" is a traditional astronomical term meaning a new or temporary star. It is used to describe the sudden flare-up of stars, previously unknown or obscure, which for a time blaze with thousands of times their former brilliance in the case of ordinary novae, and millions of times in the case of supernovae. For reasons not yet fully understood, in novae an explosive process gains the upper hand over the normal and stable means whereby stars maintain their energy output. In an ordinary nova, the explosive outburst seems to be a surface phenomenon that generally results in the expulsion of a star's outer, or atmospheric, layers. In a supernova, however, the explosive process probably operates throughout most of the star, and results in blowing off a large part of its mass, and perhaps in converting much mass into energy.

The most luminous of the supernovae have been observed to shine with a brightness of 600 million suns, and at maximum light they pour out energy at a rate that staggers the imagination: tens of millions of times the solar rate. Since the latter is a truly astronomical number, 5×10^{23} horsepower ($10^9 = 1$ billion), even the largest earthly packet of energy—the atomic bomb—pales into insignificance alongside a supernova. For arithmeticians, calculation shows that one supernova is worth about 10^{29} atomic bombs. Thus, on the cosmic scale, science's greatest achievement in releasing energy hardly amounts to so much as a gleam in a firefly's lantern.

In the giant stellar system where the sun is but an average star among many billion others, only three supernovae are known to have occurred during the past thousand years: in A.D. 1054, 1572, and 1604. The supernova phenomenon is therefore a rare one, and the odds are comfortingly close to zero that the sun may become a supernova and thereby burn to crisps its encircling planets.

The three supernovae observed in our Milky Way stellar system, the Galaxy, are of exceptional interest because, in the astronomical sense, they appeared in our own front yard, at distances of the order of only several thousand light-years (1 light-year $= 6 \times 10^{12}$ miles). At such distances the tremendous luminosity of a supernova makes it a really spectacular celestial object. For example, a supernova 100 million times the luminosity of the sun, if distant 5,000 light-years, would appear as bright as the planet Venus at maximum brilliance. The three known galactic supernovae therefore excited much interest in their time, and there is little doubt that, if another one flared up at the present time, it would excite world-wide wonder, make much work for astronomers, and much money for astrologers.

The first known galactic supernova—that of 1054—is perhaps most interesting, because the debris of its explosion is easily seen today, even in a small telescope, as a small oval, cloudlike patch, widely known as the Crab Nebula in Taurus (Fig. 1). The 1054 supernova, however, seems to have been observed only in the Orient, casually by the Japanese, and more extensively by the Chi-

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* The author wishes to express his gratitude and appreciation to Dr. W. Baade, of the Mount Wilson Observatory, who kindly provided the illustrations for this article and who has done much to place our knowledge of supernovae on a sound observational basis.

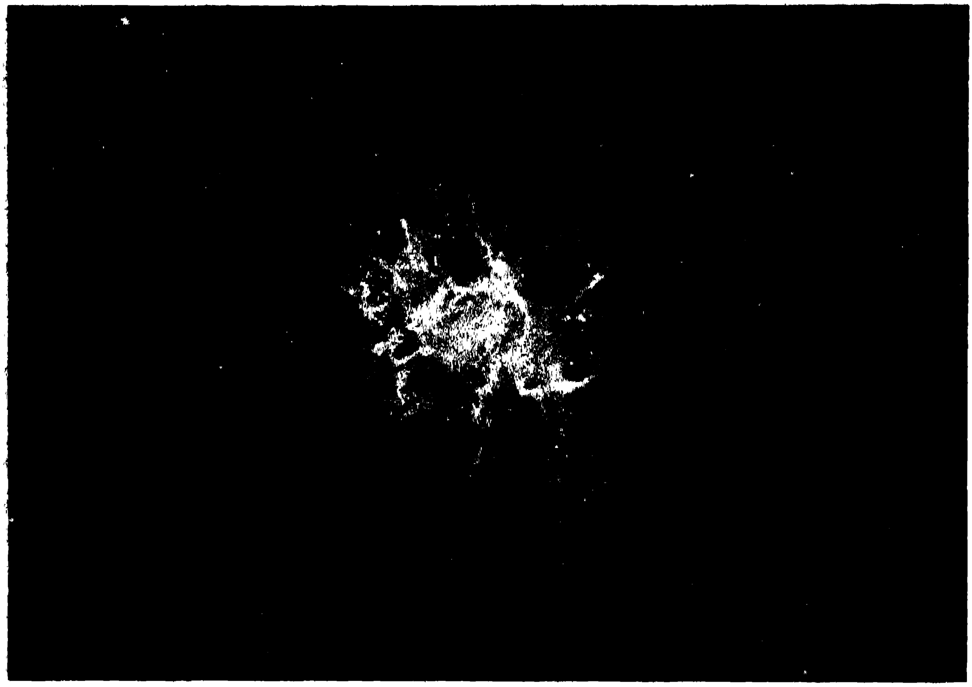

FIG. 1. THE CRAB NEBULA IN TAURUS

THIS PHOTOGRAPH, TAKEN IN RED LIGHT, SHOWS THE GASEOUS REMNANT THROWN OFF BY THE SUPERNOVA OF A.D. 1054. THE SHREDDED, FILAMENTARY STRUCTURE SUGGESTS OUTWARD MOTION, AND THE EXPANSION HAS BEEN MEASURED BY COMPARISON OF PHOTOGRAPHS TAKEN AT DIFFERENT TIMES. SUCH MEASUREMENTS, WHEN COMBINED WITH SPECTROSCOPIC OBSERVATIONS, LEAD TO A WELL-DETERMINED DISTANCE OF 5,000 LIGHT-YEARS. THE GASES COMPOSING THE NEBULA (OXYGEN, HYDROGEN, HELIUM, NEON) ARE IN A HIGHLY RAREFIED STATE, AND THEY PROBABLY ARE EXCITED TO LUMINESCENCE BY AN EXTREMELY HOT STAR NEAR THE CENTER, TOO FAINT TO SHOW IN THE REPRODUCTION. THIS EXCITING STAR, WHICH MAY HAVE A TEMPERATURE OF 500,000° C. AND A SIZE OF ONLY 2,000 MILES, IS THE ONLY KNOWN STELLAR VESTIGE OF A SUPERNOVA OUTBURST.

nese. The latter saw the supernova in daylight for several weeks, and in the night sky for nearly two years, when it faded from their unaided vision. Strange to relate, no European records of the apparition have yet turned up, possibly because popular interest in the new star was overshadowed at that time by Lady Godiva's famous horseback ride.

The other two galactic supernovae—those of 1572 and 1604—are commonly associated with two of the most famous names in astronomy, Tycho Brahe and Johannes Kepler. Tycho's nova of 1572 almost rivaled Venus at her brightest, and Kepler's of 1604 slightly exceeded Jupiter at his best. Both supernovae were observed for more than a year, and their position and brightness were systematically recorded. Although the observations were crude by modern standards—being made before the invention of the telescope in about 1609—they are, nevertheless, invaluable in the comparative study of supernovae found in other and much more distant parts of the universe.

To extend the study of supernovae from local to truly remote regions, the search for them needs to be pushed outside the Galaxy into the boundless firmament of the extragalactic nebulae. These are stellar systems, often called "Island Universes" or simply "galaxies," which are scattered more or less uniformly in space with an average separation of several million light-years. Because of these vast distances, supernovae in galaxies are seldom conspicuous objects (Fig. 2). A supernova 100 million times brighter than the sun, for example, would appear of magnitude twelve or seventeen at distances of 10 or 100 million light-years, and its detailed study would strain the resources of the largest telescopes. The latter, however, would justify their cost if a supernova should occur in one of the dozen nearest stellar systems that group around the Galaxy at distances ranging from 90,000 light-years, for the Magellanic Clouds, to 750,000 light-years for the spiral Andromeda Nebula, Messier 31. It was therefore somewhat of a tough break for astronomers when a supernova did appear in M31 in 1885, when astronomical photography and spectroscopy were in their infancy, and before the modern powerful telescopes were in operation. This supernova, known as S Andromedae, was nevertheless an exciting object, even in telescopes of moderate size, for it flared up close to the nucleus of the spiral, and at maximum light reached the seventh magnitude.

Discoveries of supernovae in galaxies were infrequent and fortuitous until about 1929. In that year a systematic search was initiated, first, at the Mount Wilson Observatory as a patrol of the great cluster of several hundred nebulae in Virgo and, later, in 1936, on Mount Palomar (the site of the 200-inch telescope) as a well-planned and extensive campaign to photograph regularly 150 fields. These surveys more than doubled the list of supernovae in distant galaxies and by 1942 brought the total to 40, a number sufficient to indicate with some reliability that, on the average, one supernova may be expected to appear in a galaxy every 500–600 years. Supernovae in the cosmic sense are therefore rare birds.

The extensive Mount Wilson and Palomar observations of supernovae in galaxies showed that the typical behavior is a sudden rise to maximum light, a brief lingering around the maximum, and then a steady decline to disappearance. This light variation, when properly calibrated, is called the light curve, and it gives in concise form most of the basic information relating to supernovae (Fig. 3). The three natural divisions of it, rise, maximum, and decline, are remarkably similar for the brightest supernovae that have been most completely observed. The light curves also indicate that probably there are two classes of supernova: Type I, the brightest, which average 100 million times brighter than the sun; and Type II, which average one-thirtieth as bright as Type I. Little is known about supernovae of Type II, except that they more closely resemble a normal nova than they do a supernova of Type I.

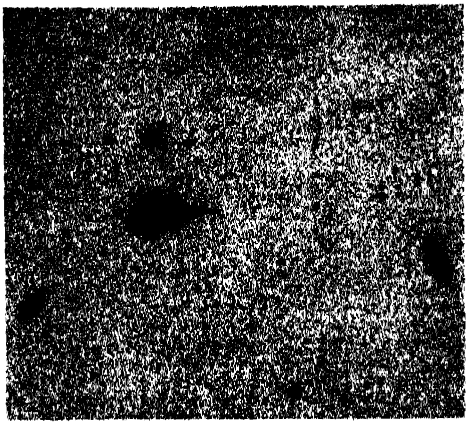

FIG. 2. A DISTANT SUPERNOVA

THE ARROW POINTS TO THE FAINT IMAGE OF A SUPERNOVA THAT OCCURRED IN 1936 IN THE SPIRAL NEBULA NGC 4273. THE SPIRAL IS DISTANT 8 MILLION LIGHT-YEARS AND IS A MEMBER OF THE GREAT CLUSTER OF HUNDREDS OF SIMILAR EXTRAGALACTIC NEBULAE IN VIRGO, SEVERAL OF WHICH APPEAR IN THE PHOTOGRAPH. THE SUPERNOVA WAS NEARLY TWO MONTHS PAST MAXIMUM WHEN THE PHOTOGRAPH WAS TAKEN, WAS OF ABOUT THE SIXTEENTH APPARENT MAGNITUDE, AND WAS SHINING WITH A LUMINOSITY EQUIVALENT TO THAT OF SEVERAL MILLION SUNS.

As might be expected, data regarding the rise are quite scanty, for the rate is so great, and the circumstances of observation generally so unfavorable, that the chances of recording a large part of the ascending branch are very slim indeed. Nevertheless, there is good evidence that a supernova rises from obscurity to its blazing maximum in 10 days or less. The increase in brightness is at least twenty magnitudes, or by a factor not less than 10 million times the initial luminosity. The supernova then hovers around maximum light for a week or so, when it shines with a whitish hue and with minor or no fluctuations. While at maximum, the supernova is comparable in luminosity with the stellar system in which it is located, although, because of the appreciable disper-

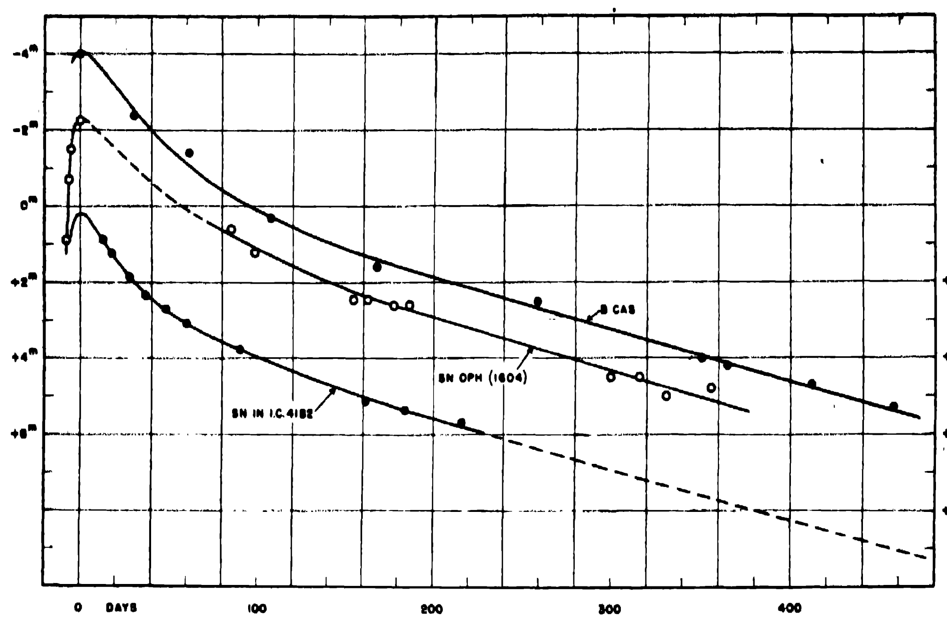

FIG. 3. LIGHT CURVES OF THREE SUPERNOVAE OF TYPE I

THE UPPER TWO CURVES ARE FOR TYCHO'S STAR OF 1572 (B CASSIOPAEIA) AND FOR KEPLER'S STAR OF 1604 (THE SUPERNOVA IN OPHIUCHUS). THE OBSERVATIONS IN BOTH CASES (*dots and circles*) WERE MADE BEFORE THE INVENTION OF THE TELESCOPE, AND THE SCALE OF APPARENT BRIGHTNESS ON THE LEFT REPRESENTS A SKILLFUL CALIBRATION OF THE OLD QUALITATIVE ESTIMATES. ON THE SAME SCALE, VENUS AT MAXIMUM IS CLOSE TO −4, JUPITER TO −2. THE LOWER CURVE IS FOR THE BRIGHTEST KNOWN MODERN SUPERNOVA THAT OCCURRED IN THE NEARBY DWARF SYSTEM IC 4182 (FIG. 5), AND ITS SCALE OF APPARENT BRIGHTNESS IS ON THE RIGHT. THE CHIEF POINT TO BE NOTED IS THE CLOSE SIMILARITY OF ALL THREE CURVES, ESPECIALLY AFTER THE HUMP.

sions in luminosity of supernovae and of galaxies, either one or the other may predominate by several magnitudes.

When the declining phase sets in, the supernova at first dims fairly rapidly, noticeably reddening at the same time. The fading, however, proceeds more slowly as time goes on, and, after approximately 3 months, the supernova loses light each day at the rate of 3.2 percent. In the best-observed case, this rate was uniformly maintained for more than 500 days, and other less completely observed supernovae substantiate the same remarkable linearity in the descending branch of the light curve (Fig. 4). The phenomenon is in marked contrast with the corresponding part of an ordinary nova's light curve, which often shows large and irregular variations.

In all cases of supernovae discovered in stellar systems beyond the Milky Way, the stars apparently vanish completely, without leaving any traces of the explosions (Fig. 5). But this fact does not necessarily mean that the star entirely transforms itself into radiation; it is more probable that, because of the great distances and our limited telescopic power, we are unable to photograph the products of celestial spontaneous combustion. In order to find out something about the remains of nature's greatest explosions, it is necessary to consider the cases of the three known galactic supernovae that have already been mentioned.

ALTHOUGH the Crab Nebula has been known for more than 200 years, it is only within the past 25 years that it has been recognized as the gaseous remnant of the supernova of 1054. The story of its identification is too long to tell here; suffice it to say that the ancient oriental chronicles and modern observations leave no reasonable doubt that the nebula originated in the outburst. Both direct photographs and spectrograms show that the nebula is expanding, as would be expected from an exploding star, and as has been observed in several ordinary novae. But of paramount importance is the presence, near the center of the nebula, of a certain sixteenth-magnitude star that *may* be the old supernova, 22 magnitudes fainter than at maximum. It is necessary to say "may" because the crucial observations are difficult to make, even with the largest telescopes. By the few tests that it is possible to make, however, there is good reason to regard this faint star as the only known end product of a supernova outburst. The properties of this faint star then become of surpassing importance, but unfortunately we know little of them beyond what can be theoretically inferred from the characteristics of the expanding gases that the star excites to luminescence. If the theoretical interpretation is correct, this faint star is an extraordinary object, having a temperature of the order of 500,000° C., a total luminosity 30,000 times, a density 180,000 times, and a radius only one-fiftieth that of the sun.

Since the positions of Tycho's star of 1572 and Kepler's of 1604 were determined as accurately as possible while the stars were visible, it is natural to wonder whether any traces of these stellar explosions can be located at the present time. The search has proved successful only in the case of the supernova of 1604. Photographs taken in red light with the 100-inch Mount Wilson reflector disclosed the presence, very close to the location of Kepler's star, of a number of faint, broken knots and filaments of a fan-shaped nebulosity (Fig. 6). Its spectrum, which is very difficult to record, closely resembles that of the Crab Nebula. All the indications are that the fan-shaped, brightest area is only one part of a larger mass, which future observations are expected to show is expanding. No stellar vestige has yet been identified, although there is a suspicious eighteenth-magnitude star at the point of the fan. The supernova appeared in the Milky Way in a region rich in faint stars highly reddened by interstellar dust, and the identification of a very faint hot star, normally blue, is a formidable job.

Although the search for the remains of the supernova of 1572 has been pushed with equal vigor and skill, nothing has been found in the location of Tycho's star. Unfortunately, because of its type of mounting, the 100-inch reflector cannot be pointed to the far-northern position of the old supernova. But the 200-inch telescope will not be thus restricted, and the odds are about even that

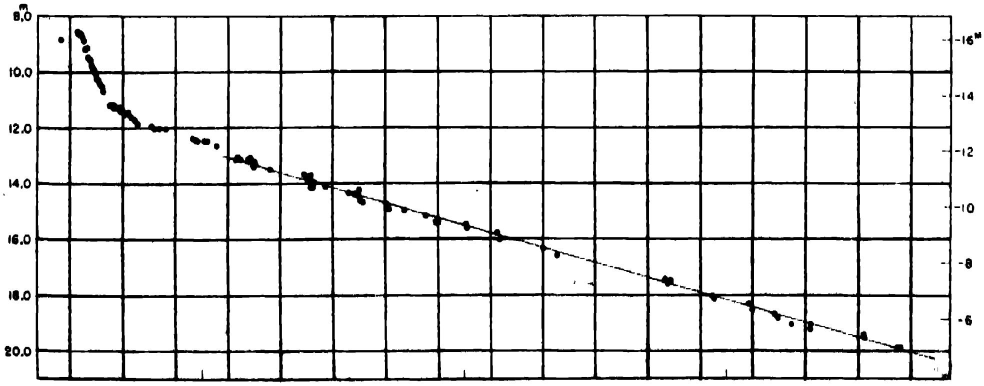

FIG. 4. LIGHT CURVE OF THE SUPERNOVA IN IC 4182

DETERMINED FROM MODERN PHOTOGRAPHIC OBSERVATIONS, THE SCALE OF APPARENT BRIGHTNESS (m) ON THE LEFT SHOWS THAT THIS SUPERNOVA WAS FOLLOWED FROM ITS MAXIMUM TO NEARLY $m = 20$, WHICH IS NEAR THE LIMIT OF MEASUREMENT WITH THE 100-INCH TELESCOPE. THE SCALE OF LUMINOSITY (M) ON THE RIGHT SHOWS THAT, AT MAXIMUM, THE SUPERNOVA REACHED AN ABSOLUTE MAGNITUDE OF $M = -16$, OR SOME 600 MILLION TIMES AS BRIGHT AS THE SUN, WHICH, ON THE SAME SCALE, HAS $M = +5$. A NOTEWORTHY FEATURE OF THE LIGHT CURVE IS THE UNIFORM RATE OF DECLINE, MAINTAINED FOR MORE THAN 500 DAYS.

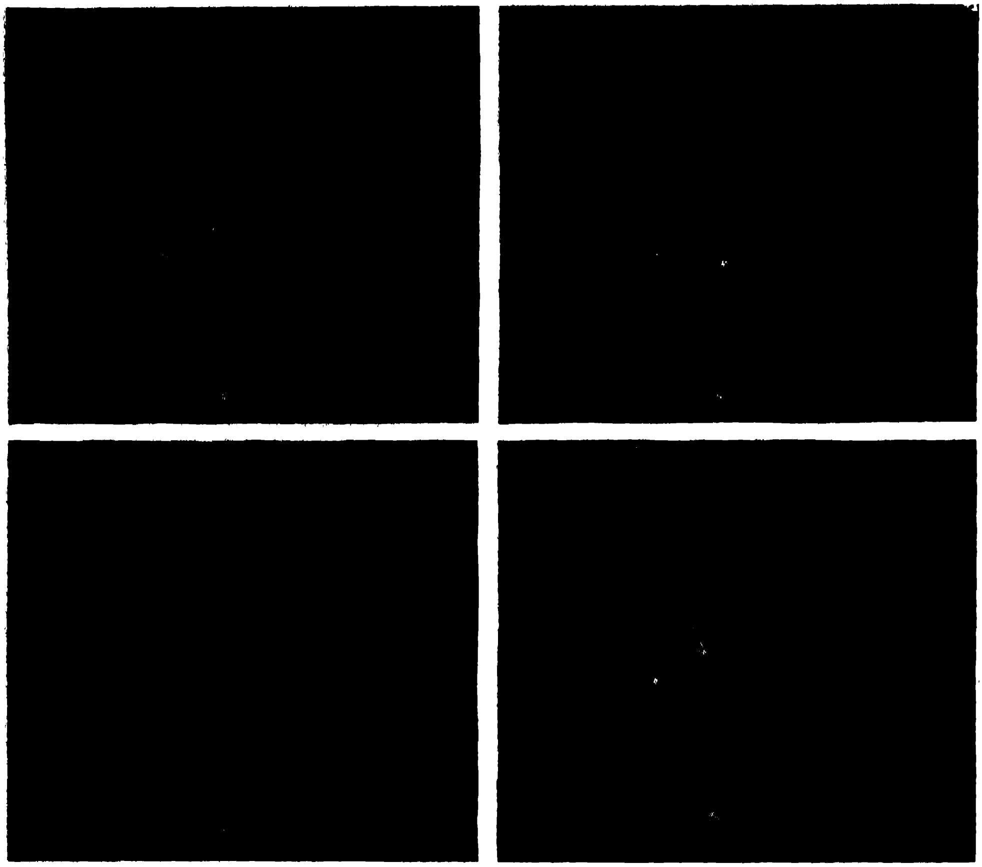

FIG. 5. THE PICTURE HISTORY OF A SUPERNOVA

(1) *Upper left,* OBSCURITY, MARCH 8, 1937; (2) *upper right,* MAXIMUM, SEPTEMBER 10, 1937; (3) *lower left,* DECLINE, APRIL 1, 1938; (4) *lower right,* OBLIVION, JANUARY 19, 1940. THESE PHOTOGRAPHS RECORD THE OCCURRENCE OF THE BRIGHTEST KNOWN SUPERNOVA, MAXIMUM LUMINOSITY EQUAL TO 600 MILLION SUNS, THAT OCCURRED IN 1937 IN THE DWARF EXTRAGALACTIC STELLAR SYSTEM IC 4182, DISTANCE 3 MILLION LIGHT-YEARS. IN (2) THE PARENT STELLAR SYSTEM DOES NOT SHOW BECAUSE THE EXPOSURE TIME WAS PURPOSELY SHORTENED TO AVOID OVEREXPOSURE OF THE SUPERNOVA. IN (1) AND (4), ALTHOUGH MANY STELLAR CONDENSATIONS APPEAR, THERE IS NO TRACE OF THE SUPERNOVA BEFORE AND AFTER ITS OUTBURST.

a search to fainter limits will uncover the shredded skeleton, or shade, of the strange star of 1572.

Of comparable importance with the light curve is the spectrum of a supernova (Fig. 7). But here, too, the difficulties of observation are considerable, and up to the present only one good series of spectrographic observations has been obtained of a supernova. This was one that had the highest recorded luminosity of 600 million suns and attained nearly the eighth apparent magnitude, although at a distance of 3 million light-years. Less complete and fragmentary spectroscopic records of other and fainter supernovae have been obtained, and all the material indicates that spectra of supernovae are astonishingly similar at corresponding parts of their light curves. Beyond that fact, however, a typical spectrum is almost an unsolved puzzle. It is not even established, for example, whether the spectrum consists wholly of very wide emission bands, of a continuous spectrum, or of a combination of both. Only two radiations have definitely been identified: two emissions in the red, due to neutral oxygen atoms, which are characteristic of a gas of very low density. These emissions are much narrower than other broad-banded features in the supernova spectrum, and they make their appearance some 180 days after maximum. These facts—smaller width and time of appearance—suggest comparison of supernovae with ordinary novae, which show similar radiations at a time when the ejected gaseous envelope has expanded into a shell of great tenuity, much larger than the initial stellar atmosphere.

The most promising direction to take for the interpretation of the mysterious supernova spectrum may be to proceed from the fairly well-understood spectrum of a normal nova, through that of supernovae of Type II to that of Type I. The reason for such a procedure is that fairly recent spectroscopic observations of a supernova of Type II, followed from maximum to 76 days afterwards, show characteristics that may be intermediate between normal novae and supernovae of Type I. The evidence suggests that a normal nova is but a vest-pocket edition of a supernova of Type II; but whether a supernova of Type I is a full-folio edition of a supernova of Type II remains to be seen. At the present stage of our knowledge of supernovae, it is difficult to know whether to stress the differences or the similarities between supernovae and ordinary novae.

Theories of the supernova phenomenon plunge into a realm as bewildering and interesting as Alice's Wonderland. Such cataclysmic events as stellar collisions, annihilation, and collapse have been postulated in order to account for the fantastically large amounts of energy liberated. The general conclusion of much intellectual abstraction is that the causes for supernova outbursts probably are to be sought within the stars themselves, and not in some external happening such as a collision or close approach of two stars. Moreover, since probably there is one stellar remnant known, and more may be found, complete conversion of a star into energy (annihilation) seems to be ruled out. The elimination leaves the field to the collapse process, which has some highly diverting aspects.

At some point in its life history, a star approaches the condition of exhausting its fuel supply, which in most cases probably is the predominately abundant hydrogen. With depletion of its source of energy, the star contracts and grows smaller, dimmer, and denser. But the process may or may not be uniform, or lead to a stable configuration, depending upon the amount and composition of matter in the star. Matter in an exceedingly dense state, wherein the atoms are entirely stripped of their electrons and are thereby packed very close together, is called degenerate. According to one theory of degenerate matter, there may be a critical range of stellar masses such that, during the contraction phase, degenerate cores of exceedingly high density may develop. For sufficiently high densities, the stripped nuclei of hydrogen atoms (protons) may combine with electrons to form neutrons. The combination would result in a sudden decrease in the central pressure, and the star would collapse with the release of a prodigious amount of energy. In another formulation of the problem, the

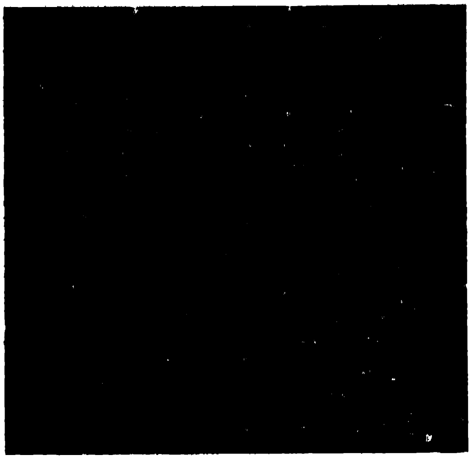

FIG. 6. A SUPERNOVA'S REMAINS

THIS PHOTOGRAPH, TAKEN IN DEEP RED LIGHT IN ORDER TO PENETRATE THE INTERSTELLAR DUST HAZE IN THE MILKY WAY, SHOWS THE SPARSE GASEOUS REMAINS OF KEPLER'S SUPERNOVA OF 1604, THE LAST KNOWN SUPERNOVA IN THE GALAXY. FUTURE OBSERVATIONS ARE EXPECTED TO SHOW THAT THE GASEOUS FILAMENTS ARE MOVING OUTWARD FROM THE CENTER OF THE EXPLOSION, AS IN THE CRAB NEBULA. BUT UNTIL, OR UNLESS, SUCH MEASUREMENTS ARE MADE, THE DISTANCE TO THIS OLD SUPERNOVA PROBABLY WILL BE INDETERMINATE, BECAUSE OF UNCERTAINTY REGARDING THE AMOUNT OF DIMMING PRODUCED BY THE HAZE OF THE INTERSTELLAR DUST.

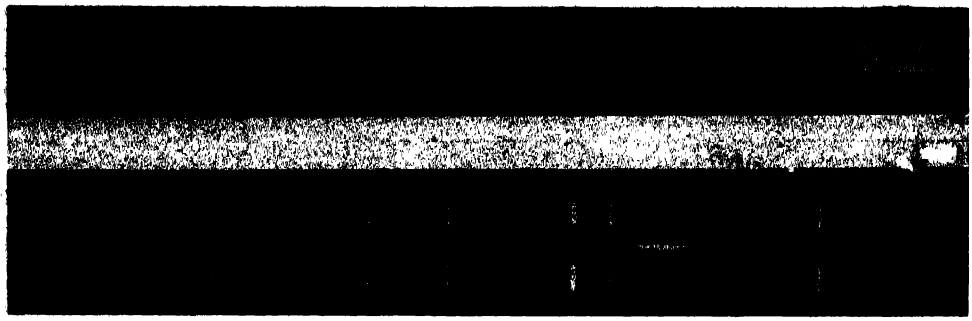

FIG. 7. THE SPECTRUM OF A SUPERNOVA

Above, 9 DAYS; *below*, 225 DAYS PAST MAXIMUM. IN THESE REPRODUCTIONS OF SPECTROGRAMS, RED IS TO THE RIGHT, AND BLUE-VIOLET TO THE LEFT; COMPARISON SPECTRA (THE TWO SETS OF SHORT VERTICAL LINES ON EITHER SIDE OF THE SUPERNOVA SPECTRA) ARE OF IRON AND NEON (*above*), AND OF MERCURY AND ARGON (*below*). ALTHOUGH THE BROAD BANDS IN THE SUPERNOVA SPECTRA HAVE NOT BEEN CERTAINLY IDENTIFIED, THEIR GREAT WIDTHS SUGGEST A STELLAR ATMOSPHERE EXPANDING WITH HIGH VELOCITY, UP TO 4,000 MILES PER SECOND. THE ONLY DEFINITELY IDENTIFIED RADIATIONS ARE TWO NARROW ONES IN THE RED (*indicated by arrows on right*) THAT ARE CHARACTERISTIC OF NEUTRAL OXYGEN SHINING AS A HIGHLY RAREFIED GAS.

decisive factors are rotational instability and central density. If the star reaches a certain critical central density before it begins to throw off matter by rotation, the central temperature can become high enough (several billions of degrees Centigrade) to produce nuclear reactions which absorb, rather than emit, energy at an enormous rate. The result is that the internal pressure due to radiation is no longer maintained, and thus the balance between the opposing forces of radiation and gravitation is destroyed. The change from emission to absorption takes place so fast—the computed time is only 100 seconds!—that the star collapses and releases its gravitational energy on a stupendous scale.

Just how far the collapse of a star can proceed is a question of much interest. If the stellar remnant of the A.D. 1054 supernova is an example, it has a computed radius of about 2,000 miles. Although this value may seem small for a star, it is by no means so small as some of the limiting sizes computed in theories of degenerate neutron cores. One calculation, for example, led to a minimum size of only a few miles, which if significant would have some curious consequences. For a stellar mass to occupy so small a space would imply an enormous Einstein red shift of the star's radiation—right out of this world. The properties of space around such an object would be so drastically altered by the strong gravitational field that light could neither enter nor leave the vicinity. If such is the fate of a supernova, we have the singular phenomenon of the greatest outburst in the universe ending as practically nothing. It is as if a star, after a cosmic binge, suffers remorse, and then achieves the ultimate in oblivion by crawling into a hole and pulling the hole after it.

INTRAVENOUS INJECTIONS

CARL A. DRAGSTEDT

Professor Dragstedt (M.D., Rush, 1921; Ph.D., Chicago, 1923) has been professor of pharmacology of the Medical School, Northwestern University, since 1926. He is a member of the committee on revision of the U. S. Pharmacopoeia.

THE administration of drugs, or of fluids such as saline, plasma, and blood, by direct injection into the veins is now such an everyday occurrence that little thought is given either to the vicissitudes involved in adding this technique to the physician's repertoire or to the numerous by-products of information that pursuit of this objective produced. There are a number of very excellent reviews of the development of this procedure presented in more or less orthodox historical fashion, to which the interested reader is referred for detailed accounts, the present review being an attempt to survey the story from the following points of view: the motives that inspired this procedure; the methods employed; the difficulties, problems, and reactions encountered; and, finally, the lessons learned incidental to the prosecution of this procedure.

According to Brown, the earliest purpose for which intravenous injections were made was the post-mortem injection of vessels made to facilitate anatomical dissection and study. This was done by Mondinus as early as 1316 at Bologna and developed to a greater extent by Silvius, Eustachius, and Vesalius. Although the scientific employment of this procedure would seem likely to presage the general practice of embalming the dead by vascular injection, this procedure does not appear to have been employed to any extent until after intravenous injections in the living had been studied rather extensively.

It seems very probable that the first purpose conceived as requiring intravenous injection in living animals or man was the transfusion of blood, but as to whether blood was actually transferred from one animal to another by this method until after the feasibility of this route was demonstrated by using other agents is uncertain. Omitting the ancient references by Ovid and others to the possibility of blood transfusion, as being so vague, on the one hand, and so barren of results in the way of recorded efforts to put the hints into practice, on the other, as to leave grave doubts that the references are any more than literary extravagances, it seems possible that the Italian philosopher Ficinus was the first (in 1489) to suggest that the blood of a healthy young man might be given to the aged for its rejuvenating effect. Villari, in his *Life and Times of Girolamo Savonarola,* recounts the purported attempt of a Jewish physician to save the life of Pope Innocent VIII in 1492 by transfusing the blood of three young boys into the aged and sick pontiff. Various historians, who take the position that the idea of blood transfusion could not have arisen prior to the knowledge of the circulation of the blood (Harvey, 1628), discount this report by asserting that it is most probable that the blood drawn from the young men was intended for oral administration. Inasmuch as the boys died and the Pope received no benefit, it seems fruitless to argue the merits of the various versions of this account. Hieronymus Cardanus (1505–76), Magnus Pegelius (1593), Andreas Libavius (1615), and Giovanni de Colle (1628) seem to have considered the possibility of transferring blood directly from the vessels of one animal to those of another, but the claimants for the credit of first successfully accomplishing this operation are usually listed as Richard Lower, Francesco Folli, Jean Baptiste Denys, Johan Daniel Major, Robert des Gabets, and Tardi. As one may infer from the names, nearly every European nation has its aspirant for this honor. Disregarding the contending claims for priority of idea, priority of accomplishment, and priority of publication, it is certain that transfusion of blood from animal to animal and from animal to man was done repeatedly by various workers during the latter half of the seventeenth century. Because of difficulties, deaths, and doubtful utility, it was abandoned until the nineteenth century, when Blundell (who advocated the transfusion of blood from man to man) revived it. The subsequent development of blood transfusion is not a part of this story, although some reference to it will be made in another section.

Not credited by all historians as valid, an early motive for resorting to intravenous injection was the morbid curiosity of one Hans Gurge of Ehrendorff, riding and kennel master to an Austrian nobleman, who is reported by Ettmüller to have injected Spanish wine into the veins of dogs in 1642 for the purpose of making them drunk, their inebriated actions and howlings providing amusement to the owner. It is pertinent to this account, whether valid or not, to note that a person with a depraved sense of humor, wishing to administer wine to an uncooperating dog in 1642, had no alternative to the intravenous route, for the apparatus and technique of administration by stomach tube were not yet developed, and wine is much too irritant a clyster to remain long enough in the short colon of a dog to permit much absorption.

Quite generally accepted as being the first experiments involving the intravenous injection of drugs, the experiments of Sir Christopher Wren and his collaborators may also be credited with being designed for serious scientific purposes. Henry Oldenburg, editor of *Philosophical Transactions* (Vol. 1, 1665), in "An account of the Rise and Attempts of a way to conveigh Liquors immediately into the Mass of Blood" states:

> 'Tis notorious, that at least six years since (a good while before it was heard of, that any one did pretend to have so much as thought of it) the Learned and Ingenious Dr. Christopher Wren did propose in the University of Oxford (where he is now the worthy Savilian Professor of Astronomy, and where very many Curious Persons are ready to attest this relation) to that Noble Benefactor to Experimental Philosophy, Mr. Robert Boyle, Dr. Wilkins, and other Deserving Persons, that he thought he could easily contrive a Way to conveigh any liquid thing immediately into the Mass of Blood.

And, according to his report, experiments to carry out these suggestions were promptly instituted.

This somewhat belated claim for Wren's priority was occasioned by the appearance of Johan Daniel Major's "Chirurgiae Infusiorae" in 1664, and John Elsholz' "Clysmatica Nova" in 1665, and the reports that Carolo Fracassato had performed similar experiments in Pisa in 1658 likewise prompted Timothy Clarke in a later communication to the *Philosophical Transactions* to correct Oldenburg's date (1659?) and fix it definitely as 1656.

These experiments, in which opium and crocus metallorum (sulphuret of antimony) were injected into the veins of dogs, established the fact that these drugs produced the same effects (stupefaction and vomiting, respectively) as they did when administered by mouth, and thus dealt a mortal blow to the prevailing doctrine of sympathy, which held that drugs produce their characteristic effects by acting upon specific loci in the gastrointestinal tract, thereby inducing reflex effects in those organs sympathetically related to these sites. These may, therefore, be called the first experiments in pharmacology directed toward the analysis of the mechanism of the action of drugs.

The experiments with intravenous injection by Major, Elsholz, Scotus, Hoffman, Fabricius, Schmidt, Ettmüller, and others, which followed closely after those of the Oxford group (Wren, Boyle, Wilkins, Lower, Clarke), may be regarded as in the main motivated by the desire to determine whether drugs would have the same curative effects when administered in this fashion as when given by mouth. Various of these early investigators commented upon the fact that their results did or did not indicate that drugs had to be modified by the gastrointestinal secretions in order to be effective, that the method enabled physicians to give drugs to unconscious or uncooperative patients, and that the promptness of effects might be of advantage in certain instances, etc.; but, generally speaking, all the seventeenth-century experiments may be viewed as efforts to appraise the intravenous route as merely an alternative to oral medication and of only theoretical interest in view of the extreme hazard. As might be expected, miraculous and incredible cures were claimed by some and denied by others. But, plagued by frequent and severe, and often fatal, reactions, the practice lapsed for about a century.

Thus it was that in 1821, when E. Hale wrote his Boylston prize essay, "On the Propriety of Administering Medicine by Injection into the Veins," he felt called upon to survey the necessities and advantages of the method as follows:

> The extravagance of the effects imputed to the injections so entirely destroys the credibility of the accounts, that no reliance can be placed in them, except to show that the attempt has long since been made to take advantage of this mode of administering medicine. In every other point of view we must regard this as a new question, at least so far as the practicability of the injections is to be determined.

Hale reduced the motives or indications to two: to remove a mechanical obstruction of the esophagus by injecting an emetic intravenously, and to give a cathartic intravenously when irritability of the stomach precluded its oral administration. In view of the frequent mention of the former indication by other writers as well, it seems that our ancestors of this era were often guilty of bolting their food in such a manner as to render esophageal obstruction of this sort a not-infrequent episode.

With a greatly renewed interest in the feasibility of intravenous injections in the nineteenth century, we find an equally expanding variety of motives. Magendie, seeing the piteous victims of hydrophobia, unable to swallow and often thrown into convulsions at the mere suggestion of swallowing that the sight of water provoked, advocated the surreptitious intravenous injection of water in such cases. Thomas Latta, noting the marked loss of water and salt in cholera victims, suggested the intravenous injection of saline solutions as a replacement measure; and various workers, noting the evident starvation effects in patients with intractable vomiting, made attempts at intravenous feeding, using milk, egg white, and sugar.

In 1841 James Blake injected baryta into the jugular vein of dogs and watched for its appearance in the opposite carotid as a means of determining the circulation time in animals. This agent, detectable chemically, was thus the forerunner of a host of agents detectable in a variety of ways (dyes—colorimetrically; salts—by electrical conductivity changes; physiologically active substances—by specific symptoms; radioactive agents—by detection of radioactivity) that have been used for this purpose since. Intravenous injections were employed by the experimentalists of this period, for, as Blake states:

> In my experiments I have always investigated the physiological action of a substance by introducing it directly into the veins or arteries of a living animal. It is thus with the smallest disturbance possible to the normal condition of the animal, brought into direct contact with the tissues whose reactions we wish to investigate. Its subcutaneous injection or introduction into the stomach gives rise to complications which are better avoided.

To come back to clinical motives of the nineteenth century, cold water was injected into the umbilical vein to facilitate separation of the placenta, and, noting that absorption of drugs was often slow in patients with pronounced edema, practitioners proposed to produce an artificial plethora by the injection of tepid water into the veins to delay absorption in cases of drug poisoning.

The recognition that a characteristic feature of the intravenous injection of drugs was promptness in the exhibition of the full effects of the dose administered and a corresponding promptness in recovery owing to rapid elimination brought about the attempt to use the intravenous route for the administration of anesthetics in the nineteenth century with the agents then available (ether, chloroform, chloral hydrate), because these attributes were recognized as desirable features of anesthesia. It was not, however, until the discovery of the agents that became available in the twentieth century that intravenous anesthesia became a feasible and successful procedure.

Toward the close of the nineteenth century came the recognition of the bacterial causation of many diseases, the knowledge that in many of these cases bacteria are present in the blood stream, and that many chemical agents are more or less destructive of bacteria. Countless attempts to cure infectious diseases by the intravenous injecton of phenol, mercury and silver salts, and all sorts of antibacterial agents promptly followed. These attempts were so numerous, so extensive, and so thoroughly planned that their futility and lack of promise were disappointing and discouraging in the extreme. Von Behring gloomily concluded:

> It can be regarded almost as a law that the tissue cells of man and animal are many times more susceptible to the poisonous effects of disinfectants than any bacteria known at present. Therefore, before the antiseptic has a chance either to kill or to inhibit the growth of the bacteria in the blood or in the organs of the body, the infected animal itself will be killed. The pessimism of him who declared that disinfection in the living body is for all time impossible appears to be only too justified.

WITH the twentieth century new indications and motives, as well as extensions and embellishments of older motives, appeared. Although suggested long since and sporadically employed, the intravenous injection of various saline solutions did not come into general use until World War I. Improvements and development of the technique of blood transfusion had taken place, but not to the extent that it was promptly applicable in cases of shock and hemorrhage. Thus the search for blood substitutes, with which the circulating blood volume could be restored, was extremely vigorous, and much of our current knowledge of the injectability of various things had its origin in these war-inspired researches. The use of saline solution to replace lost fluid and lost electrolytes, to correct disturbances in the acid-base balances of the blood, etc., came out of these studies. So, too, did recognition of the fact that the protein level of the blood plasma is important in the regulation of the water deposits in the body and that the intravenous injection of plasma proteins, or substitutes, such as acacia, would relieve cases of edema due to hypoproteinemia.

The recognition that certain substances are eliminated exclusively by the kidneys in the urine or by the liver in the bile resulted in the development of methods for determining the functional capacity of these organs, involving the intravenous injection of readily detectable substances. Rendering the injected agents radiopaque provided means whereby the urinary or biliary tract could be outlined radiographically for diagnostic purposes.

Shortly after the war came the use of intravenously injected hypertonic solutions (first, sodium chloride; later, dextrose and sucrose) to reduce cerebrospinal fluid pressure by osmotically dehydrating the brain and ventriculo-subarachnoid system when, in cases of concussion, skull fracture, or brain tumor, the indications for doing so were present.

Injection of radiopaque materials to outline blood vessels and of fluorescent material to determine the patency and competency of the vascular supply where signs of gangrene or vascular obstruction warn of the necessity of surgical intervention are some of the more recent motives for this procedure.

The methods employed do not need to be described at length. Whether we place any credence in the legend recounted by Pliny that the ancients learned of the potential uses of the enema from the Ibis, which agile bird, according to this account, "washes the inside of his body by introducing water with his beak into a channel by which our health demands that the residue of our food should leave," the fact remains that the clyster art was highly developed, and there were many types of apparatus adaptable to the procedure of intravenous injection when this latter was first thought of. And so we find the earliest experimenters using quills, hollowed-out chicken bones, silver tubes, etc., as the instruments to insert in the sectioned vein, and compressible bags (the scrotum or bladder from various animals, silk purses, etc.) or piston-type syringes or, on occasion, the mouth of the operator as the container of, and propelling mechanism for, the injected material. The designation of the intravenous injection procedure as *Ars Clysmatica Nova* or *Chirūrgia Infusoria* gives testimony to the lowly origin of the techniques employed, and we find most of the early workers using the expression that such and such a substance was "thrown into" the blood.

The sharp-pointed, detachable syringe needle and syringes, which may properly be called the precursors of the conventional all-glass syringe of today, evolved about the middle of the nineteenth century, owing to the labors of Rynd, Thomas Wood, and Pravaz. Thus it is that intravenous injections antedated subcutaneous injections by hundreds of years, and also that surgical exposure of the vein to be injected, with its accompanying hazard of infection and phlebitis, was necessary until these more modern instruments had been perfected. Learning that it did not take great force to impel fluids into the veins, various types of gravity infusion apparatus were also developed. These, too, have their current counterparts in everyday use.

The difficulties, problems, and reactions encountered throughout the development and perfection of the intravenous technique were numerous and disconcerting. Those peculiar to blood transfusion need not be recounted fully here, as the tale is formidable in itself and has been dealt with extensively by numerous writers. Suffice it to say that in 1667 one of the pioneers noted that "transfusing a great quantity of blood into several Doggs, observed alwayes, that the Receiving Doggs pissed Blood" and that, although homologous transfusions were advocated by some as early as 1818, it was not until 1882 that Cohnheim, upon the basis of his own studies and those of Creite, Landois, and others, concluded that "the serum of one species is a direct poison to the corpuscles of another"; not until 1900 that Landsteiner added the fact that there could be incompatibility of blood within a species because of isoagglutinins, not until 1914 that the problem of keeping blood incoagulable as a means of facilitating its administration was solved; and not until more recent years that the problems of the Rh factor, etc., were understood. And—it may be added—the end is not yet.

As to the tribulations involved in intravenous injections in general, Oldenburg in his 1665 account (of Wren's work) gave hint of what might be anticipated as follows: "Some whereof, though they may conceive, that Liquors thus injected into the Veins without preparation and digestion, will make odde commotions in the Blood, disturb Nature, and cause strange Symptoms in the Body." Major had noted (1664) that the injection of acids coagulated the blood and thereby caused death promptly. These observations were quickly corroborated by Fracassatus and others, who noted, in addition, that the animals appeared to die of suffocation and at post-mortem showed a characteristic frothy, slimy appearance of the lungs. Although it was a long time before this accident (of pulmonary embolism due to thrombosis) was clearly defined and its hazards appreciated, it cannot be doubted that it was a frequent cause of the severe reactions and fatalities with which this procedure had to contend, and that in a sense it was the first hazard peculiar to intravenous injections which was partially resolved and understood.

Although septicemia and pyemia as well as phlebitis were frequently encountered and commented upon by the early pioneers, these had been experienced as hazards attending bloodletting procedures alone, so that they were not construed as primarily related to intravenous injections per se. Accordingly, it may be said that air embolism was the next hazard associated with the procedure to be recognized. There can be little doubt, in view of the techniques of 1664, that it was air embolism (owing to the aspiration of air through the gap in the vein because of the negative pressure incident to respiration) that explained Major's findings that he could inject into the leg veins of dogs without harm many things which caused more or less instant death when he attempted to inject them into the jugular vein. But Major did no more than comment on this strange paradox. According to Fortescue-Brickdale, it was Friend, whose complete works were published in 1773 after his death, who was the first to demonstrate the danger of the entry of a large quantity of air into a vein and the first to note the characteristic frothy distension of the right ventricle at post-mortem after such an accident.

That the serious consequences occasioned by the accidental injection of air are brought about by the ability of air bubbles to act as emboli in obstructing the pulmonary circulation was not recognized for some time. Many workers presumed that it aroused a peculiar excitability of the coats of the veins and therefore argued that, if such a bland, nonirritating substance as air could do this with fatal consequences, it would be unlikely that anything save blood itself could ever be used successfully. Hale, who gave voice to such sentiment, concluded, however, that it was fruitless to explore the proposition on theoretical grounds, or even to determine it by animal experiment alone, and thus arrived at the decision to make his tests upon himself. His account of the bold experiment in which, in spite of great difficulties, he injected one ounce of caster oil into his arm vein, entitles him to a place on the honor roll of martyrs in medicine, for though he survived the experience and his arm was not permanently ruined, his good luck in surviving was such that, as Fortescue-Brickdale put it, "It seems surprising that circumstances did not compel him to limit his protest to the length of an epitaph." It was not until some years after Hale's experiments that authorities concluded "that oils, injected into the veins interrupt the circulation and produce a kind of asphyxia." Fat embolism, as we now call it, was thus another variation in this type of hazard that came to be recognized.

In 1841 Rees showed that—

> agents which alter the specific gravity of the serum of the blood exert an influence on the blood corpuscles by affecting the endosmotic currents through their membranes; agents which increase the specific gravity of the serum collapse the corpuscles, while those which lessen the specific gravity distend them. Water injected into the jugular vein of the dog caused rapid distension of the corpuscles and rupture of their membranes.

Crenation of corpuscles, on the one hand, and hemolysis, on the other, thus became recognized, and the desirability of injecting materials in isotonic concentration was established. Or, in other words, it came to be realized that substances intended for intravenous injection should have their miscibility with blood determined.

Nausea and vomiting, chills and fever, have dogged the steps of intravenous injection throughout its history. In a letter translated by Oldenburg and appearing in the *Philosophical Transactions* of 1667, Fabricius, commenting upon the results of the injection of "a Laxative Rosin, dissolved in an Antiepileptical Spirit" into three patients, states: " 'Tis remarkable, that it was common to all three to vomit soon after the injection, and that extremely and frequently; the reason whereof we leave to intelligent Physicians to assign." Since the drug used produced only catharsis when administered orally, it seems capricious of Fabricius to veil the explanation in this whimsical fashion, for even now, nearly three hundred years later, although nausea and vomiting can be attributed to distinct causes in many instances (e.g., it is part of the symptom complex produced by embolic, pyrogenic, and anaphylactic reactions, etc.), there remain cases in which it seems to have a mysterious psychogenic origin, occurring in patients after a first injection, but disappearing with succeeding injections.

With the advent of the science of bacteriology, it seemed plausible that all instances of chills and fever consequent to intravenous injection could be attributed to the unavoidable accompanying injection of contaminating bacteria. But, alas, asepsis and antisepsis failed to prevent febrile reactions, and it was not until 1923 that Florence Seibert showed that bacteria, growing in more favorable media or even in distilled water, produced a filterable substance (stable to boiling, the usual sterilizing procedure) called pyrogen, which produced chills and fever when injected intravenously; and it was not until about ten years later that a reasonably successful elimination of pyrogen from injection materials was accomplished.

The hazard of anaphylactic reactions was only tardily appreciated, as might be expected in view of our current knowledge. That certain substances (chiefly proteins, but not all proteins) could be innocuous upon a given injection and then markedly poisonous upon a subsequent injection (if the latter were given at an appropriate time interval) was a startling and apparently incredible observation. But, securely established as a fact, it became the starting point for a tremendous effort at its understanding and, more pertinent to the present story, the established practice of studying the reinjectability of everything that was considered as meriting injection in the first place. It has been learned that the agents that may provoke anaphylactic reactions are those that engender the formation of specific antibodies; hence, they are called antigens, and agents to be injected are now tested for their antigenicity. In view of the relative seriousness of anaphylactic reactions, this question is of paramount importance.

Perhaps the most recent claim to the role of a hazard peculiar to intravenous injection is that of the untoward effects associated with rapid administration, which has been given the name of "speed shock." Noting that various substances, innocuous upon slow injection, produced marked fall of blood pressure, respiratory irregularity, etc., upon rapid injection, investigators postulated that some of the injected molecules, speeding rapidly to the liver and doing damage to the liver cells, thereby caused the observed effects. The studies of DeBakey, Milbert and Rothwell and Crocker, and others have shown that there is no rational basis for this conception, but it still remains a fact that the speed of injection is of the utmost importance with several kinds of agents. For example, the rapid injection of a soluble barbiturate salt will cause respiratory arrest before enough has been injected to produce anesthesia, and the rapid injection of a soluble quinine salt will cause cardiac embarrassment and fall of blood pressure in a dose that is perfectly safe if slowly administered. Similar experiences obtain with bile salts, potassium salts, ether and chloroform, etc. It is quite clear that these are concentration effects occurring with substances which diffuse readily throughout the body so that, when given slowly, they produce their characteristic pharmacological effect when the total body concentration of the drug is at the requisite level, but when injected rapidly there may be a very high concentration of the drug carried in the blood to the susceptible tissues. It is also clear that for drugs like arsphenamine, which are poorly soluble at the pH of the blood, there will be some drug coming out of solution and producing the effects of particulate matter (embolism, etc.) when injected rapidly—effects that can be avoided by slow injection. Thus, there is some validity in the idea that there is a "speed-shock" factor in connection with certain intravenous injections, without drawing upon some mysterious velocity factor that should obtain for all.

It seems unnecessary to add that the total volume of fluid to be injected is now also recognized as a potential source of strain to the heart and circulation.

Thus, we find the progress of intravenous therapy an oft-repeated series of interruptions. Enthusiastically pursued by the pioneers of the seventeenth century, Wren, Boyle and Company, Major, Elsholz, Fabricius, Fracassatus, and others, the method was soon abandoned, voluntarily because of its difficulities and involuntarily in many places by virtue of the misgivings of both civil and professional authorities. Revived in the 1770s by Liberkuhn and Laesecke and others, it lapsed once more; revived in 1814 by Baron Percy and others, it declined again; revived in the 1840s by the experimentalists, Bichat, Blake, Magendie, Bernard, and others, it receded again; revived by Landerer in the 1880s, it had its last decline, largely because of the disappointments associated with the early efforts at chemotherapy. It was revived finally by Ehrlich (in 1909), whose signal success with Salvarsan in the treatment of syphilis and whose convincing demonstration that this drug could not be given effectively in any other way, placed intravenous therapy on the firm ground it has held ever since. It is almost true that each interruption and each renewal of effort marked the recognition of a hazard, on the one hand, and a partial solution to that problem, on the other.

FINALLY, let us consider the lessons learned indirectly from the prosecution of this procedure. It is a virtual impossibility to give the correct genealogy of facts, and so it is likely that in this account I shall be guilty of attributing both too much and too little to this endeavor. It would seem indisputable, however, that we have learned what we know about the desired characteristics a substance intended for injection should have as the direct result of making such injections: that is, it should be sterile and hence either sterilizable or available in sterile form, as is blood; it should contain particulate matter not at all, or at least not as large in size as the blood cells; as a corollary of this it should be soluble in water and in blood and should give rise to no particulate aggregates by agglutinating blood cells or precipitating plasma proteins; it should be isotonic, or nearly so, and without specific hemolytic effect upon the red blood cells; it should be nonirritating, or nearly so, to the vascular endothelium; it should have approximately the same pH as that of the blood, or the quantity to be injected must remain quite small; it should be free of pyrogen; and it should be nonantigenic. Many drugs that are injected intravenously as a routine matter do not meet all these criteria, some because we have no alternative, and some because the violation is intentional (e.g., hypertonic solutions as used for raised intracranial pressure). The organism has considerable tolerance for some departure from the ideal, particularly if care is exercised and undue haste avoided.

A large part of what we know about the blood arose directly from the efforts to use it as a material for venous injection. The vast scope of the subject of hematology precludes any recital of this achievement. A number of curious pharmacological facts were observed as the direct result of this procedure: for example, that drugs may have quite dissimilar effects in different species of animals (e.g., morphine was observed as depressant to dogs and stimulating to cats by the first pioneers); and that, although a number of drugs (snake venoms and curare, in particular) could be innocuous by mouth and deadly poisonous by vein, this was by no means a universal tendency, for cherry laurel was found to be innocuous by vein, although highly poisonous by mouth. As indicated earlier, the first intravenous injections of drugs were made for the purpose of solving, at least in part, the mechanism of the action of drugs, and such injections have been indispensable in later studies of this problem.

Intravenous injections led to the recognition of pulmonary embolism as a pathologic entity, as well as to that of air and oil embolism and embolic phenomena in general.

As a by-product of the attempt at intravenous feeding, Schmidt-Muhlheim observed in 1882 that the split products of harmless proteins may be highly toxic (producing what is called peptone shock). This observation, focusing attention upon the necessity of learning the nature of the ultimate nutrients to which the processes of digestion reduce our foods, may be considered in many respects the cornerstone of the subject of nutrition.

The observations regarding anaphylaxis, made incidental to the prosecution of this technique, are a similar cornerstone to the subject of allergy. The recital is by no means complete, but it seems enough to warrant the statement that seeking a new way to do something may lead to many and unpredictable adventures.

THE NATIONAL FERTILIZER ASSOCIATION

CHARLES J. BRAND

Mr. Brand (B.S., Minnesota, 1902) was born in 1879 on a farm in Lac qui Parle County, Minnesota. After an aggregate of twenty years in research, administrative, and consultative capacities in the U.S.D.A., he became executive secretary-treasurer of the NFA, a position from which he has only recently retired to work as an economic consultant on agricultural and industrial problems. In 1946 he founded at the University of Minnesota the Conway MacMillan Research fellowship in botany, honoring a distinguished scientist and beloved teacher.

THE great functions of the commercial plant-food industry are the preservation, restoration, and improvement of the nation's soils, increased production of food, feed, fiber, and other crop plants, and improvement of their quality. The industry encourages every good farming practice relating to soils and crops. Among these are use of good seed; growing desirable varieties; saving and effective use of farm manures and crop wastes; good tillage; crop rotations, including seeding of clovers useful in supplying nitrogen; plowing under green manure and summer cover crops; use of lime when needed; terracing and contour farming to reduce erosion; and use of insecticides and fungicides.

Too little is known by the public and farmers about the removal of plant food by crops and erosion. Few people realize the devastation of the soil that results from continual maximum crops. In 1946 the nation was blessed beyond anything that could be expected—and this on top of five or six prior years of bountiful harvests. We had crops of 3,380,000,000 bushels of corn, 1,170,000,000 bushels of wheat, 2,500,000,000 pounds of tobacco, and 478,000,000 bushels of potatoes. These, plus cotton, used about 70 percent of all the fertilizer consumed in the United States. Fruits and vegetables, oats, hay (including alfalfa), and sugar crops consumed most of the other 30 percent. Table 1 shows the draft certain crops make upon soil fertility.

The principal elements on which the present chemical fertilizer industry is based are nitrogen, phosphorus, and potassium. Other soil elements are important, but these are regarded as the major plant-food elements. Nitrogen is expressed as pure N (formerly it was expressed as ammonia); phosphorus, as phosphoric acid (P_2O_5); and potassium, as potash (K_2O).

Nitrogen materials include, among others, nitrate of soda, sulphate of ammonia, ammonia solutions, ammonium nitrate, cyanamide, urea, and, to a minor extent, such organic materials as tankages and seed meals.

Phosphorus in the United States is obtained almost exclusively from superphosphate, made by treating phosphate rock with sulphuric acid. There is also a minor use, tonnage-wise, of ammonium phospate, ground phosphate rock, bone meal, and basic slag. In western Europe basic slag is almost as important a source of phosphorus as phosphate rock. Before World War II, Germany alone produced more than 5,000,000 tons of basic slag, of which she used approximately 2,500,000 tons in agriculture, exporting the remainder to other countries.

Potassium is obtained chiefly from muriate of potash (potassium chloride), sulphate of potash, sulphate of potash-magnesia, and manure salts.

Secondary elements—calcium, magnesium, and sulphur—and minor elements—iron, boron, manganese, iodine, zinc, copper, and still others—may be required in some areas and for some crops. Carbon, hydrogen, and oxygen are also essential and are obtained from air and water. These three usually account for about 90 percent of the total weight of plants.

In the United States the chief use of commercial fertilizers is in the form of mixtures, or complete fertilizers, consisting of suitable compatible materials containing the three primary plant-food elements. Materials are described and sold by name and plant-food content; e.g., muriate of potash, 60–62 percent. Mixed or compound fertilizers are described by a series of three numerals, such as 5–10–5, 8–16–8, 5–8–7, 0–10–10. The 5–10–5 grade contains 5 units of nitrogen, 10 of phosphoric acid, and 5 of potash. The 0–10–10 grade contains no nitrogen. The unit is 20 pounds, or 1 percent of a ton. The effectiveness of fertilizers is frequently related to the proportional amount of each plant food present in a mixture.

TABLE 1

PLANT FOOD REMOVED BY CROPS*

CROP	YIELD BASIS	POUNDS PER ACRE			
		Nitrogen	Phosphoric Acid	Potash	Total
Cotton	250 lbs of lint, plus seed	18.7	7.3	7.5	33.5
Tobacco	900 lbs. of leaf	24.5	3.9	35.5	63.9
Wheat	15 bu., plus straw	29.6	19.2	27.9	76.7
Corn	30 bu., plus stover	47.2	20.8	42.8	110.8
Alfalfa	2 tons	94.0	21.9	89.0	204.9

* Per acre of stated yields.

Early organizations in the fertilizer industry. In 1860 there were 47 fertilizer manufacturers. The value of the finished product was $891,000. (It is estimated that in 1946 there were 800–900 manufacturers and that a total of 14,000,000 tons of commercial fertilizers and fertilizer materials, valued at the farm at $440,000,000, were produced.)

The first commercial fertilizer to enter world-wide commerce was guano, obtained chiefly from the islands off the west coast of South America. Consuming countries began importations in quantity in 1841. By 1852 no less than 20 countries were exporting guanos of widely variable plant-food content. Great Britain and the United States were the chief consuming nations. Great Britain imported 283,300 tons in 1844. United States imports rose from 1,000 tons in 1847–48 to 175,800 tons in 1853–54, of which 164,000 tons came from Peru.[1] The demand for guano was so insatiable that the best deposits were largely exhausted in three or four decades.

Trade and maritime conditions, state and municipal laws and regulations, tax problems, and other factors resulted in the organizing of "fertilizer exchanges" in the chief receiving ports, particularly Baltimore, New York, Philadelphia, Charleston, and Savannah. The utility of these exchanges grew as the list of fertilizer materials and compound fertilizers grew. By 1876 this list included nitrate of soda, phosphates, brimstone for making sulphuric acid, kainite, muriate of potash, and some organic materials like bones, fish, and tankage.

There were many common problems, especially those related to lack of uniformity in chemical analyses and fertilizer control by the states. As a result, the National Association of Chemical Fertilizer Manufacturers was organized in 1876 in Baltimore. This was the first comprehensive organization of which there is record. It met regularly for several years to consider "matters upon which our interests cease to be divided and become common" and after a discontinuous existence, became inactive.

On May 31, 1883, in the rooms of the Chemical and Fertilizer Exchange of Baltimore, the constitution of the first body, known as The National Fertilizer Association, was adopted. This organization was the successor of the one formed in 1876. The annual meeting for election of officers was held August 29, 1883, and three interesting addresses were made. One dealt particularly with the problem of chemical analysis, which continues to be a subject of active work even to the present day. Committees on transportation, trade rules, arbitration, state laws, agricultural chemistry, and so forth were appointed.

The new organization functioned until 1887. From 1888 to 1893 no general organization existed. In March 1894, at Columbus, Ohio, the forerunner of the present National Fertilizer Association, known as The Association of Fertilizer Manufacturers in the West, was organized. Its first annual convention was held at Buffalo, New York, November 20, 1894. At its eighth annual convention in June 1901, its name was changed to the Fertilizer Manufacturers Association. At the fourteenth annual convention, held during the Jamestown Exposition at Norfolk, Virginia, in October 1907, the name was changed back to The National Fertilizer Association, as it had been from 1883 to 1887.

If the old National Fertilizer Association that held its first meeting in 1883 had been continued, it would now be nearly sixty-five years old. The present organization, and its immediate antecedents dating from 1894, have been in existence nearly fifty-four years, a long period in trade-association history.

In June 1906 there had been organized at Old Point Comfort, Virginia, a regional association called The Southern Fertilizer Association. The National and the Southern, with a number of members paying substantial dues to both, continued their separate existences until June 1925. Then, after several months of negotiation, they were merged into a truly national body bearing the name of the The National Fertilizer Association.

This Association (hereinafter, with its predecessors, referred to as the NFA), was a voluntary, unincorporated body. Organization was perfected at White Sulphur Springs, West Virginia, in June 1925. On June 11 it held its first meeting, adopting bylaws, electing officers, and holding a brief program session. Its initial active membership of 145 companies covered the production of about 80 percent of the total annual tonnage of the industry, including superphosphate and compound fertilizers. By June 1926 there were 168 active members and 74 associates.

The bylaws provided for dividing the fertilizer-using territory, excluding the Pacific Coast states, into nine districts for the purpose of electing an executive committee, which was constituted the governing body. One member was elected to represent each district, and nine were elected at large at the annual convention. To assure democratic management, six members were from small, six from medium-sized, and six from large companies.

On June 1, 1927, the NFA was changed from a voluntary association to a corporation under the laws of Maryland. This action was taken in consequence of an antitrust action which did not involve the Association but did involve a number of its members. New bylaws were submitted to the Department of Justice to assure as far as possible procedures that would not be regarded as in themselves likely to run counter to the antitrust laws.[2] The former Executive Committee was discontinued, and a Board of Directors was established, with an authorized maximum of 21 members.

In 1927 California was constituted the tenth district, and on January 1, 1928, all manufacturing members of the California Fertilizer Association became affiliated with the NFA. Subsequently, Weller Noble, chairman of the Soil Improvement Committee of the California Association, was elected to the Board of Directors of NFA and made chairman of the Pacific Coast district.

This situation continued until the period of the National Recovery Administration in 1933–35, when the whole country was divided into 12 districts and the number of directors was increased to not less than 33. The Board is the policy-making agency, with absolute control of the purse strings, subject to a vote on the annual budget by the members in convention assembled. An executive secretary–treasurer was elected to administer the affairs of the Association under the Board's supervision.

Fertilizer-control legislation. Prior to 1869, no recorded attempt was made to institute any statutory control over importation, manufacture, and sale of fertilizer. For twenty-five years hundreds of cargoes of guano had been imported with no determination of their fertilizing value. Farmers and distributors had soon noticed that one cargo produced crop increases decidedly different from another cargo, despite the fact that physical appearance and price were the same. This led to so-called manipulated guanos, really mixed fertilizers, prepared by fortifying and balancing the natural product. Agricultural workers and the growing fertilizer industry were aware of the undesirable situation, and in 1869 the first fertilizer-control law was adopted in Massachusetts. Dr. Charles A. Goessman, then chemist of the Massachusetts Agricultural College at Amherst, was largely responsible for the adoption of this law. After receiving his training in agricultural chemistry in the University of Göttingen, he had come to the United States in 1857. Today, 47 of the 48 states have fertilizer laws. Only Nevada lacks one. In 1873 the Massachusetts law was revised because of its faulty character. Later, when fertilizer organizations had come into existence, many states passed new laws and revised old ones, often in line with the experience and recommendations of Association members.

A single case will illustrate the need of uniformity in chemical control. Potash salts began to come from Germany in the 1860s. By 1883 consumption of German potash in the United States approximated 25,000 tons. The technique of chemical analysis was so undeveloped that four reputedly experienced chemists reported the content of potash in a single fertilizer sample as follows: 0.50, 0.51, 1.28, and 1.65. Two chemists, evidently using too much heat, failed to find any potash at all on their first run. Experiences like this caused the NFA in the early 1880s to cooperate with the agricultural interests in the state legislatures to prepare a draft of a uniform law regulating fertilizer-control work. The officers of the NFA and of the Chemical and Fertilizer Exchange of Baltimore, particularly A. de Ghequier, then secretary of both, aided in drafting state legislation. In 1884 de Ghequier also assisted in organizing the Association of Official Agricultural Chemists (AOAC) at Philadelphia. He participated in the principal preorganization meeting in Atlanta in 1883 and was a member in its early years.

Baltimore is recognized as the cradle of the American chemical fertilizer industry. It has been the leading center of production since before 1850. Philadelphia was also an early manufacturing center. One of its first producing concerns, organized by John P. Baugh in 1855, operates today at Baltimore under the name of The Baugh and Sons Company. Another pioneer was G. Ober & Sons Company, of Baltimore, established in 1857. In later years it was acquired by The Davison Chemical Corporation, also one of the oldest in the industry, originally operating under the name of Wm. Davison & Company.

Before closing our comments on the early history of fertilizer legislation, it should be said that the AOAC, state chemists, commissioners of agriculture, and directors of experiment stations endorsed the NFA's efforts to obtain uniform state legislation. Dr. Harvey W. Wiley, for many years chief of the Bureau of Chemistry of the U. S. Department of Agriculture, and celebrated leader in pure-food legislation in America, was also secretary of the AOAC for many years. In an address at the annual convention of the NFA at Norfolk, Virginia, in October 1907, he stated with reference to fertilizer laws, definitions, methods of analysis, labeling, etc.: "In all these matters it is highly desirable that there should be unity of action by The National Fertilizer Association and the Association of Official Agricultural Chemists."

When the present NFA was organized,

one of its earliest activities in 1925 and 1926 was to redraft the model state fertilizer law in the light of all the state laws then in effect, and to assist states in adapting it to their individual needs. Work of this kind apparently never ends. The lead article in the November 2, 1946, issue of the *American Fertilizer* (Philadelphia: Ware Bros., **105**) was entitled "Proposed Uniform State Fertilizer Bill," with the subheading "Control Officials Association prepares measure; plant food content set at 20 per cent minimum; official list of grades included."

The annual volume entitled *The Fertilizer Movement During the Season 1885–86,* published by the NFA in August 1886, contained a full reprint covering 167 pages of the laws relating to the sale and inspection of fertilizers and the rules and regulations established under these laws. The laws of 24 states were reproduced. The NFA's interest in constructive legislation still continues. Every other year it prepares and distributes a detailed tabular analysis of all state fertilizer laws.

Cooperation between NFA and AOAC. This activity goes back to 1884, when the AOAC was organized. As shown by Dr. Carl L. Alsberg's Preface to the first (1920) edition of *Official and Tentative Methods of Analysis,*[3] it was organized by men well known in the field of chemical control of industrial chemicals, foods and feeds, and other products. A few of them, like A. de Ghequier, were not government officials. Later, as members were engaged almost exclusively in law-enforcement work, membership was and now is confined to representatives of governmental bodies.

With the continuing growth of the fertilizer industry and the increasing perplexity of its chemical control problems, the AOAC set up a special Committee on Definition of Terms and Interpretation of Results on Fertilizers. New developments were constantly taking place, such as manufacture of concentrated superphosphate, 43–48 percent P_2O_5 (1907, United States); air fixation of nitrogen by the cyanamide (1898–1901, Germany) and direct synthesis (1905–13, Germany) processes; production of more concentrated potash carriers; and ammoniation of superphosphate (1928–31, United States). The NFA cooperated constantly with the AOAC, making its representations from the industry standpoint, so that the AOAC rules and procedures would conform to the needs of the industry and the farmer.

Promoting uniform terminology and practices. Only a listing of some of the activities for promoting uniform terminology and practices is possible. They include—

1. Use of the term "nitrogen" instead of the term "ammonia."
2. Expressing fertilizer grades or analyses in even units.
3. Stating the plant foods in order of N, P, and K.
4. Use of the term "superphosphate" instead of "acid phosphate."
5. Including grade numerals as part of the brand name.
6. Increasing the minimum plant-food content in mixed fertilizers and superphosphate.
7. Reduction in the number of analyses offered for sale.
8. Simplification of guarantees and of printing on bags and tags.
9. Proscription of cutting of materials.

Space precludes extended discussion, but a few comments may be of interest. Formerly nitrogen was expressed in terms of ammonia, although few nitrogen carriers contained N in the ammonia form. As chemical analysis cannot determine with certainty how much of the nitrogen in a specific fertilizer is obtained from each different nitrogen source, an illustration of diversity in prices is illuminating. Cyanamide became freely available in 1910, the American Cyanamid Company having been organized in 1907. The nitrogen contained therein behaves the same as organic nitrogen (for example, in fish), but at that time a unit of 20 pounds of cyanamide nitrogen was valued at $1.00, whereas a unit of organic nitrogen, termed "organic ammonia," from fish, was valued at $3.00, and the unit value in cottonseed meal was $2.50.

In recent years unfriendly critics in and out of Congress have said much about the alleged "dirt," and "worthless," or "inert" material that manufacturers are accused of selling to the farmer. There is little merit in the accusations since farmers owning cooperative companies sell pretty much the same mixed fertilizers as do the individually owned and corporate units of the industry.

In earlier years there was more basis for complaint. Even twenty-five or thirty years ago some mixed fertilizers contained as little as 10 or 12 units of plant food. Grades such as 1-8-1, 2-6-2, and 2-8-2 were not unknown. The terms "low-grade" and "low-analysis" were then to some extent applicable to such goods, but this condition was partly a result of the low plant-food content of the materials then available.

When average plant-food content was probably about 14 percent, the board of directors of the NFA instructed its officers and staff to take every proper means to promote raising the minimum content first to 16 and then to 18 percent. If the content had remained at the 14 percent level where it stood at the end of World War I, the 14,000,000 tons used in 1946 would have had to be increased to about 19,000,000 tons. Think of the added freight, bagging, handling, and other costs!

At the 1921 convention of the NFA, in an address on "Economic Factors in Higher Analysis Formulas," Loren W. Rowell, Swift & Company, Chicago, said:

> A 2-8-2 was considered a high-grade fertilizer not long ago, and the man who buys a 3-12-3 . . . can be sure . . . he has a guarantee that a high-grade plant food was used. . . . If a man buys two tons of 3-12-3, he will get axactly the same amount of plant food as if he had bought three tons of 2-8-2. [At prices then prevailing it was] the same as a reduction in price on 2-8-2 of $5.00 a ton.

At the conclusion of Rowell's address, the NFA voted to recommend the use of higher-analysis fertilizers.

Later in the same program Professor W. D. Hurd, then director of soil-improvement work, discussing "Some Tendencies in Soil Fertility Thought and Research," said: "When the Soil Improvement Committee started its campaign three years ago toward the elimination of undesirable analyses, and asked for support for high analysis goods, some [agricultural college and experiment station workers] were lukewarm; others actually hostile."

The plant-food content of fertilizer materials now available ranges from 5 percent in some organic nitrogen carriers to as high as 70 percent in a concentrated material like ammonium phosphate, which contains two plant foods—N and P_2O_5.

When the superphosphate industry was young, 12 and 14 percent finished material was common. Today, the average content is about 20 percent P_2O_5. It is, of course, completely wrong to assume that the other 80 percent is "inert" and "worthless." It is largely calcium, and it is not too much to say that throughout fertilizer history in the southeastern United States, until a few years ago, farmers were almost wholly dependent on the lime in their superphosphate for quickly available calcium. In these days, calcium is recognized as a necessary plant food as well as a soil amendment. At present, ground limestone, especially dolomitic limestone containing magnesium, is used as a fertilizer filler in order to even-weight the ton for sales purposes.

Educational work by the NFA and the agricultural forces of the nation, and economic causes, have led to the almost complete elimination of low-grade, or at least low-plant-food-content, fertilizers. Gradually the industry won the cooperation of the college, station, and extension staffs for its efforts to increase plant-food content, and today the average has risen to 21 percent of N, P, and K in compound fertilizers. The savings achieved are easily apparent.

Not only did the NFA bring about the hearty cooperation of the industry, but it coordinated the efforts of all other affected agencies in these matters. Two national conferences were held under its sponsorship, the first at Louisville, Kentucky, in September 1927, and the second at West Baden, Indiana, in 1928. Both were under the chairmanship of the late Dr. Tait Butler, editor of *Progressive Farmer* (Raleigh, North Carolina) and formerly one of the most prominent state control chemists. Other groups, including the farm press of the nation, cooperated.

In the somewhat helter-skelter, competitive development of the industry, a tremendous number of grades or analyses came to be manufactured. In 1926 the NFA made a survey of the number of grades being made. Reports received included about 709 analyses, totaling 3,250,000 tons. The first 5 constituted almost 50 percent of the total; the first 10, almost 60 percent; the first 15, 67 percent; the first 20, slightly under 72 percent; the first 25, a little more than 75 percent.[4]

When the recently formed Association of American Fertilizer Control Officials settled down to business in October 1946, it recommended that future state laws restrict grades to a relatively small number, probably not to exceed twenty, and that the minimum plant-food requirement should be not less than 18 percent for available phosphoric acid, and not less than 20 percent for complete mixed fertilizers, except those that contain at least 25 percent of their total nitrogen in water-insoluble form obtained from plant or animal materials. An account of the creation of this Association is contained in *Commercial Fertilizer* (Atlanta: November 1946, 16–17).

During World War II, the Federal war agencies, in cooperation with manufacturers and state authorities, limited grades to as few as three mixtures in the case of Mississippi, plus a list of fertilizer materials applicable to all states.[5]

Statistical work. Early in its existence, the NFA recognized the value of statistics. In 1883 *The Fertilizer Movement of 1882–83* was published in Baltimore, the first of a series of annual publications. These volumes provide statistics of shipments from each important port or interior shipping center, giving tonnages by months and years for each railway serving fertilizer centers. They also provide figures on imports into the United States of crude brimstone, nitrate of soda, acetate, sulphate, phosphate, guano, bones, bone dust, and all other fertilizers. An interesting survey of the countries of destination is given for exports and imports; a separate chapter is devoted to the statistics of imports to the United Kingdom, with incidental information concerning a number of other countries. For some twenty years, the NFA has continued the statistics on fertilizer exports and imports and published them in its regular series of *Service Letters.*

As one of its early activities the NFA began the publication of the first comprehensive *weekly* wholesale commodity price index available in this country. It antedates that of Professor Irving Fisher, of Yale, and of course the present weekly index of the Bureau of Labor Statistics. For more than twenty years this index has been published regularly each Monday and appears as a separate release to the press and news ticker services and in the *N. F. A. News.* It is published in a large and varying number of papers, sometimes as many as 100. The well-known New York journal, the *Commercial and Financial Chronicle,* has published it every week since its beginning.[6]

The most complete record available on the monthly trend of fertilizer sales is that contained in the NFA report on fertilizer tag sales in 17 states. As these states use about 70 percent of the fertilizer sold, the report indicates the trend of consumption in the industry as a whole.

For many years the NFA was practically the only source of relatively complete national statistics of fertilizer consumption, which is the substantial equivalent of production, as there is very little carry-over of stocks. In *Recent Developments in the Fertilizer Industry,* published in 1930, the statistics were perfected by the use of all available data from 1910 to 1929. These were revised again in 1937 in *Fertilizer Consumption in the United States,* by Herbert Willett, then NFA economist. In recent years the data have been collected and published in cooperation with the U. S. Department of Agriculture.

Since 1925 NFA has issued a monthly report on production, shipments, and stocks of superphosphate, the most important single fertilizer material. Working in collaboration with the Bureau of the Census, these statistics are unusually complete, probably representing no less than 95 percent of total production. The combination of NFA statistics with those supplied by nonmembers is now published regularly by the Department of Commerce.

Trends in farm income and prices, and their effect upon fertilizer sales, have been given constant study also.

Government intrusion into fertilizer manufacture and distribution. Shortly after World War I, the War Department completed two nitrogen plants at Muscle Shoals, Alabama. One was a cyanamide plant that produced approximately 1,700 tons of cyanamide, sufficient to warrant delivery of the plant to the government, The second was a modified Haber-Bosch direct-synthesis plant. It was so imperfect that it never went into production.

Beginning with 1920, there was constant pressure to inject the government into fertilizer manufacture, using the obsolete Muscle Shoals plants. By 1928 the proponents of government operation made their most determined effort to overwhelm industry opposition. The executive officer of NFA prepared a series of eight full-page advertisements, which were run in Washington dailies and a few farm papers. In those days, such advertising to educate legislators was a new and little-used method of petition.

A second method was the appearance in Washington of large numbers of fertilizer executives who came for frank and open conferences with their Congressmen. This procedure was referred to in the press of the day as "open opposition, openly expressed." As many as 100 executives came at one time, and NFA boldly issued statements listing their names, addresses, and business connections. The industry's efforts were well received by both the public and the press. Again, in May 1930, full-page advertising was resorted to.

At first, proponents of government operation were determined upon nitrogen production. While the debate was going on, private industry expanded nitrogen production so rapidly that everyone realized that government production was unnecessary. Then emphasis was shifted to government production of large quantities of phosphoric acid, although for sixty years the United States had been the world's largest superphosphate producer. The industry made every proper protest, and it was not until 1935, when indirect government invasion through creation of the Tennessee Valley Authority was put into effect, that fertilizer manufacture was actually begun—about 2,000 tons of 43–45 percent P_2O_5. Nitrogen manufacture (ammonium nitrate) was not begun until World War II came with its tremendous requirements of nitrogen for explosives.

Research on chemical problems. Encouragement of research began in the earliest fertilizer industry organizations. Then the work related especially to problems of chemical analysis and chemical control, especially as to dependable procedures to determine the content of ammonia, phosphoric acid, and potash in materials and mixed fertilizers. With the formation of AOAC, this work was taken over largely by the state control agencies, agricultural colleges, and experiment stations.

About fifty years ago, manufacturers began to employ staff chemists. By 1908 their number was such that the annual convention of NFA voted to form a chemical section. About the same time the American Chemical Society invited industrial chemists to join it and asked NFA to assist in forming a Fertilizer Division of the American Chemical Society.

On July 7, 1908, NFA voted that each member-company should instruct its chemist to join the Chemical Society and cooperate with its Fertilizer Division. Cooperation still continues, and each year the Society devotes an important section of its program to fertilizer research, including control problems, methods for preventing reversion of P_2O_5, compatibility of various carriers of

N, P, and K in mixtures, and simplification of terminology.

The Chemical Control Committee, established many years ago, continues to handle relations with AOAC and fertilizer-control officials, and leads in handling chemical problems within the industry. The primary purpose is to protect the farmer and the manufacturer against fraud. The importance of chemical control is such that all states but one have statutes dealing with plant-food content. Every lot of fertilizer material used in manufacture must be tested, and every lot of mixed fertilizer sold must be guaranteed under severe penalties.

SOIL-IMPROVEMENT WORK

On October 12, 1905, at the twelth annual convention at Asheville, North Carolina, a special committee was appointed to devise means to obtain more funds to continue propaganda work. Statistics showed that the members produced about 3,000,000 tons of fertilizer annually, and the committee recommended a yearly contribution of 2 mills a ton, which would raise about $6,000. It recommended also that funds be sought from nonmembers and from miners and vendors of phosphate rock, pyrites, sulphur, nitrate of soda, and potash salts, and from manufacturers and processors of tankage and dried blood, cottonseed meal, fertilizer bags, etc.

At the seventeenth annual convention in 1910, discussions that had gone on for years came to a head. Following an address by Professor H. D. Haskins, Massachusetts Agricultural Experiment Station, Amherst, it was voted that propaganda work be started by fertilizer manufacturers in cooperation with the railroads, the experiment stations, and the state agricultural departments. In urging the policy that should be followed by the Propaganda Committee, Charles H. MacDowell, Armour Fertilizer Works, Chicago, stated that the work could be done better by the Association than by individual companies; that it should be conceived on broad, constructive lines; that it should be carried on by expert agronomists who could give unbiased information; and that the propaganda organization should intelligently, aggressively, and properly defend the industry against unfair attacks. Evidently, the perfectly good word "propaganda" soon lost caste, for shortly, when preliminary work began, John D. Toll, Philadelphia, was elected chief of the "Educational Bureau." Toll served the industry for many years, and after the death of William G. Sadler, Nashville, Tennessee, was secretary until 1925. From 1910 until his death in 1932 he had also served as editor of the *American Fertilizer*, a semimonthly trade magazine, and the *American Fertilizer Year Book.* C. A. Alling, Darling & Company, Chicago, served as soil-improvement chairman until 1915, when Warner D. Huntington, The Davison Chemical Company, Baltimore, was elected. He served for eighteen years.

The urge for educational work developed so rapidly in the Middle West that by the time of the eighteenth annual convention at Atlantic City in July 1911, President John T. Welch reported that he considered the formation of the Middle West Soil Improvement Committee of The National Fertilizer Association (Mr. Alling, chairman) the most important work ever undertaken by the industry. Mr. Toll was appointed secretary, and Professor Henry G. Bell, Chicago, agronomist.

In 1916 Bell was succeeded by Sidney B. Haskell, who came from Massachusetts State College, where he had been an instructor and professor of agriculture since 1904. He immediately selected as his assistant Ove F. Jensen, of Michigan State Agricultural College. In 1920 Haskell resigned to become director of the Massachusetts Agricultural Experiment Station, and Professor William D. Hurd, who had been his assistant, succeeded him. Hurd, also a graduate of Michigan State, had had a wide experience as an administrative and research worker in agriculture. He continued as director until his death in 1924, when he was succeeded by Harold R. Smalley, a graduate of Purdue University, who had entered the service of the Soil Improvement Committee at Kansas City in 1920.

Observing the useful work of NFA in soil improvement, the Southern Fertilizer Association began similar work in 1916. Both Associations conducted almost identical activities until they united in 1925. Thereafter until 1929 the Northern and Southern activities were continued in separate divisions. In 1929, when J. C. Pridmore, a graduate of Clemson College, director of the Southern work, resigned, Smalley was made chief agronomist and director of all soil-improvement work. He did distinguished work throughout his long period of service, including the great depression of the 1930s and almost to the conclusion of World War II. He died in February 1945; an account of his work may be found in the *Fertilizer Review* of January-February-March 1945.

Almost incredible changes have occurred in our agricultural institutions since the industry's educational programs began more than thirty-six years ago. Agricultural extension work was then in its infancy—there were almost no county agricultural agents, and no vocational agriculture teachers. It was impossible to foresee all the varied economic activities inaugurated in 1913 with the creation of the Bureau of Markets, U.S.D.A., and carried on by its successors, The Bureau of Agricultural Economics, the Agricultural Adjustment Administration, and the present Production and Marketing Administration. While there are only about 3,100 agricultural counties in the United States, the total extension staff as of September 30, 1946, was 10,868, including 4,409 white and 362 colored county agents and assistant county agents. There are also probably no less than 7,500 vocational agriculture teachers. The majority are agricultural-college graduates, engaged in teaching and advising farmers and their children concerning better agricultural practices, including the use of fertilizers. In one state alone a few years ago, county agents tested farm soils at the rate of more than 100,000 samples a year, and made fertilizer recommendations based on those tests.

Cotton was long the chief consumer of fertilizer. With government programs for reducing cotton acreage, it became necessary to find another market for the fertilizers formerly applied to cotton. In 1926 the country planted 45,839,000 acres to cotton; in 1946, less than 18,000,000. In the early years of acreage reduction, 700,000 tons of fertilizer had to find new outlets. The industry's cooperation with the agricultural forces switched consumption to pastures, peanuts, fruits and vegetables, and other food, feed, and industrial crops.

Soil-improvement work has covered many subjects and problems. Its constant aim has been to give the farmer the best information available to assist him in conserving soils and improving crops. It is possible to indicate its development only with some examples.

Research fellowships on agronomic problems. Research inaugurated or participated in by NFA attacked the whole problem of more efficient use of fertilizer materials and mixtures. Mostly, it took two forms: financing fellowships in certain universities and agricultural colleges and giving of grants-in-aid to institutions for experimental work.

In 1919 NFA began to finance fellowships devoted to specific problems. The first was established in Wisconsin under Professor Emil Truog, to study the effects of readily available fertilizers applied in different ways; e.g., broadcast, in hills, rows, under the seed, over the seed, at one or both sides of the seed, etc. The second was established in Vermont in 1920 to study the effect of different fertilizer ratios on the growth and maturity of crops, and it continued for three years. The third was established at Iowa State College to study the effects of distribution and concentration of plant food, and machinery for applying fertilizers to various crops. Later, a graduate research fellowship was established at Purdue University to study the effectiveness of fertilizers in controlling the root and ear rots of corn; another at the University of Maryland to study the economic use of fertilizers on the soils of the Norfolk series; and a number in Southern states.[7]

As an illustration of the second method, for several years around 1928–30 NFA appropriated $3,000–$5,000 yearly to the agencies concerned in the U. S. Department of Agriculture and the state agricultural colleges to start work promptly on determining the proper methods of applying fertilizers, especially to row crops. Ultimately, the Department financed the work, and at one time $80,000–$100,000 a year was spent, with tremendous economic results.

Research committee on fertilizer application. As a result of data obtained between 1919 and 1925, and at the suggestion of the late Dr. J. G. Lipman, director, New Jersey Agricultural Experiment Station, NFA created the National Joint Committee on Fertilizer Application. This Committee brought together the expert ability of NFA, the American Society of Agronomy, the American Society of Agricultural Engineers, the National Association of Farm Equipment Manufacturers (now the Farm Equipment Institute), and the American Society of Horticultural Science. Each of these agencies had competent representatives on the Committee. Its work continues to the present day, and new types of fertilizer machinery and remodeled old types are giving farmers the benefit of the research. There are still on American farms probably between 5,000,000 and 6,000,000 fertilizer distributors of one kind or another, the vast majority of which do not place the plant food at the place in relation to the seed that will give the farmer the highest economic return. This illustrates the magnitude of the mechanical problem.

Pasture fertilization. When NFA inaugurated its pasture work in 1926, almost no chemical fertilizers were used on American pastures. The Consumer Survey based on 1938 indicated that by then over 1,400,000 acres, or 3.8 percent, of the plowable pasture land in 23 states received some chemical fertilizer. Reports from 28 agricultural colleges in 1938 disclosed that about 80,000 farmers fertilized permanent pasture. In addition, about 1,000,000 acres of supplementary pasture were fertilized. Soil-conservation workers reported that thousands of acres of submarginal land were being fertilized for new pasture seedings. Many farmers, especially dairymen, found, according to the Survey, that each dollar spent for fertilizer returned $3.50–$5.00 in increased pasture yield. The work was supervised by Professor John B. Abbott, NFA agronomist for New England, with headquarters at Bellows Falls, Vermont. Subsequently, it was continued by various experiment stations. It showed that crude protein in barn-fed silage cost approximately $17.50 a hundred pounds, whereas crude protein derived from well-fertilized pastures cost approximately $3.50 a hundred pounds.

Educational publications. These were prepared for distribution directly to the agricultural forces and, through fertilizer manufacturers and dealers, to such of the nation's 2,500,000 fertilizer-using farmers as could be reached. Some of these publications are *Mineral Hunger in Livestock,* hundreds of thousands of copies of which were distributed; *Putting Plant Food to Work; Fertilizer Industry Forges Ahead; Growing Quality Tomatoes; Producing Low-Cost Cotton; Making Pastures Pay in the Northeast; Green Acres; Organic Matter: The Life of the Soil.* Since 1911, no less than 300 pamphlets have been issued. A careful estimate made several years ago credited the present NFA with distribution of more than 10,000,000 copies.

Hunger Signs in Crops. This is the title of a book published under the sponsorship of the American Society of Agronomy and NFA, and completely financed and supervised by NFA. It contains 9 chapters written by 14 distinguished scientists, who received no compensation; about 350 pages; some 80 full-page color illustrations; and 95 half tones. The NFA Board of Directors authorized the printing of 10,000 copies. Prepublication orders for 6,500 copies convinced the executive secretary that a larger edition could be sold readily, and he personally guaranteed the cost of an additional 4,000. Beyond doubt it is the world's outstanding text on the symptoms of malnutrition in crop plants. Nearly 22,000 copies have been sold, requiring a third printing.

Plant-food research problems. In June 1936, the Board of Directors authorized amending the bylaws to provide for a Plant Food Research Committee to consist of 15 industry agronomists. Among the committee's duties were coordination of research work already under way, and recommendation and conduct of new projects. One of its early activities was to assist in planning *Hunger Signs in Crops.* In the eleven years of its existence, the Committee has paid special attention to diagnosis of symptoms of malnutrition in crops, peanut fertilization, heavier-than-normal applications of fertilizer in crop rotations in the Middle West, fertilizing corn in the South, fertilizing fruit trees, plow-sole fertilization, study of granular fertilizers, soil testing, and quick methods of testing plant tissues to determine plant-food deficiencies.

Moving pictures. This activity was inaugurated more than twenty years ago. In a recent year 108 copies of three moving pictures in Kodachrome were in circulation. By 1945 they had been shown to more than 1,000,000 agricultural people in 46 states and several foreign countries.

Nitrogen utilization studies. In September 1942, Dr. William H. Martin, dean, College of Agriculture, and director, Experiment Station of New Jersey, suggested that when the war was over the United States would have a large surplus of nitrogen-fixing facilities, and that steps should be taken to ready agriculture and the industry for their effective use. Accordingly, the executive officer of NFA took steps to institute the necessary organization. The first meeting of the National Joint Committee on Nitrogen Utilization was held in Washington in the fall of 1942; the second, in Cincinnati in November 1943. More than 225 agriculture and industry representatives, including research agronomists and horticulturists from nearly all states, attended the second meeting. The Committee is composed of not more than 5 persons from each of the following organizations: Association of Land Grant Colleges and Universities, American Society of Agronomy, American Society of Horticultural Science, the Tennessee Valley Authority, U. S. Department of Agriculture, Society of American Foresters, and The National Fertilizer Association.

Some of many other soil-improvement activities can only be named: County-agent soil-improvement contests; conferences with agricultural workers; agronomic charts; filmstrip lectures; and preparation, circulation, exchange, and sale of natural-color slides showing nutritional deficiencies.

SURVEYS AMONG FERTILIZER CONSUMERS

In 1928 NFA, assisted by 56 companies and nearly 1,000 of their salesmen, interviewed more than 48,000 farmers on their experiences in 1927. *American Fertilizer Practices,* a report of 157 quarto pages, analyzed the returns. It disclosed how many and what papers are read by farmers; what bulletins on the use of fertilizer they received from state experiment stations; what booklets, folders, and advertising matter they received from fertilizer companies; the extent to which farmers themselves have conducted experiments with fertilizers; how many visited experiment stations, agricultural meetings, and demonstration farms; agencies helpful in selecting fertilizers; effect of fertilizers on crop quality; amounts of fertilizer used per acre; acreage of specific crops fertilized and quantities applied; and farmers' estimates of increased yields obtained by the use of fertilizer.

A second survey was made in 1939. *American Fertilizer Practices, Second Survey,* gave the results. More than 32,000 farmers were interviewed by 656 salesmen and field men. At this time farmers were spending nearly $210,000,000 annually for almost 7,500,000 tons of fertilizer. In 1946, according to advance estimates, they spent approximately $440,000,000 for more than 14,000,000 tons.

These surveys provided information that was indispensable in planning agricultural programs involving fertilizers during World War II.

At the request of officials in the U.S.D.A., a third survey was undertaken in 1944 to cover the period of World War II. Because of lack of manpower and gasoline, only about 15,500 farmers were interviewed. Additional data were obtained from 555 vocational agriculture schools in 42 states covering 13,600 farms. The results have been published in part in the *Fertilizer Review* and in mimeographed form.

Public relations. In the early years questions of national policy were handled by the NFA staff under the Executive Committee and later the Legislative Committee. When necessary special subcommittees were appointed, such as the Potash Committee, created in 1909 to deal with contract problems arising from the growing regulation of the potash industry by the German government.

After World War I (1920–26) the Washington Committee (operated in cooperation with the Manufacturing Chemists Association) handled such duties. Then many new problems loomed on the industry's horizon, including Muscle Shoals, a proposed Federal fertilizer law, the appearance of the first definite farm bloc, and a bill to appropriate $10,000,000 to finance government purchase and sale to farmers of Chilean nitrate of soda. This last proposal arose from the success that had attended a similar activity during World War I.[8] Since 1927 all such work has been carried on by the Public Relations Committee.

War work. During World War I, NFA was not staffed to provide the full services now expected of a trade association. The same was true of other chemical-industry organizations. In May 1917, the Council of National Defense set up a Committee on Chemicals. When the War Industries Board succeeded the Council, the Committee became the Chemical Division, of which Charles H. MacDowell was chairman. In the meantime, the chemical industries organized The Chemical Alliance, Inc., with 12 sections, of which the Fertilizer Section was the largest. The Alliance cooperated with the War Industries Board throughout the war. Horace Bowker, later president, The American Agricultural Chemical Company, New York, the largest fertilizer manufacturer, was the second president of the Alliance.[9, 10]

During World War II, the members and staff of NFA were in constant cooperation with the official agencies responsible for handling agricultural problems. The executive officer assisted in the selection of the Fertilizer Industry War Emergency Advisory Committee, and subsequently in the selection of contact committees for the War Production Administration, the War Food Administration, and the Office of Price Administration. As a matter of public recognition and historical interest, the names and connections of the industry executives who served the government through government-industry committees during World War II were listed in the *Proceedings of the Twentieth Annual Convention of NFA* (1944, 101–2).

Throughout the war and after, at the request of the government war agencies, NFA issued *Fertilizer War Notes.* These informed the industry and agricultural officials concerning supplies, distribution, government actions, policies, and regulations. They were sent regularly to more than 2,500 addresses. NFA also provided government agencies with statistical and other information as to productive capacity, processes, grades, plant-food content, rates of application, sales procedures, trade practices, materials, containers, and commercial and technological operations of the industry.

Dues and expenditures. Association funds have been raised chiefly through dues on tonnage of mixed fertilizers and superphosphate sold; through voluntary contributions from producers of potash, nitrogen, and sulphur; and from fixed annual dues of associate members. The rates of dues varied from 4 cents a ton (1925–26) to 0.75 cent in the depression (1931–32). They rose quickly during the period of the N.R.A code to 5 cents a ton, and later receded to 2.5 cents for the period ending in 1945.

In its first twenty years, the present NFA expended approximately $3,750,000 for all types of cooperative industry work. Soil-improvement work received the largest appropriations, totaling more than $1,250,000.

In 1925–26 total expenditures were $183,824. Owing to heavy emphasis on soil-improvement work, they rose to $243,839 in 1928–29. They fell to a twenty-year low of $58,312 in 1932–33, and then rose under the N.R.A. code to $276,735 (1934–35). During recent years they have ranged between $150,000 and $200,000. The average of expenditures for the period of twenty years was somewhat less than $185,000.

Variations in membership. Like some other associations, only a few of which still exist, NFA originated out of the economic depression of 1893. C. J. Judkins, Department of Commerce, has estimated that there were 120 national trade associations in 1894, 400 in 1910, 1,100 in 1930, and between 1,800 and 2,000 in 1946.

After the present NFA was formed in 1925, it had 168 active and 74 associate members. In the depth of the great depression in 1930–31, active members were 132 and associate, 54. The maximum number occurred in the period of the N.R.A., when there were 525 active and 44 associate members. In 1944, at the peak of the war there were 399 active and 70 associate members.

SPACE limitations prevent more than a mention of many additional activities such as insurance work, begun in 1909 and discontinued in 1933; cost accounting activities, begun in 1907 and discontinued in 1941; traffic and transportation work conducted throughout the life of NFA but carried on energetically since 1925; work on tariff problems, particularly prior to adoption of the Fordney-McCumber Bill in 1922, the Smoot-Hawley Bill in 1931, and when trade agreements were being negotiated actively in 1934–36; sales conferences and dealer education; surveys of sales opportunities; and defensive litigation such as the suit commonly known as the "South Carolina Compulsory Open Formula Case," which NFA carried through the Supreme Court of the United States; cooperation in trade practice work under Federal Trade Commission conference rules; preparation and administration of the National Recovery Administration Code of Fair Competition (1933–35); Department of Justice investigations (1939–41); and preparation of a bibliography on the influence of fertilizers on crop quality.

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ONE ATOM AND MANY*

J. H. MANLEY

Dr. Manley (Ph.D., Michigan, 1934) is on leave from Washington University to devote his time to the problems of atomic energy. Secretary of the General Advisory Committee to the Atomic Energy Commission, Dr. Manley divides his time between Committee duties in Washington and the Los Alamos, New Mexico, Scientific Laboratory, where he was formerly head of the Division of Research.

THE physical scientist is accustomed to speak of interactions. The physicist or the chemist will think of a hydrogen atom and of its interaction with another to form a hydrogen molecule. He will think of the interaction between hydrogen, carbon, and oxygen atoms as stabilizing a complex organic molecule. He may occasionally think of the interactions between many such molecules which, so far as elements go, make his own body. He will probably be cognizant of other interactions that make him a human being, but I am afraid that he oftentimes neglects those interactions, especially in the broadest sense.

The word "interaction" is a fine word. It will give me an excuse to say many things which are tied together by many kinds of interactions. There has been much written about the interaction of science on human society; there has been much written about atomic energy and the atomic bomb. Perhaps I have been unduly impressed with this development because I have had a part in it, but, nevertheless, it is my thesis that in this endeavor we have a most striking illustration of the interaction between the achievements of science and the individuals and institutions of society.

I should not like simply to repeat what has already been said, although that is, to a certain extent, unavoidable. At least I may take a different approach. Let me take one atom, the one called U-235 and first of all consider the relation between this one atom and the many atoms that make a nuclear physicist—myself. Later, lest I be thought selfish, I shall use the many-atom concept to denote all of us.

My story opens on June 10, 1940. On that day this particular assembly of atoms held together by reasonably well-understood interactions, this nuclear physicist of whom we speak, was in his laboratory investigating a relatively new interaction, the interaction of neutrons with atomic nuclei. Suddenly, from the radio: "FLASH: Mussolini attacks France." Sound waves to the ear, response of the aural nerve, messages in the central nervous system, muscular reaction—a kick and a dent in a can of borax powder—the physical evidence of an interaction between an event 5,000 miles away, this particular nuclear-physics laboratory, and a scientist in it. One could hardly attach profound significance to this little incident at the time, but now it seems somewhat symbolic. World events had invaded the laboratory. Work in that laboratory for the pure, unadulterated benefit of science did not long continue. Everywhere scientists began to leave the laboratories and the academic halls. A strong interaction between society and science, between situations and the scientist, was making itself felt. With other things of science, the interaction of a neutron with a nucleus was no longer just a matter of scientific curiosity, it was part of a matter of life or death for democracy—an institution of society based on the dignity of the individual. In time the apparatus of that laboratory, including the dented can, found its way to a New Mexico mesa, a war-born Shangri-La, Los Alamos. So did the physicist, but by a more devious route. He found himself at the newly established Metallurgical Laboratory of the University of Chicago, to use his acquaintance with neutrons for something more than just the publication of an article in a relatively little-read scientific journal.

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[*] From the John Wesley Powell Memorial Lecture, given at Colorado Springs on May 1, 1947.

Although nuclear physics and the workings of a nuclear physicist are familiar to many, it may be neither uninteresting nor irrelevant to our subject to digress in order to indicate the role of the nuclear physicist in attaining that outstanding scientific achievement—the first sustained nuclear chain reaction, the continuing, controlled release of nuclear energy. In so doing, however, I must emphasize that his role was that of a member of a team comprised of other physicists, of chemists, mathematicians, biologists, metallurgists, engineers, geologists—representatives of nearly every branch of science.

What is this chain reaction, what makes it go, what do neutrons have to do with it? It would hardly be appropriate to expand to a graduate lecture on nuclear physics, but perhaps with a little fancy we may approach an understanding of the nuclear physicist's role.

IMAGINE a village populated with U-235 atoms, female elements, if you like, homebodies that have an interest in a particular physical process—fission. In this village are some neutrons, male elements, rather averse to being tied down and rather interested in a bit of philandering. All atoms have strings attached, electrical interactions that make them interested in electrons and in other atoms, but the neutron is an exception. He goes to the heart of matter, the nuclei of atoms, before he shows any interest in the other residents of his world. So our poor, lonely U-235 atom, like all others, must wait patiently for her chance to capture a wandering neutron. But, unlike others, when she succeeds, the marriage is consummated by a rather unique event: Lady U-235 disappears with a flash of energy into two radioactive wenches and a few neutrons. The word "few" is quite important here, for if "few" did not mean "more than one" the chain reaction would not be possible and the world would have to get on with more conventional kinds of energy.

Assuming that our village possesses enough U-235 residents so that the number of deaths does not appreciably affect the number of marriages for a long period of time, then the problem of obtaining a chain reaction is the problem of making the neutron population grow to a desired value and then become constant. Unfortunately for the ease of attainment of this goal and for the satisfaction of Miss U-235, a wandering neutron is subject to a number of possible diversions that may prevent the establishment of a brief, but presumably happy, home life with a U-235 atom, and the birth of a new generation of neutrons. He may wander off into the woods around the village and die a hermit, having had no truck with the beauteous U-235. He may die in the arms of seductive but sterile atoms that await him along the graphite streets of the village. One particularly unfair but interesting cause of his death occurs in the same household with Miss U-235. She has an older sister, U-238, normal life a few billion years, not without charm of her own, but she begets no neutron children. Her union with Mr. Neutron produces the new element plutonium.

Nature has provided the elements for a stable population—the neutron and the fertile U-235 atom—but she left to man the task of solving the problem of village design. It was necessary for the scientist to collect suitable constructional materials, lay out the streets, put in plumbing, assign quarters to the Misses U-235, shelter the Misses U-238 to give the younger sister a chance, remove the dangerous sirens intent on enjoying the company of a neutron without the responsibilities of a younger generation, and make the village large enough so that only a few neutrons would find their way beyond its limits. And then, of course, since the scientist wished to control his planned society, he provided a troupe of itinerant chorus girls, the Cadmium Sisters, to go into the village at any time and calm any tendency for things to get out of hand between the neutrons and the Misses U-235 by taking out of circulation some of the neutrons of each generation.

As everyone now knows, after months of strenuous effort on suitable materials and different village plans, the goal was achieved on December 2, 1942. On that day the planning of the scientist bore fruit. Some of the Cadmium Sisters were called out of the village, more neutrons found U-235 mates, the population increased, and then the village settled down to a stable population of determined size, each birth exactly replacing a death, and each generation contributing to the power output and plutonium production of the village. The interaction of the scientist in the life of one atom had given a new form of energy to the many human atoms that populate this sphere.

The public did not hear of this event, the press teletypes were not full of the momentous news. Mussolini had not just attacked distant France, but Tojo had attacked Pearl Harbor. An interaction between elements of human society had cloaked in military secrecy an achievement as important to mankind as the discovery of fire.

Four months later the locale changed. Our nuclear physicist, with a truckload of that laboratory apparatus of which I spoke, was riding up a narrow, crooked, bumpy road toward the top of a New Mexico mesa. The problem ahead was the construction of a new planned village, a village in which the neutrons do not stroll, they run; a village of no streets and spacious lots, but of closely packed U-235 atoms. The object was to arrange that the whole village be consumed in one sudden burst of energy in an amount more than that stored in thousands of tons of TNT.

To do this there was assembled the best-equipped nuclear-physics laboratory in the world—Van de Graaff generators, high-voltage machines, cyclotron, betatron, chain-reacting units. And this was only the physics section. There were facilities for chemistry, metallurgy, and various strange new processes. It is my guess that that laboratory turned out the equivalent of a Ph.D. thesis every week or so. In the mind of each scientist was always the question, Is there time? There were twenty-four-hour shifts, frayed nerves, depths of gloom, and heights of achievement.

I find it difficult to limit myself in telling this story, so much was packed into those few years. There were not only the things of interest to the scientist, there were things of interest to any curious individual. We might explore, for example, the social interactions within this new isolated community of Los Alamos—a community in which it was the policy to encourage wives to work, not just to keep the number of inhabitants as small as possible, but as a preventive treatment against the dangers of an enforced isolation of which they did not understand the purpose. Travel, except for business, was permitted only within a well-defined region. The world for us was bounded as it was for the ancient Pueblo Indians—by the North, South, East, and West Mountains. City attractions were supplied by Santa Fe, but, even there, if a lone scientist wished to relax in the La Fonda bar, he had first to peer in and count the number of occupants from Los Alamos. If there were five he might join them in comradeship; if there were six he had to drink in solitude with no glance of recognition toward his fellow-citizens.

But I must carry my story on to the spring of 1945. The technical work had changed by this time. Scientists were designing and building special recording equipment. I was no longer studying the behavior of neutrons; a giant popgun for making shock waves had taken the place of a neutron source in my laboratory. As the spring progressed there was a marked decrease in the male population at Los Alamos. One wife would say to another, "I haven't seen Bob lately," and the other would reply, "He's away." No more than that, for that was all she knew. Then a few days later Bob would appear, kiss his wife, and make a beeline for the shower to rid himself of sweat, grime, and gypsum. Bob had been at "Trinity."

Trinity was the code name for a corner of the Alamagordo Bombing Range; there in the desert, shelters were being constructed, field stations laid out, miles of wire strung. It was quite a place, a few hundred square miles of sand and bush under close guard and alive with scientists and their helpers. I was told that, if a lonely sentry issued a challenge to some nocturnal disturbance and the reply was "Moo," no further challenges were given, but a shot would ring out in the stillness. My only scientific observation in this regard was that the camp seemed well supplied with beef.

In early July it appeared that if we could keep up the effort we might make a favored date. It was not simply a case of firing when everything was ready; weather was an important factor. Our meteorological section favored July 16, and the weatherman was respected since none of us was disposed to have a deadly radioactive cloud settle down around his head. It was a grand rush, but the nights before the fifteenth found us making dry runs—complete tests of every operation except a firing signal all the way through to the bomb. The betting pool on our success or failure was closed. Many a scientist had gambled a dollar on his ability to predict the number of tons of TNT equivalent. There were some of little faith, for the number zero was not without a few dollars.

The evening of July 15 brought a fine collection of desert thunderstorms into our valley from every direction. Had our medicine man given up the ghost? Lightning struck around the tower on which our gadget rested uneasily, and this did not contribute to the peace of mind of those working underneath. Would we fire? The weather certainly looked anything but good, but it would be days before conditions would be favorable again. For both photographic and meteorological reasons the shot was to take place at night, and 3:00 A.M. had been chosen. In the early hours of July 16 it was finally decided to postpone the firing time to 5:30 A.M.

I was an occupant of the shelter at West 10,000, six miles from point zero and the tower holding the product of not only Los Alamos but Berkeley, Chicago, Hanford, and Oak Ridge. The doors of the shelter, nailed open as a precaution against the blast, looked away from zero. The inside was lined with apparatus to record nearly every effect imaginable—blast, light, heat, earth shock, and radioactivity. Near the door was the screen of a projection periscope on which we would see what happened at 5:30.

By 3 o'clock the strain of the past days and weeks and months was beginning to tell. I was the only one awake in this shelter and I would have gladly given up the responsibility for the shelter in order to catch a bit of sleep. Everything had been checked; this was no time to speculate on the interaction between this event and the world, and sleep was a very sensible thing.

At 4:50 I woke the forms huddled on the floor; the guards came in from the area and the roads; each individual assigned to this shelter was accounted for; and similar reports from the other two shelters brought the announcement that the area was certified clear, the arming had been complete, everyone was under shelter except those ten miles or more away. The loud-speaker continued announcing time—minus 20 minutes, minus 15, and so on. At minus 45 seconds the robot mechanism removed all human responsibility except for that which would result at time zero. I remember vividly minus 2 seconds, then time seemed to stop.

I have been told of John Wesley Powell, his trip, and his difficulties in describing the country around the confluence of the Colorado and Green rivers. I know how he must have felt. There are sights that do not transfer easily to the written or spoken word. I have never seen the light of 1,000 magnesium flares, but perhaps what I saw that morning was comparable. For a brief instant the region around point zero was hotter than the sun. Our periscope screen was blinding. The whole valley, seen through the open doors of the shelter, was as bright as at noonday. I think I said, "Gentlemen, you have seen something. Stay inside for the blast." For a full half minute we waited while it sped across six miles of sage and yucca. Then suddenly there was a wind in our shelter and a roar as the sound filled the whole valley and echoed back from the mountains. Everything was over except for dashing outside and watching the colored cloud mushrooming into the dawn sky.

When the blast came, Enrico Fermi, outside at ten miles, was dropping bits of paper from his hand. I suppose his companions thought the usually imperturbable Fermi was nervous, but from the angle of fall of the paper as the blast swept by he quickly announced an approximate figure for the energy release of the bomb. The lack of that genius forced me to climb to the top of the shelter and examine one of the microbarographs there. The result was clear, the optimists in the betting pool had won.

The El Paso paper carried a few stories of a strange event: a locomotive engineer on the Santa Fe had seen a great light in the sky; a woman, driving eastward from Arizona into New Mexico thought that the sun rose, set, and rose again that morning. The next day there was an official announcement: "An ammunition dump in a remote section of the Alamagordo Base blew up with pyrotechnics. No one was injured." This was the public announcement of the most astounding experiment in history—the sudden release of a vast quantity of energy stored in quite a small package, the energy of matter itself. The scientist had brought to earth for a minute fraction of a second the temperatures and pressures of the stars.

In the high councils of government the news was startling—the two-billion-dollar gamble, authorized by one who had faith in knowledge, had paid off. The way was now open to bring the war to a swift but awful conclusion. The first application of atomic energy in the affairs of men must involve the death of some that others might live. One atom was no longer a scientific curiosity—it was to show its interaction with man in a most drastic, vivid, and fearful way.

MUCH of what I have related is already well known. Surely everyone is familiar with the events from that July day until now. Let us therefore glance at the future. A scientist does not like to do this—he prefers to leave it to the feature writer—but I have presented a thesis: that the development of atomic energy is a most striking example of the interaction between the achievements of science and the institutions of society. Our story has suggested some things; we must guess at the future for others.

I have the feeling that most people have been sufficiently exposed to forecasts of the effect of the military application of atomic energy and that they would be pleased if predictions concerning atomic war were omitted. This I shall do.

I also have the feeling that I may be expected to paint a rosy picture of the future with atomic energy as a source of energy, of a future with autos and home-heating plants run by uranium pellets, of an age of plenty based on atomic power. This I shall not do. In fact, I cannot. If I am pressed, I shall say that much of the speculation on this aspect is simply impossible because it is incompatible with the facts of nature. Atomic power is possible, yes. A plant that is not small can be built. Its waste heat might heat your homes. Its power might charge a battery to run your electric car. It will not be a strikingly cheap source of power, and it will not be available tomorrow or in one year. My guess is that in ten years we should see a successful unit producing some kind of conventional power—electricity or steam—on a modest commercial scale. I do not believe that these statements are ultraconservative or unrealistic. If I am being unwittingly biased, it is because I am frequently distressed by excess enthusiasm in things I read, things usually written by those who have no intimate connection with the development work in this field.

Does this mean that the whole phenomenon of atomic energy is much overrated? I think not. As a source of energy it will be important—how important is difficult to say. But energy is an obvious feature of this new discovery. I should like to consider some others that are less so, but which, to my mind, hold great promise of benefit to mankind. Since we are accustomed to think in terms of physical benefits—health, leisure, and technological sedatives for the difficulties of living—let us choose a few examples, examples obviously chosen by a scientist.

One of the aspects that interests me is our sudden wealth in available neutrons as the result of the operation of chain-reacting units. Of many things that can be done with neutrons, I shall choose two and merely suggest where these two things may be the beginning of new paths in our world of knowledge.

A hydrogen atom in a sodium hydride crystal has revealed its presence by diffracting neutrons. X-rays, which we usually think of as waves, have long been a useful tool in revealing the locations of atoms in molecules. But X-rays, electromagnetic in nature, are affected by the electrons of an atom, and the one electron of the hydrogen atom produces only a small effect. The neutron, on the other hand, pays no attention to the electrons of an atom, but only to the nature of its nucleus. For a neutron, therefore, the nucleus of a hydrogen atom may be quite as important as that of some other element. If such a neutron-effect helps us learn about the location of hydrogen atoms in complex molecules, who knows what may derive from this new knowledge?

There is another curious but true fact about neutrons. While bounding around inside a material at room temperature, they may have temperatures even up to and beyond thousands of degrees centigrade. Temperature has been a tool of the chemist and the chemical engineer for a long time. Can neutrons provide a cold heat which may be useful in catalyzing new, interesting chemical reactions? It would hardly be sound to think of our crude oil of the future being cracked to gasoline in a bath of neutrons, but many chemicals are used in far less bulk than gasoline. Who knows what will happen if an atom is knocked out of a hormone by a hot neutron?

Another aspect, one that is always mentioned, is the greatly increased abundance of radioisotopes. Since many uses have already been publicized, I shall take but one example.

Through the use of radioactive carbon it has been established that a certain enzyme can utilize carbon dioxide directly in the production of more complicated molecules. I am not a biochemist or a biologist—I've even forgotten which enzyme and which molecule—but I am intrigued by the thought that other living organisms than green plants may perform similar functions. This is but one example of a possible path along which this new tool may lead us to a greater understanding of the processes in living things, perhaps even of life itself. Small wonder that an eminent biologist has characterized the radioisotope as the most important tool in his field since the discovery of the microscope.

When these examples are multiplied, as they can be, I am led to think that the new things that have been brought to science and to scientific research by the release of atomic energy may prove of greater benefit to all of us than just the energy itself. At least we must not lose sight of these possibilities.

I have said that we are accustomed to think in terms of the physical benefits of atomic energy. I am afraid that this is largely true, but I am not happy that it is so. Our story began with an interaction between a political event and a tin can in a laboratory where a purely physical interaction between neutrons and nuclei was being studied. It continued with the interaction of a neutron-U-235 system on a nuclear physicist within a framework supplied by the political and social interactions of World War II. It has attempted to suggest possible future interactions on the welfare of men arising from the development of atomic energy. Is this where we stop? Are we, you and I, uninterested in anything more from a great scientific event? Can it be that we are so enchanted by technical achievement that we see only that? Are we not individuals concerned with interactions between ourselves and our fellow-men?

The road of history along which we travel is bounded by shadows. Occasionally a great achievement or a great person throws a spot of light into the shadow, which illuminates not just the future things for man but his future nature. The light is not seen by many, only those whose eyes are rapidly adaptable. An awful light in a New Mexico desert has made some close their eyes in fear. Its brilliant illumination has made others see more clearly than ever before the import of a faith which is of the nature of man—the faith that he, within himself, has the power over his own destiny. What clearer example could he need than his ability to release the very force that holds the elementary particles of matter together, that makes this earth through the energy supplied by our sun a living place for human beings?

Recent events will, I think, give us a picture of where, as men, we are. It would be unfair if I were to gloss over those that have been and remain discouraging: The wrangling over domestic legislation for atomic energy; the failure to reach international agreement on control; the sorry spectacle of

Congressional debate on the confirmation of the Atomic Energy Commission; the fact that we are still committed to the insecure path of security through superiority in atomic armaments. But there are things that are, after all, encouraging: The mere fact of the writing of the Acheson-Lilienthal Report, with its meeting of minds of scientist, businessman, industrialist, and public servant; the position before the world in the Baruch proposal that this nation is willing to restrict its national sovereignty for the benefit of all; the passage of national legislation with the wording,

> it is hereby declared to be the policy of the people of the United States that . . . the development and utilization of atomic energy shall . . . be directed toward improving the public welfare, increasing the standard of living, strengthening free competition in private enterprise, and promoting world peace.

I want to believe that these words of the Atomic Energy Act of 1946 are not to be interpreted within the narrow framework of technological benefits, but that they are a statement of recognition by our people that science and technology must have a moral purpose—they must continually increase the importance of the individual.

I suspect that John Wesley Powell in his search for land for his fellow-men must have seen some of the vision—the dignity of man as reflected in his self-sufficiency, his control over his own destiny on his own land in harmony with the nature around him. Science and technology expand the need for harmony—what Powell could suggest and one man could do for himself seventy years ago, a T.V.A. is doing for many men today. I am inclined to the thought that it is not a mere act of chance that the man responsible for carrying out the vision of T.V.A. is now Chairman of the Atomic Energy Commission. It is my hope that atomic energy, involving the ultimate force of our physical world will stimulate and combine with the visions of men to mean far more in the way of human dignity throughout the world than any of us can now foresee. It will be a part of our life and our obligation to watch and understand the interaction of one atom on many.

THE JOHN WESLEY POWELL LECTURE

The John Wesley Powell Lecture was established in 1929 by the Southwestern Division, American Association for the Advancement of Science, in honor of the first man to explore the Grand Canyon of the Colorado River. The Powell Lecture is delivered on the opening night of each annual meeting of the Division by a distinguished investigator in some field of science, on a topic of his selection.

Major Powell's interest in the natural sciences was manifest early in life. When his family moved from New York to Illinois, Powell joined the Natural History Society, making collections and serving as secretary.

After the War Between the States, in which he lost his right arm at Shiloh, he was appointed professor of geology at the Illinois Wesleyan College. There, concerned with the future welfare of other comrades handicapped by war, he remotely envisioned the basic principles of land settlement and reclamation now practiced in the Western states. Pursuant thereto, in 1867 and '68, he led geological expeditions into Colorado and Utah, making comprehensive studies of the climate and the land and water resources of the upper Colorado River basin. Inability further to follow the Green and Colorado rivers by land through their rugged canyon courses led to his plan of exploration by boat. In May 1869, under the direction of the Smithsonian Institution, Major Powell began his Geographical and Geological Survey of the Colorado River by boat. In the course of this expedition his party passed by boat through the entire length of Grand Canyon—a hazardous feat first described in his *Exploration of the Colorado River of the West, 1875.*

Major Powell was later engaged in geological and ethnological explorations in Arizona and Utah and from 1874 to '79 carried on an agitation for a reorganization of the many rival surveys in the West. This led to the establishment of the United States Geographical and Geological Survey in 1879. He was at once made director of the Bureau of Ethnology and two years later became director of the entire Survey. He contributed importantly to organization of the early work of the Survey, and his many valuable contributions to ethnology are found principally in its reports.

Major Powell resigned in 1894; in 1902 he died at the age of sixty-eight.—(*From the introduction to Dr. Manley's lecture by Dr. Jesse L. Nusbaum, senior archeologist, National Park Service, and consulting archeologist, Department of the Interior, Santa Fe, New Mexico.*)

THE PRINCIPLES OF POOR SPEAKING

HAROLD F. HARDING

Following World War II, in which he served as a colonel in the Pacific theater, Dr. Harding (Ph.D., Cornell, 1937) studied on a Rockefeller postwar fellowship in the humanities. He joined the Department of Speech at Ohio State in 1946. Dr. Harding was recently elected editor of the Quarterly Journal of Speech.

CONTRARY to popular opinion, it is fairly easy to write or to speak well. But writing poorly and speaking poorly are really difficult. To give these arts their proper due requires hard study.* Many accomplished practitioners of poor speaking are not aware of their methods. It is time that poor speaking be given the systematic treatment it deserves.

The serious student of poor speaking can well afford to consider these first principles:

1. Make no preparation in advance.
2. Give the speech no order; let it ramble.
3. Avoid a conclusion.
4. Mumble your words and don't look directly at the audience.
5. Never analyze an audience and never evaluate your performance.

PLANS AND STRATEGY

Preparation is irksome and time-consuming. Therefore, don't prepare until the night before you are to deliver the speech. Better still, give your speech impromptu and do your preparing on the spur of the moment. A sensible plan is to give again a ten-year-old lecture without revision.

Never make a study of the kind of people in your audience, and don't adapt your speech to their intelligence or their interests. When you begin, don't bother to define any new technical terms. Throw out three of four new words at the start and give them unusual or unheard-of pronunciations. This will distinguish you as an erudite speaker.

In planning an introduction, don't attempt to narrow down the subject of your talk. Cover the whole field, giving the early and the recent history. Don't omit a detail. Ability to dissertate on the entire background will mark you as a person thoroughly familiar with the tradition of your subject.

Read your speech, it's far more scholarly; don't try to master its ideas in outline form —that's the surest way to keep your audience awake. Whenever you read from manuscript, don't read it aloud beforehand. This will enable you to speak with your nose close to the manuscript. If you do weaken and speak extempore with the use of notes, don't go through the speech in advance; above all, pay no attention to any set time limit. If you use up your allotted time in the introduction, you can always invade the time of the rest of the program. This adroit maneuver will serve to delay the proceedings, and it will certainly cause people to remember you as the speaker who was full and flowing over.

Modesty is old-fashioned, so use "I" frequently. The word "my" at the beginning of successive sentences always attracts attention.

Personal appearance has little relation to what an audience will think of you. Be sloppy in your dress, or be flashy, as you prefer. Stand directly behind the speaker's stand. Have the light adjusted so that it restricts the audience's view of you while speaking; then begin to mumble, holding your head in an attitude of reverence.

GENERAL RULES OF COURTESY

Be late in arriving for the session and make enough commotion to attract notice when you enter. Be sure to shake hands with friends on the aisle while your predecessor is talking.

If a public-address system is available, avoid it. If one is not available, complain that you cannot speak decently without one; then proceed in a slow monotone. If you speak before a microphone and to a radio audience, let the audience before you go hang. Your outside audience is greater and, naturally, far more important.

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* See the enlightening article, "The Principles of Poor Writing," by Paul W. Merrill, in THE SCIENTIFIC MONTHLY, January 1947.

As for tempo, or speaking rate, try to cultivate extremes of either 75 or 200 words a minute. Avoid 125 words per minute—it's a dull rate, and, if you should enunciate distinctly, you gamble on having your hearers understand individual words.

If you have a specific purpose, conceal it. When you use charts or diagrams, make them small and the lettering faint. Talk to the chart rather than to the audience. If you use slides or film strips, make certain that the projector does not function. This will allow you to make small sketches on the blackboard with your back to the audience while you talk in a low, confidential voice.

Writers of textbooks on speaking always harp on "conversational quality." It's a flat failure in poor speaking. To succeed, either talk to yourself or make an oration. Conversing directly with the audience is just another one of those impractical modern theories.

A sure-fire stratagem is to ensure somehow that your hearers are physically uncomfortable. If it is a warm day, see that the windows are kept closed, for poor ventilation lulls people into thinking (but not about what you are saying). Don't forget to arrange for strong lights in the audience's eyes. This is the same device used so successfully in the third degree. Again, have the folding chairs wedged so closely together that there is no room between persons. Don't ask those in the rear of the room to come forward—it's vital to keep the audience scattered.

Insult your listeners. Either explain theories with which they are already familiar or tell them they wouldn't understand if you did explain. After all, they can always read your book if they want real enlightenment.

Do not exert your lung power. If you wish to succeed, speak so that the man in the back row wishes he were in the front row, the man in the front row wishes he were on the platform, and the man in the middle of the room wishes he were back home. This is known as complete coverage.

If you know you have only five minutes left, triple your rate and get in every word. Don't lose a single sentence.

When the speech is over, forget it. Don't recall audience reaction during the speech. It is nobody's concern, except possibly your own, whether you made yourself clear or whether you persuaded anyone.

THE SPEECH

Use long and involved sentences throughout. Join clauses with *and, but,* and *however* frequently. If your sentences run about fourteen words in length, you risk becoming downright perspicuous. Shape them into rounded periods like those of Edmund Burke and William Pitt. A sentence is scarcely worth uttering if it is less than 150 words long.

Stretch out the speech. Repeat your points. Present the same idea in any number of different ways. Then backtrack and start all over. Never organize your speech—it's too confining.

Avoid humor like the plague. If you tell stories or anecdotes or capitalize on amusing incidents of the meeting, you will be marked as an unlearned and unscientific person. Try to stupefy. Look dull and act the part. (It may take less effort than you think.)

Whenever possible use anticlimactic order. For making a reputation, there is nothing like letting down your listeners. Aristotle says that a speech should have a beginning, a middle, and an end. He was right about the the first two parts, but the best poor speeches really have no end. Those who unload them just go on and on. The past masters cultivate the false, or pseudo, conclusion. You, too, can temporarily arouse your audience with such phrases as "In conclusion," "To summarize," "To conclude briefly," "Let me now restate," "I want again to recapitulate."

Don't leave any time for questions or discussion at the end of the talk. But if you are caught unawares, give one or two curt, flippant replies and sit down. Sharp controversy, don't forget, becomes a speaker, and if anyone should disagree with you the weapons to use are sarcasm, disregard of the main point, argument *ad hominem,* and some ill-natured questions of your own in return.

The best-known speakers have become personalities. Audiences seldom remember what they said. The moral for speakers on scientific subjects is: Let your audience remember you, the speaker, and not your speech.

The rules for poor speaking are simple. The inherent good character of the speaker or his education and experience have little connection with them. The classical concept of the good orator being the *good* man skilled in speaking needs to be re-examined. A 1948 version is better: The poor speaker is the *inadequate* man with nothing to say who nevertheless can painfully consume 30–60 minutes of an audience's time without profit and without the slightest qualm of conscience.

Don't begin now or later to look at any books on speech organization or delivery. You will regret it if you seek advice on how to improve your speaking. If anyone suggests that you have a recording made of your voice, shun the idea. You will be disillusioned and may even become so upset as to want to do something about improving your voice—always a dangerous symptom of incipient good speaking.

FINAL SUGGESTIONS FOR POOR SPEAKING

Do not read:

ANDERSON, V. A. *Training the Speaking Voice.* New York: Oxford, 1942.
BRYANT, D., and WALLACE, K. R. *Fundamentals of Public Speaking.* New York: Appleton-Century, 1947.
FLESCH, R. *The Art of Plain Talk.* New York: Harper, 1946.
MONROE, A. H. *Principles and Types of Speech.* (Brief ed.) Chicago: Scott, Foresman, 1945.
OVERSTREET, H. A. *Influencing Human Behavior.* New York: Norton, 1925.
SARETT, L., and FOSTER, W. T. *Basic Principles of Speech.* Boston: Houghton Mifflin, 1946.
WINANS, J. A. *Public Speaking.* New York: Century, 1915.

TEXTURE OF SNOW

We had forgotten the texture of snow . . .
only the children remembered,
welcoming it back
with outstretched hands and hearts.
Slowly it fell at first,
sifting, drifting to earth,
lighter than thistledown, feathery,
star after white star . . .
then it fell heavily,
closing the sky,
forming whirlpools of white cotton.
We had forgotten its reassuring warmth,
burning the cheeks, the palms,
but the children remembered;
they remembered, too, the soft snow-fur,
hugging it to them, a round Angora kitten—
caressing it, stroking it . . .

MAE WINKLER GOODMAN

IMPROVED METHODS OF TRANSPORT AND THEIR SIGNIFICANCE*

THOMAS H. MACDONALD

Mr. MacDonald received his B.C.E. from Iowa State College in 1904. In 1929 his alma mater awarded him an honorary degree of doctor of engineering and the Marston Medal for achievement in engineering. He has been commissioner of public roads in charge of the Public Roads Administration of F.W.A. since 1939, and received the Medal of Merit for outstanding services during World War II.

TRANSPORTATION rates its own high and secure place in the sun. Although it is not in itself the objective, it is the most potent tool for human accomplishment. There is far too great blindness to this definitive truth.

Economists have in a high degree predicated their major conclusions as to current human affairs and trends of future progress upon two considerations—production and consumption. They have been inclined to place little emphasis upon the effects of transportation on the existing status or the actual attainment of the latent potentials inherent within these two elements.

Humanists measure their conclusions by other considerations, such as capacity for self-government, proficiency in the arts and sciences, and opportunity to develop mental discipline and to advance the ethical conduct of the individual. These men of good will recognize the importance of environment in shaping the life of the individual, but they focus their ideas too closely. Back of the immediate surroundings, one of the major elements in the creation and perpetuation of these conditions, good and bad, is transportation.

If these criticisms are reasonably fair, how much greater, then, must be the lack of understanding of the ruling influence of transportation on the part of the general public. Such criticisms reflect adversely upon the colleges and universities because of their failure to include in their curricula the courses that will give the needed academic training for public leadership in the transportation field. They reflect adversely upon all of us who are connected with the many phases of transportation, because of as yet too-limited research of the character and scope needed to uncover the knowledge upon which to found sound laws and administrative policies.

It is high time that we recognize and accept the fact of the profound influence exercised by the kinds, the availability, and the characteristics of transportation upon human affairs. These factors have been potent in the past and will be increasingly more determinative of the future trends which affect the daily life of the individual and the many currents of national and international relations.

In our thinking, certainly transportation with its essential concomitant, terminal facilities, including storage, must be accorded equal weight with production, consumption, and social relations in their combined determination of human economy and social trends.

The inherent characteristics of each method of transportation as it has developed have exerted a ruling influence in creating the present pattern of agricultural and industrial development and consequent population distribution. Certainly the availability of natural resources, including fertile soils, determined what has been done *in toto*, but the methods and costs of transportation have fixed the pattern of the physical national structure as it exists today.

Beginning with the earliest form, water transport (which in the fifteenth century opened the New World to exploitation and settlement by European countries) fixed the location of our great population centers. There are only nine American cities of more than 200,000 population that are not served by navigable water.

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* From a paper presented at a symposium on "Improved Methods of Transportation and their Import" at the Princeton University Bicentennial Conference on Engineering and Human Affairs, October 2-4, 1946.

These foci of water transport became the natural origins and termini of rail lines to serve inland areas and to integrate them internally and transcontinentally. The development of rail transport radially from established ports brought this service to large inland areas. Communities dependent upon the horse and ox for local transport power developed along the rail lines at distances fixed primarily by the limitations of animal travel. At important convergings of railroad routes, communities gradually grew into cities. Their location was largely dependent upon topography, itself a determiner of rail-line location. They became the gateways from which supplies were distributed radially and to which the products of the land flowed for processing or for shipment beyond.

More recently, air transport has demonstrated its capacity for relatively light net loads to reduce the time-distance ratio to a fraction of that previously existing. The whole world is brought into physical contact measured by hours in place of days and weeks. Major lines of air transport are dependent for sustaining income upon the already established metropolitan areas and intercommunication between these.

The result of the development of water and rail transport, particularly in combination, has been to build population concentrations that, until about 1900, were compact in form and thus occupied limited land areas.

With minor exception, all three types of transport—rail, water, and air—are predicated upon regimentation of the individual. No matter how commonplace or how luxurious his personal accommodations, he becomes one of a group subject to the discipline of the fixed schedule for departure and arrival over a fixed route. Once he adjusts his personal convenience to the schedule, he normally enjoys a fast, comfortable, and safe trip. Except for suburban train service, these three types are chiefly used for passengers over relatively long distances and take on the aspect of state-wide, nation-wide, or international movement.

The transport of commodities, livestock, and all articles of commerce at present does not fall into as easily defined groups. The bulk of the tonnage moved in carload and larger units goes to the railroads and to the waterways. The average distance of the movement is relatively long. There is, however, in the aggregate a very large movement of goods of commerce and types of articles such as mail, parcel post, perishable foods, and other items of relatively small bulk and weight and relatively high value that is divided among all the types of transport.

HIGHWAY transport in its major uses is the antithesis of water, rail, and air transport. Its growth in the past two decades has been nothing less than fantastic, yet none of its early pioneers who are honest lay any claim to foreseeing its present dimensions. There was celebrated last year at Detroit the fiftieth anniversary of the automotive industry, but the early years gave little indication of the stature this type of transport was destined to acquire.

Between 1921 and 1941, the number of motor vehicles in use increased threefold, and the annual use of the average vehicle, measured by miles operated, doubled. Thus in the two decades prior to World War II, the motor-vehicle mileage increased 600 percent. After deducting trucks and busses, the state with the minimum ownership of passenger automobiles in 1941 had one car for every 9.9 persons, and the state with the maximum ownership had one car for every 2.6 persons. The average for the nation was one car for every 4.5 persons. Each car traveled in 1941 an average distance of 9,285 miles. The passenger-car capacity was thus equal to the moving of the whole population of the nation simultaneously. Although the number of cars in service was reduced during the war years, the prior highest level will be regained rapidly.

These facts are perhaps indicative of the over-all growth of highway transport, but carry little illumination as to the intensity of its impact upon existing conditions and its potency to effect changes.

The growth of industry brought with it a constantly increasing urban population and concentrated more people in compact areas. The typical city developed without much planning and without an over-all conception as to the form that might have a chance for survival. The stage was being set for a movement so spectacular it has been well named "the explosion of the cities." The motor vehicle did not create this situation. The blighted and slum areas had been slowly developing until now the estimate is made by Commissioner W. E. Reynolds that "the health, safety, and morals of some 25 millions of Americans are being seriously affected by blighted areas." Rather than creating these conditions, highway transport has become one of the basic tools to remedy them, to shape the pattern of daily life, and to make possible a new urban culture. There are 140 urban areas of 50,000 or more, which aggregate about one-half of our whole population.

There is a wide variation in the pattern of the central city and its satellite communities, but the problems of both are common to those of other metropolitan areas. Actually, some of these metropolitan areas are approaching the autonomous quality of the old Grecian city-states. Los Angeles, for example, has an actual corporate area of 452 square miles, but the urban area spreads over 600 square miles, equal to more than one-half the area of the state of Rhode Island. Within this urban area there are 1.5 million people and 30 cities of 5,000 or more population. For the larger urban areas highway transport is being used as a tool to effect—although as yet in the early stages—city redevelopment, abolishment of slum areas, opening of parks and parkways and suburban communities of far superior living facilities, with the minimum of time required to reach place of employment. There are some 2,042 cities with more than 5,000 people within their corporate limits, and the concentric bands of urban population widely overflow the corporate limits. Statistics based on the 1940 census, show that 56.5 percent of the total population live in urban surroundings and are very largely dependent upon highway transport for their daily movements and services.

Probably the most significant service in its long-range effect is the change made possible by the school bus. The little red schoolhouse is being rapidly replaced by the consolidated elementary and high schools. The relative opportunities of these two need no comment.

Rural free mail delivery started with horse-drawn vehicles, but the service has been greatly augmented by the speed and capacity of the motor vehicle, plus better roads.

Traveling libraries and other potentials have already contributed, and in the future will in an increasing degree contribute, to rural life. Opportunities for recreation, social contact, or education are now quite generally shared by the farmer, and these will be extended by reason of the rural road-improvement program now under way.

War production required many new plants employing astounding numbers of people who were dependent upon highway transport. The Willow Run and Chrysler plants at Detroit and many of the Chicago plants were built away from the hearts of the cities in locations where land was available quickly and at lower relative costs. Here, revolutionary ideas were put into practice because highway transport was available. Industrial plants were located in the areas where labor of the required skills and in sufficient numbers was established. Highway transport became a part of the assembly line. All kinds of raw materials were moved to plants over the highways, and units built in plants situated miles apart were brought together for assembly over the highways. There were numerous instances of almost fantastic procedures. For example, wings for bomber assembly at Tulsa, Oklahoma, were built at Willow Run.

One industrial organization during the war had four plants, located in three cities. It hauled, by means of motor trucks, forgings from plant A to plant B, 100 miles away, for heat treating; then hauled the heat-treated forgings another 100 miles to plant C for machining; and finally to plant D, 15 miles away, for assembly into finished unit. The prime contractor in this instance had an assembly line of approximately 215 miles between the forging plant and final assembly.

The hauling on highways in normal peacetime is shown in Table 1.

TABLE 1

COMMODITIES TRANSPORTED ON RURAL HIGHWAYS

ITEM	MASSACHUSETTS 1938	CALIFORNIA 1936–37	WYOMING 1936–37	43 STATES—SURVEY YRS. 1936–38
	Percent	Percent	Percent	Percent
Products of agriculture ...	8.05	16.96	17.17	12.8
Animals and products	12.37	9.93	10.00	13.2
Products of mines	4.71	2.85	12.08	6.4
Products of forests	3.31	3.80	4.01	5.2
Manufactures and miscellaneous .	62.03	66.46	56.74	53.5
Mixed freight ..	9.53			8.9
Total	100.00	100.00	100.00	100.0

The specialized equipment available for transport over the highways of products difficult to handle has been particularly marked in the growth of the milk industry, contributing alike to farm income and the provisioning of the cities with better-quality foods. The consumption of milk in Washington, D. C., in 1925 was 40,850 tons; in 1945 it had increased to 216,290 tons—an increase of more than five times the earlier figure in a two-decade period during which the population doubled. This milk is produced largely in the near-by areas of Maryland and Virginia. The fine dairy farms with their sanitary buildings and highbred dairy cattle have become perhaps the most pleasing elements of the rural landscape in the vicinity of Washington.

THERE is far too great emphasis upon the element of competition between the available types of transportation, and far too little accent upon the degree to which each type is necessary and supplementary to the others. Each type of transportation is supreme in the service it can best render. This does not mean necessarily the cheapest service. As a general rule, the public has evidenced its willingness to pay a premium for time-saving, but this has decided limits. Since, ordinarily, water, rail, and air transport cannot perform a complete service—that is, from origin to destination—highway transport must supplement each one. Highway service is chiefly important in the short-haul field. In the transportation of persons, the figures in Table 2 are indicative.

TABLE 2

PASSENGER TRIPS—AVERAGE LENGTH

	MILES	YEARS
Rail	82	1936–40
Air	525	1946
Private motor vehicles	14.6	1937–38
Bus—intercity	30–40	1941–44

Studies of the use of the passenger car reflect in a major degree the repetitive daily travel of the average individual and his family within the environs of his own community. Eighty-five percent of all individual trips of the passenger car are within a 20-mile radius. Focusing this more closely upon the use of passenger cars in four cities of different population, origin-and-destination studies of 1944–45 (Table 3) show within what narrow limits passenger cars operate.

TABLE 3

CITY	POPULATION (METROPOLITAN AREA)	TRIPS ENTIRELY WITHIN CITY	ALL TRIPS—TRANSCITY, INTO AND OUT OF, AND WITHIN THE AREA
		Miles	Miles
Denver, Colo.	361,100	3.12	3.67
Fort Wayne, Ind.	120,000	2.07	2.61
Greenville, S. C.	75,000	1.47	1.81
Spartanburg, S. C.	45,000	1.37	1.98

In the field of truck use there seems to be little evidence to show that distance in itself is a determining factor in competition between rail and highway transportation, although trucks are at present little used for transcontinental hauling. Trucks are used, however, in regular hauling for distances of 1,000–1,500 miles. The nature of the commodity to be moved, the particular conditions surrounding the shipment, and the importance of the time element are more significant than distance in the shipper's choice of mode of transportation.

Fruit and vegetables are hauled by truck to New York City from states as far south as Florida and as close as New Jersey. Furniture is conveniently moved by van, principally because of the elimination of the need for crating. Once loaded, the van may traverse a few blocks or, as readily, as many states. Livestock movement by truck is increasing, with trips often covering substantial distances, for truck movement is quicker and loss of weight of the animals is reduced. In Ohio large quantities of coal are hauled from the southern Ohio fields to Columbus and other cities by truck. Here the product of the small mines tunneled into the hillside by one or two miners is loaded onto light trucks or semitrailers and hauled directly to the consumer. In areas of large-scale operations, however, in which loading facilities are designed for use of railroad cars, shipment of the same product by rail over the same or even much shorter distances would be the rule. Milk moves into New York City from near-by farms and likewise from the Adirondack milkshed by highway, now that suitable truck bodies are available, with the obvious advantages of its receipt in bulk rather than in cans. Many other examples could be cited in which the product to be moved and the peculiar advantages of truck movement direct from producer or shipper to consumer or processor are of far greater significance than distance alone.

Another factor in actual or potential competition between rail and highway, the importance of which should be investigated as opportunity offers, is the degree in which truck haul between points served by rail is in reality not competition. Undoubtedly, much Southern fruit is sold in Northern markets because it can be brought in quickly and with little handling by truck, when such shipment by rail would not have been practicable and would not have been made. In other words, is it not possible that the truck has developed markets that otherwise never would have existed? Probably few trucks are actually competing with the freight trains they pass.

The idea that trucks compete in tonnage hauling with freight trains becomes somewhat ludicrous when it is shown that in 1941, of the 3,711,000 registered trucks, classified according to manufacturer's capacity in tons, 3,212,000, or 86.6 percent, were in the 1.5-ton-and-less class, and over half of these, in the less-than-one-ton class.

Although highway transport in the aggregate reaches tremendous proportions of passenger-miles and ton-miles, its sphere is in the local field, supplementary to, rather than competitive with, other kinds of transport. The road program is geared into this aspect and will be governed by the principles of developing urban, main rural, and secondary roads on a balanced program to serve primarily the objectives of the communities in which they are located. In the cities the most distinctive newer type of improvement will be the controlled-access road, which is designed to carry traffic rapidly and safely into and through the metropolitan areas. The main rural roads, when they reach a daily volume of 4,000 vehicles, will be considered for improvement as four-lane, divided highways. Rural secondary roads will be built to serve the travel with the quality of minimum annual maintenance cost.

The highway builders are not fumbling the problems. The state-wide planning surveys that started generally in 1934 have been continued, and the highway program reflects the actual and potential uses that the individual highways composing the different classes or groups are called upon to serve. Many new techniques have been developed. The origin-and-destination studies alone have added tremendously to our knowledge of how and why humans behave as they do on the roads. Because the motor vehicle is so closely tied to the needs and demands of the individual, it is through research into these factors that we must determine the future highway pattern and thus the ability of the motor vehicle to serve.

SALOMON'S HOUSE: A STUDY OF FRANCIS BACON

RUFUS SUTER

Dr. Suter received his Ph.D. from Harvard in 1932. From Harvard he went to the Library of Congress as a cataloguer in philosophy; while there he held a fellowship for two years in the Asiatic Division. Engaged during the war in confidential work, he is still in Washington, now with the Army Map Service.

IN THE same year that the Pilgrim Fathers landed at Plymouth Rock a group of gentlemanly sailors embarked from Peru for China. Contrary winds carried them instead to the Island of Bensalem in the South Seas. There an official having the look of one who pitied men told such a wondrous tale that it is remarkable no one has ever returned to hear more. But the fact is that even the best maps of the South Seas compiled from the aerophotography of our Army fliers show no trace of the Island of Bensalem.

The official with the compassionate look was a Father of Salomon's House. His marvelous story is of interest to us today because we are concerned about the organization and the government control of scientific research. Salomon's House was (and is, if it still exists) the perfect answer to such problems. It combined the experimental skill and the learning of the Smithsonian Institution, the Carnegie Foundation, and the Library of Congress. It had, to boot, the social prestige and awesome authority of the Church of England and the Supreme Court.

At a time when men in Europe were barely awake to the knack of using observation and experiment for decoding the cryptogram of nature, Salomon's House had built observatories and laboratories, experimental forests, zoological reserves, sanatoriums, experimental kitchens and breweries, and clinics for advice on marital problems. When the as yet sporadic revelations of science in Europe were fighting for their existence against the vested authorities, Salomon's House had developed an efficient service for communicating scientific knowledge to the public.

In the biological laboratories some discoveries were in advance of the aims of our investigators today. The gap between the organic and the inorganic was fully bridged. Plants were grown from synthetic seeds. Living insects, worms, and reptiles were evolved from putrefaction. Crossbreeding was practiced in various combinations, occasionally resulting in hybrids able to reproduce their kind. The old Biblical challenge of adding a cubit to one's height had been met. Physicians could preserve life when vital organs had been destroyed.

In physical laboratories many natural phenomena were reproduced artificially: for instance, metals and mineral baths, electrical storms, rain, and snow. Not only were telescopes and microscopes in use, but also instruments with the opposite purpose of making near things seem far and large things seem small. There were devices for magnifying sound. All sensory phenomena—the perceptible properties of our physical environment—were studied empirically: heat, light, color—even smell and taste, neglected by modern physicists.

Like us, the Bensalemite scientists had built air-conditioned rooms, but they used them for therapeutic purposes.

If we glance at the Constitution of Salomon's House we find a statement of objectives worthy of being inscribed above the door of the Rockefeller Foundation: "Our aim is the knowledge of causes and secret motions of things; and the enlarging of the bounds of human empire, to the effecting of all things possible." In modern language: "Our purpose is to discover natural laws and to apply this knowledge to engineering, medicine, agriculture, eugenics, etc., for the benefit of the human race."

Membership, according to the Constitution, was divided into classes of Fellows with somewhat bizarre titles, but with nonetheless recognizable duties. Several groups of Fellows were occupied with gathering data. The Merchants of Light made periodic trips to foreign lands to bring back reports about scientific achievements. The Mystery-men and Pioneers gleaned accounts of experiments from all the bulletins, magazines, and books they could lay their hands upon, and supervised original experiments. Other classes of Fellows, such as the Compilers, were engaged in the clerical duty of elaborating these data into synoptic tables. The Lamps pointed out new experiments hinted at in the records of their colleagues. The Dowry-men, or Benefactors, suggested implied practical applications. The Interpreters of Nature studied the huge synoptic tables for the purpose of codifying natural laws and axioms.

A point to be noted in the Constitution is that the officials—the Fellows—were never concerned with the spadework of observing and experimenting. This much of the aristocratic prejudice of the scholars of classical Greece and China against working with the hands persisted in Bensalem. The august Fellows, in their caps and gowns and seated in their armchairs, either composed and edited documents or occupied themselves with the high points of broad administration. They suggest lawyers or learned scribes rather than laboratory scientists. Actual experiment and observation were left to plodding hermits under orders, who lived all their lives on the mountaintops or in caves: the observatories and laboratories.

If we have become suspicious that this account of Salomon's House is romance, we should be reminded that truth is stranger than fiction, for the romancer is not H. G. Wells but Sir Francis Bacon, Lord Verulam, author of the famous *Essays* many of us read and memorized in high school. We have been retelling parts of Bacon's posthumously published and unfinished novel, *The New Atlantis,* written at the dawn of modern science. In this fragment Bacon visualized the specialization and empirical method that were to characterize the growth of science through the next three centuries. This actual feat of Bacon's is, in its way, as incredible as some of the wonders of Salomon's House.

We could mention other fascinating points in *The New Atlantis:* for instance, that the Old Atlantis, alluded to in Plato's dialogue, the *Timaeus,* was America. Or we could mention Bacon's wholehearted pique at the Chinese for the prohibition against Europeans entering their kingdom. Here we have a sidelight on what must have been a real sentiment in English official circles. *The New Atlantis* was written toward the end of the Ming Dynasty, when English mariners were trying hard and unsuccessfully to gain access to Chinese ports.

But rather than entertain ourselves with more tales from a secondhand source, let us visit Bensalem to learn whether the precise methods of Salomon's House in its fecund inquiries may not be available to foreigners. To be sure, we could get the same information from the second half of Bacon's *Novum Organum,* but this interminable series of aphorisms is an ordeal to read whether in the original Latin or in translation. The magnificently attired Father, whom Bacon described as having the look of one who pities men, can give us the knowledge we seek in more palatable form.

Our guide, accordingly, introduces us to the chief of the Heat Division in the Bensalem National Physics Laboratory. Fruitful inquiry into the law of heat [that worthy informs us] depends, like the study of any natural phenomenon, upon the distribution of data into three files. The first of these is called *The Table of Essence and Presence.* A catalogue of the cases where the phenomenon under examination occurs, it is, of course, in our Heat Laboratory a file of as many and as various instances where heat occurs as are ascertainable by us and our field agencies.

In a modern Kardex he shows us thousands of entries, among them "Rays of the sun," "hot vapors," "flame," "wool," "bruised vegetables confined," "animal heat," and "dung."

The second file [continues the chief] is called *The Table of Absence in Proximity.* It is a catalogue of cases where the phenomenon being studied is absent, but where the cases otherwise resemble those in the first file.

He pulls out a drawer of this second Kardex and shows us that the cards not only have their own proper legends, but also an elaborate cross-index system. For instance, on the card marked "Rays of the moon, stars, and comets: *no heat,*" is a cross reference from "Rays of the sun." On the card marked "Vapors in their natural state: *no heat*" is a cross reference from "Hot vapors (i.e., vapors newly exhaled from, or associated with, hot bodies or compressed)." In short, only those instances of the absence of heat are recorded that show more or less similarity to the instances where heat is present. The reason for this limitation is practical. To file all cases of no-heat would require too many boxes.

We are puzzled, however, by certain cards in this second file: those bearing the blunt legend, "No negative instance known." Each has a cross reference, for example, from "dung," "animal heat," or "flame."

For certain types of phenomena [explains the chief] where heat occurs, no comparable phenomenon has been found without heat. The very fact of this deficiency is significant. Hence the "zeros," as it were, in the file of *Absence in Proximity,* indicating that with such types of phenomenon there is no case of absence in proximity.

The third file impresses us as more complicated than the other two. It is called [says the chief] *The Table of Degrees,* or *of Comparison,* and it is a catalogue of the comparative magnitudes with which a phenomenon is exhibited. Heat, of course, exhibits itself in various degrees of intensity. Thus, in our *Degrees* file we record the cases where heat occurs according to a system of ratings: 1, 2, 3, etc., indicating the relative intensity of the heat.

He pulls out a drawer at random and shows us the following series, where the larger the rating number the more intense the heat:

Ignis fatuus (Jack-o'-lanterns)	1 or less
Sparklings from the sweat of animals	1 or less
Spirit of wine	1 or less
Flame from vegetable matter; e.g., dried leaves	about 2
Flame from hair	about 2
Flame from wood (small faggots with little pitch)	3
Flame from wood (trunks of trees)	4 or more
Flame from oily substances; e.g., wax	5
Flame from pitch	6
Flame from sulphur	7
Flame from certain chemical compounds; e.g., gunpowder	8
Flame from certain imperfect metals	9
Lightning	10

In another drawer he shows us that "flame (quiescent)" is rated at 1; whereas "Flame subjected to motion, e.g., blown by bellows or lashed in a wind," is 2 or more. We observe also that this thermal rating of certain metals is increased when the metal is subjected to the motion of pounding (as the anvil under the blacksmith's hammer). It is clear to us, on the other hand, that thermal ratings in general decrease as motion is checked.

A subtle part of our technique [says the chief], having a direct relation to the efficiency of our filing system, is the matter of Prerogative Instances. Politeness may have prevented you from making the obvious criticism that if our selection of instances for our files is wholesale, if it is merely a patient accumulation of all the cases of heat and of no-heat that our Merchants of Light, Pioneers, and other field agencies, pick up, our files will be overcrowded. The fact is, we try as rigorously as possible to limit our selection of data to Prerogative Instances. Our highest-paid research workers are those who have the knack of ferreting out Prerogative Instances. There are dozens of kinds. We have them analytically catalogued in another Kardex. But I shall explain only one or two.

You have already perceived [he goes on] that we record in our File II (*Table of Absence in Proximity*) only instances that have cross references from our File I (*Table of Essence and Presence*). That is to say, we file only instances of no-heat that resemble in other respects instances of heat. Thus, what we file are all, more or less, Prerogative Instances. To speak with strict theoretical accuracy, however, a Prerogative Instance of no-heat would be an instance that had literally every feature in common with instances where heat occurs, except one, namely, the feature that constitutes the law of heat, the laying bare of which is the aim of our technique. Such strict Prerogative Instances are exceedingly rare, and one alone, if it could be found, would theoretically be enough to solve our problem.

You also commented especially about our legend [he continues] "No negative instance known" in our *Table of Absence in Proximity;* and you acutely observed the cross references from certain cards in our *Table of Essence and Presence.* These legends lend an especial dignity to the cases they are cross references *from.* Such instances, in File I, are more or less Prerogative. Again, however, to speak with strict accuracy, a Prerogative Instance of the presence of heat would be a case that had literally no feature in common with any situation where heat is wanting. In such a theoretically perfect Prerogative Instance of heat, the entire remaining complex of features, other than the heat itself, would be unambiguously the law of heat. But, in the absence of such perfect cases, we must rely upon the hundreds and thousands of cases approximating more or less to Prerogative Instances. I could go on for hours explaining the various kinds of Prerogative Instances: for example, what we call the Prerogative Instances of the Door or Gate, made available to us by telescopes, microscopes, and other instruments for strengthening the power of the senses. But I have explained enough to make clear to you the "empirical" part of our empirico-inductive method.

Here the chief hesitates and gives us a knowing wink as if he expects us to understand that these Prerogative Instances are the most delightfully clever inspiration in the whole history of Bensalem.

All that remains to be elucidated [he says] is the "inductive" part of our empiricoinductive technique. This can be done in a few words. It is a mechanical operation. Our office force just runs through our files crossing out every item that appears in association with heat in Kardex I and also in association with no-heat in Kardex II, and that fails to dovetail with any of the graded series in Kardex III. The few remaining items, after this wholesale deletion, automatically are those that are present always when heat is present, and absent always when heat is absent, and that bear a correlation with increases and decreases in heat intensity. They constitute the law of heat. Q.E.D.

Of course we are curious to know what results the Heat Division of the Bensalem National Physics Laboratory has achieved in the application of this method. The chief informs us that after many years of research the staff has arrived at a provisional formulation of the law of heat, called "an indulgence to the understanding," or "a first vintage." We are confident [he says] that with progressive enlargements and revisions of our files we shall approximate more and more closely to an adequate formulation. At present our formulation stands as follows:

> Heat is a violent motion, expansive in all directions but especially upwards; restrained; and acting upon the smaller particles of bodies.

It is characteristic of our scientific outlook [the chief adds] that every law we unravel we regard as not only an insight of pure or fundamental science, but as also a practical instruction of how, as engineers, we can control and direct nature. Thus, our provisional formulation of the law of heat may also be stated:

> If in any natural body you can excite a dilating or expanding motion, and can so repress this motion and turn it back upon itself that the dilation shall not proceed equably, but have its way in one part and be counteracted in another, you will undoubtedly generate heat.

AND so we end our interview with the Fellow of Salomon's House. Let us consider again, however, that what we learned from the chief of the Heat Division of the Bensalem National Physics Laboratory may be read in the second half of the *Novum Organum,* by Francis Bacon (minus some twentieth-century streamlining). Bacon's work, however troublesome it may be to read, whether in the original Latin or in the Ellis and Spedding translation, belongs on any worth-while list of scientific classics, because it is the first attempt to formulate the empiricoinductive method.

Its very title is significant: *Novum Organum,* "The New Instrument," suggesting the supplanting of the *Organum,* or orthodox "Instrument," by which the logical writings of Aristotle were known, and which was the bible of all the college professors and preachers against whom the founding fathers of modern science struggled. Bacon wrote in the age when men like Kepler, Galileo, Gilbert, Torricelli, Pascal, and Boyle were beginning to use an empiricoinductive method. It is one of the striking anomalies of history that the original formulation of this method came from a lawyer rather than from a physicist or mathematician. He was relatively unqualified for the task. Suspicious of Galileo's telescopic astronomy and contemptuous of the studies of magnetism by Gilbert, his personal physician, he was inept in his own experiments, had little if any first-hand acquaintance with the telescope and microscope, invented—and so spectacular—in his lifetime, and was incorrect with amazing fidelity in his interpretation of specific natural phenomena. He did not even accept the Copernican astronomy. One passage in the *Novum Organum* hints that he was blithely unaware of Galileo's refutation, at the Leaning Tower of Pisa, of the Aristotelian dogma that the speed of a freely falling body is a function of its weight.

One must stand in awe, nevertheless, of a legal talent able to direct its energies as happily as it did toward the codification of the empiricoinductive method. The doctrine of Prerogative Instances is a brilliant anticipation of John Stuart Mill's treatment of induction in the mid-nineteenth century—a treatment sometimes regarded as the definitive discussion in the English language.

Bacon's ever-present shortcoming is the overemphasis on the documentary, or filing, side. There is too much to-do about the scribe and not enough about the laboratory technician. But this is a weakness to be expected from a lawyer and from the Lord High Chancellor of Great Britain. A graver criticism is that Bacon concentrated off-center in identifying the scientific method with a technique of inventing hypotheses, the "hunches" scientists have of what may be a law. For the results of the Herculean labors of accumulating and manipulating the vast synoptic tables of Salomon's House are, after all, hypotheses. The central task of the scientist, in reality, is the empirical testing of the hypotheses. It is upon this empirical testing that all the ingenuity and exactitude of our modern scientific devices and the inventive genius and resourcefulness of our scientific technicians are brought to bear; and it is by this empirical testing that a hypothesis either is discarded or in time is raised to the level of a law.

MANUSCRIPT TRANSLATIONS FROM THE RUSSIAN

All geologic publications. The Committee on Russian Literature of the Geological Society of America has begun the compilation of a list of translations from Russian into English of geologic papers and books; that is, a list of translations extant in manuscript form in the United States. Please send information as to any such translations to the committee chairman or to Geological Society headquarters. The word *geologic* is used here in its widest sense—from geophysics to paleontology.

Current geologic publications. If information about translations of recent publications is received soon enough, it will be included in forthcoming volumes of the *Bibliography and Index of Geology Exclusive of North America* and the *Bibliography of Economic Geology,* subject to permission from proprietors of translations.

Ronald K. DeFord, Chairman
Box 1814, Midland, Texas.

SCIENCE ON THE MARCH

INTERNATIONAL RECOVERY IN SEISMOLOGY

INTERNATIONAL cooperation is essential to some departments of seismology. Investigation of the interior of the earth from seismic data, reliable statistics of earthquakes, study of earthquake geography—all require a widely distributed network of seismological stations. It is gratifying that the world-wide seismological program is now vigorously recovering from the disasters and interruptions of World War II. The following account corresponds to information available at Pasadena in September 1947. It does not include projects for the restoration or improvement of old stations or the establishment of new ones.

Establishment of an international seismological association was followed by publication of collated data; these were issued from the international headquarters at Strassburg and cover the years 1904–8. At this point the work was interrupted by World War I; many stations were discontinued, temporarily or permanently.

After World War I, international headquarters remained at Strasbourg, under French administration; but an international seismological summary was now published from Oxford, under the direction of H. H. Turner, who had already prepared a series of similar reports for the British Association. This summary in its official status begins for the year 1918. It has never lapsed, but has fallen more and more in arrears. During World War II it was kept up largely by the devoted efforts of Miss Ethel Bellamy. She has now retired from the work, which has been transferred from Oxford to Kew Observatory. The latest issue of the *International Seismological Summary* covers the quarter January–March 1936.

An active service of collecting and collating preliminary reports from all stations willing to cooperate, and distributing the results promptly, was developed at Strasbourg by E. Rothé, and continued there until 1940. Since the war, and following the death of Rothé, the same work has been resumed and vigorously prosecuted by his son J. P. Rothé, assisted by E. Peterschmitt and others.

Turning now to the activities of groups of stations and of individual stations, we find little change in the stations associated with, or sponsored by, the U. S. Coast and Geodetic Survey. The latest report lists twenty-eight of these. Those outside continental United States are: Balboa Heights, Canal Zone; Bermuda; College (University of Alaska, near Fairbanks); Honolulu; Huancayo, Peru (hitherto under the Carnegie Institution of Washington, but now being transferred to Peruvian administration); San Juan, Puerto Rico; and Montezuma, Chile (Smithsonian Institution). Seismograms for these stations are analyzed at Washington. As only a small staff is available, the reports are retarded; the latest available at this time (September 1947) is for April–June 1944.

During World War II, by arrangement with the Danish authorities, reports for the two Greenland stations at Ivigtut and Scoresbysund were included with the U.S.C.G.S. group; the records and reporting have now been retransferred to Copenhagen.

Delay in publication of full data for American stations is partly compensated for by preliminary determinations of epicenters and origin times for the larger shocks, circulated promptly by post card. Readings for these and other shocks are included in the preliminary bulletins of the Jesuit Seismological Association, issued from its headquarters at St. Louis University. This organization includes most of the remaining stations in the United States, and some overseas; nearly all issue individual reports, either directly or through St. Louis.

Reports are issued from the University of California at Berkeley for a set of seven stations in central and northern California, and at Pasadena for eight stations in southern California.

Readings for six Canadian stations are reported from the Dominion Observatory at Ottawa, and those for nine stations in Mexico from the Instituto de Geología, Universidad Nacional de México (the central station is at Tacubaya, D.F.).

All these functioned regularly through the war, and bulletins were issued with various delays; in most instances omitted or delayed reports have now been supplied. In the West Indies, a first-class station at Fort-de-France, Martinique, was discontinued during the war and has not yet resumed reporting. Several new stations established in the Caribbean area, in connection with the program of using microseisms to track hurricanes, are now reporting earthquakes.

South America has always been inadequately represented in proportion to its high seismicity. Huancayo, Peru, continues in service. Two first-class stations, at La Paz, Bolivia, and La Plata, Argentina, have resumed reporting and are in process of making up arrears in their bulletins. A new first-class station is in operation at Bogotá, Colombia. Reports from Rio de Janeiro are still some years behind date. The seismological service in Chile is concentrating its attention on the collection of reports of earthquakes as felt by persons and as causing damage; the station at Santiago is in operation, but no instrumental bulletins are issued. This is to be regretted; in a country like Chile, where large areas are sparsely populated and where many earthquakes originate at great depth (down to 300, or even occasionally 600, kilometers), accurate instrumental records are highly desirable, and noninstrumental reports alone may lead to conclusions that are entirely false.

European stations have been severely affected by the conflict. The Spanish stations suffered more during the civil war. The instruments at Tortosa were then destroyed, but have since been replaced. Current bulletins are now being received, with irregularities partly due to the vagaries of the postal service, from the long-established stations at Alicante, Almería, Barcelona, Cartuja (Granada), Málaga, and Toledo. The station at San Fernando (Cadiz) has discontinued pending removal to a more favorable location. The station at Coimbra, Portugal, issued bulletins for part of 1945, but none since. That at the University of Lisbon (Observatório Central Meteorológico do Infante D. Luiz) has enthusiastically resumed activity, issuing reports for the years beginning with 1940, supplemented by current preliminary bulletins.

Most of the French stations were able to continue through the war, although data were not generally available until afterwards. In 1940 the French University at Strasbourg was evacuated to Clermont-Ferrand, and the work of the international office was continued there under the Vichy regime. The station Strasbourg remained in nearly uninterrupted operation, even during the spring of 1945 when the battle front was passing through Alsace.

For Italy, current bulletins are being circulated from Rome (Istituto Nazionale di Geofisica), Pavia, and Florence (Firenze; Osservatorio Ximeniano). Reports for others, notably Prato, are received and relayed by Strasbourg. Under the direction of C. Morelli, the station at Trieste has survived the vicissitudes of the war, including Allied bombing, which on one occasion scored a direct hit on the Geophysical Institute, resulting in the loss of several weeks' records. At this time it is uncertain what effect the newly established free-state regime will have on the activities of this very enthusiastic and cooperative observatory.

One of the best of the German stations, at Stuttgart, has resumed reporting since 1946, under its able director, W. Hiller. Stuttgart is in the American zone of occupation, so that bulletins are received regularly at Strasbourg and in this country. Hiller has also sent out reports covering the early war period down to June 1942, after which the station was closed. The valuable auxiliary station at Messstetten is also operating; it is located in the German foreland of the Alps, where minor shocks are relatively frequent.

Stations are operating at Collmberg (near Leipzig), Jena, and Potsdam, but their data do not yet reach this country.

The troubled history of our time is reflected in the vicissitudes of one of the minor Central European stations, located in the Sudeten area of Czechoslovakia. Previous to World War I this was in Austria and reported under its Germanic name of Eger. After that war, reports were issued, first from Praha and then separately, under the then official name of Cheb. During the Hitler regime, reports were sent out as from "Eger (Sudetengau);" and now current reports edited at, and issued from, Praha are being received under the name of Cheb. Another unfortunate station has alternated between the Hungarian name of "O-gyalla" and the Czech "Stará Ďala."

Among European stations that have continued through the war or have resumed since its close may be listed Athens, Beograd (Belgrade), Bucharest, Budapest, De Bilt (near Utrecht), Helsinki, Praha, Uccle (near Brussels), Upsala, and Zurich. Others, such as Copenhagen and Sofia, have issued delayed bulletins, but are not circulating current readings.

The most remarkable recovery since the war is that of the stations in the Soviet Union. Extending from the Crimea to Vladivostok, these are an important part of the international network. The Seismological Institute of the Academy of Sciences at Moscow is now issuing a monthly bulletin, which is reasonably detailed although nominally preliminary, giving readings for twenty-three stations. These include all those in operation before World War II, with the following exceptions: Pulkovo, located at the observatory destroyed during the siege of Leningrad; Leningrad, a former minor station in the same city; Sebastopol, one of four stations in the Crimea of which the other three have been restored; and Tiflis, which is under independent administration. (The Academy of Sciences of the Georgian S.S.R. has recently issued reports giving readings for Tiflis through September 1939, but none of later date.) The list includes several new stations; notice of the latest, Ashkhabad, in Central Asia, is included with the bulletin for June 1947.

Africa is poorly represented. The stations at Algiers and Tunis are part of the active French group. Helwan (near Cairo) is in operation, with reports several years behind date. The Cape Town station, which was at first at the Astronomical Observatory and later moved to the University, has apparently been discontinued.

Fortunately, the very important station at Tananarive (Madagascar) continued through the war, and is now reporting regularly through Strasbourg. Ksara, near Beirut (Lebanon), continues to issue excellent reports; during the war, and occasionally since, this station (like some others) has been handicapped by running out of photographic paper, leaving only less sensitive instruments, recording on smoked paper, in operation.

Bulletins for several past years, as well as current reports, have been received from Istanbul (Observatory of Istanbul-Kandilli). The group of good stations in India continued through the war period; printed and manuscript data are available at Pasadena through 1945. Disturbed conditions resulted in rather frequent loss of records. Recent correspondence indicates that the political changes in India may lead to further reorganization.

In the Netherlands East Indies the principal station at Batavia continued in operation through the war. A bulletin was recently issued giving data through 1941 for this and its two principal auxiliaries at Amboina and Medan, which were destroyed later.

There has been no word of the fate of the station at Phu-Lien near Hai-phong, Indo-China. The station at Hong Kong was discontinued in 1940.

The observatory at Manila, with all its records, was destroyed in 1945 by the Japanese soldiery, who poured gasoline on the seismograms and burned them. Of all the accumulated data at Manila, only a history of Philippine earthquakes by Father Repetti was salvaged; this has been published in the *Bulletin of the Seismological Society of America.*

At Zi-ka-wei, near Shanghai, Father Gherzi succeeded in keeping the instruments operating through the war, in the face of many difficulties, among them the inevitable lack of photographic paper. The stations at Nanking and Chiufeng (near Peiping) were put out of action by the Japanese invasion in 1937. With great effort Dr. S. P. Lee, formerly in charge at Chiufeng, succeeded in constructing a seismograph which recorded from 1943 into 1946 at Pehpei, near Chungking.

A small instrument formerly operated at Agaña, Guam. This was destroyed, but a new station, established to record microseisms for use in tracking typhoons, has begun reporting earthquakes.

Information as to the present status of Japanese seismology was obtained by Professor Gutenberg during a recent visit. There were two rival research centers. One, the Earthquake Research Institute, connected with Tokyo Imperial University, maintained several stations in the Tokyo area for the study of local earthquakes. These have mostly survived the war and are in operation. The present director is H. Tsuya, a geologist.

The other center was at the Central Meteorological Observatory in Tokyo, which reported data for more than one hundred stations in the Japanese islands and possessions. This institution is now under the directorship of Dr. K. Wadati, a well-known seismologist. Reports reaching this country through U. S. Army channels give readings for the stations at Sapporo, Sendai, Tokyo, Yokohama, Nagoya, Osaka, Kobe, Fukuoka (sometimes spelled Hukuoka), and, of all places, Hiroshima. The many minor stations are presumably now out of action, but it may be expected that they will be reoccupied and operated when possible. Some, such as those in Formosa and Korea, are now out of Japanese hands. So far there is no news of the international latitude station at Mizusawa, where there was also a seismological installation.

Seismology in Australia is suffering from governmental economies antagonistic to scientific work. The station at Melbourne is definitely closed. No reports have been received from Adelaide since 1942. On the other hand, Perth and Sydney continue to report, and the excellent station at Riverview (near Sydney) is operating under the very productive direction of Father D. J. K. O'Connell. At Brisbane (University of Queensland), a station was initiated in 1937; sensitive Benioff instruments have been recording there since September 1943, and their data contribute much to the seismic survey of the southwestern Pacific area. Apia (Western Samoa), though equipped with less sensitive seismographs, is very favorably located; its continued reports are of great value. An improved station began recording at Suva (Fiji) in 1943, but no later reports have been issued. The valuable station at Papeete (Tahiti), which operated from 1937 to 1939, is another war casualty.

The seismological program in New Zealand, with headquarters at the Dominion Observatory, Wellington, continues to expand and improve, with emphasis on the local earthquake problems.

An interesting temporary addition to the world network is provided by the Ronne Antarctic Expedition, located at Marguerite Bay, which is reporting by radio the readings from a sensitive seismograph. This assists greatly in locating shocks in the Southern Hemisphere.

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EXPERIENCE IS THEIR LABORATORY

WHEN a newcomer to the tropics learns that the family next door lives day after day on what appears to be a submarginal diet of rice, salt, and vegetables, he is likely to wonder how its members survive. And yet there are thousands of people in Indonesia who, though apparently defying the normal rules of nutrition, enjoy the best of health.

During the past twenty years I have studied the average diet in Indonesia. I have learned that many people have acquired a practical knowledge of nutrition—a heritage of empirical knowledge that has enabled them to live in harmony with the tropics. And after studying examples of judicious feeding I believe I have found evidence that is important not only in the tropics, but everywhere that scarcity and famine are a threat.

Of course, many still eat unwisely. For example, in a village in the immediate neighborhood of a tea plantation in Java, government officials reported a child death rate of 7 percent because of xerophthalmia. Investigation showed that the older children were neglecting the younger ones, feeding them almost exclusively on white polished rice. The parents, who worked every day on the tea plantation, had no time to prepare the noon meal; and the older children did not go to the trouble to collect the vegetables that grew in abundance on their own property. This is an example of improper feeding, and there are others. Yet malnutrition is relatively rare. Through trial and error and trial again, these people and their ancestors have found their way to abundant and nutritious foods. And what they have learned through experience, specialists in nutrition are now proving true in the laboratory.

In some parts of Indonesia the population depends principally on sago, a starchy food that comes from the sago palm (*Metroxylon* sp.). After eating sago for several months people become weak and need a change. The remedy is to move to districts where they can eat an abundance of pili nut (*Canarium moluccanum* Bl.). The pili nut, known for its recuperative power, has a content of 15 percent protein, 66 percent fat, and 140 I.U. Vitamin B_1.

A densely populated area in Java furnishes another example. There, because of poor soil conditions, the farmers cultivate tapioca extensively; their diet consists almost exclusively of tapioca roots, a one-sided carbohydrate nourishment. Results of a medical investigation indicated that the population was not excessively fat. It seemed highly probable, however, that on account of this simple diet, a deficiency was bound to show up somewhere. The reason it did not occur is interesting: exhaustive investigation proved that the leaves of the tapioca, eaten daily as vegetables, contained 8 percent protein, 5,000 I.U. Vitamin A, and 50–100 I.U. Vitamin B_1. This observation is of great scientific value because it indicates the digestibility of plant proteins, alone or in combination with others. Not only from a medical but also from an economic-agricultural viewpoint, these studies were important in solving feeding problems of population groups in other parts of tropical countries. Similarly, they can be beneficial to our Western civilizations, where every year our cities face problems in feeding and nutrition.

It is no accident that on hundreds of small islands, the so-called drumstick trees (*Moringa oleifera*) are found in nearly every dooryard. Every day inhabitants of those islands use the fruits and leaves of the *Moringa oleifera* as a side dish. Chemical analysis shows that the leaves contain 8 percent protein and 2 percent fat. The drumstick tree, easy to cultivate and bearing after a few years, is an indispensable source of vegetables found by experience to have high nutritive value as compared with other plants.

Also important is the fact that every day a dish is served that includes Spanish red pepper, with its 4 percent protein, 900 I.U. Vitamin A, and 70–250 I.U. Vitamin C (ascorbic acid).

I noted another benefit of empirical knowledge in a very densely populated section of Java. The average family had just sufficient rice and vegetables, but fat from the coconut was available in large quantities. Because of widespread unemployment, protein from animals was out of reach of the population. The people had chickens, but they did not eat them, preferring to sell the eggs for high prices. The people got their protein, however, and for almost nothing. Every small farmer kept bees, which fed on the nectar of the coconut flowers. But the farmer was not seeking honey; he valued his bees for the wax and white larvae they produced. Young white larvae, fried in coconut oil, furnished plenty of animal protein. The beeswax was used in making batik cloth.

What was the reason for the apparent good health in the heavily populated sections of Java, where the population per square mile averages from 1,100 to 1,300 persons? From the greatest altitudes at which men live down to the shores of the sea, whether on clay or sand, on flooded swamp or dry ground, the entire wealth of the tropics is laid open. It is a wealth of fruits and vegetables, which hold the dominant position in the population's diet. Here is to be found the remnant of the primitive system of collecting plants and animals from the wilds. In the beginning, only plants that were suitable for food, for medicinal uses, for ornaments, or for firewood were grown in the dooryards. This is still true today. Even though dooryard plants sometimes find other uses, they can revert at any time to their original purposes. It is estimated that these dooryards take up about 15 percent of the arable land in Java. They are usually found in compact complexes adjacent to the houses and within the villages. The dooryards are planted, as it were, in stories. The upper story consists of coconuts, mangoes, and species of *Durio, Spondias, Gnetum, Lansium, Parkia, Artocarpus,* and other similar trees. The middle story is the level of the smaller plants, such as bananas, and species of *Averrhoa, Annona,* and *Eugenia.* On the lower story are found undergrowth varieties, especially tuberous plants, including tapioca, sweet potatoes, *Coleus tuberosa,* the Zingiberaceae. such Leguminosae as *Cajanus* and *Psophocarpus,* and the ever-present and indispensable red Spanish pepper. Between these layers wind other tuberous plants belonging to the Dioscoreaceae and Cucurbitaceae families.

The botanist who strolls about an Indonesian dooryard will notice plants of many varieties. As a result of this diversity the proteins from the dooryard are of numerous kinds and of almost uniformly good quality. Thus, although the consumption of animal proteins is very small, the dooryard supplies a good substitute in plant proteins, as well as furnishing vitamins and minerals. The coconut is an adequate source of fats. Thus, the role played by the dooryard in the daily menu of Java and other islands in the tropics can hardly be overestimated. This may also be responsible for the fact that in Java, as the total amount of land owned falls off, the area of the dooryard per person increases. It is as though it were felt that, as the absolute amount of available food becomes less, more attention must be paid to the composition of the menu. Few forget their heritage of empirical knowledge.

Even though owners in some coconut districts in Java are periodically prosperous, the old remnant of "collecting agriculture," which has proved of such great value, still remains largely intact. We must not lose sight of this factor of ethnological economy, which has proved important here, as in so many other parts of the world. Investigation is still far from complete, and it is not merely for curiosity's sake that scientists must find correct answers to many questions. What is learned can help prevent famine and illness caused by food deficiencies, not only in the tropics, but also in other parts of the world.

In his production of home foodstuffs, the small tropical farmer is often able by empirical knowledge to solve his difficulties in the field of nutrition. He knows the importance of special food crops for his existence, and he has a variety of minor plants such as grow nowhere else in the world. This variety gives him a certain protection. Otherwise his food problem, incomprehensible to the uninitiated, would be much more complicated than it is; there would be more food problems and more deficiency diseases.

J. J. OCHSE

Miami, Florida

BOOK REVIEWS

DANISH SCIENTIFIC HISTORY

Naturforskeren Johannes Schmidt. Øjvind Winge and Å. Vedel Tåning, Eds. 187 pp. Illus. Kr. 12.75. Gyldendalske Boghandel. Copenhagen. 1947.

IN A world where a man's work is often only too quickly forgotten, it is a pleasure to see the volume the Danes have dedicated to the late Johannes Schmidt, one of the best known of the many students of marine biology Denmark has produced. The typography is up to the usual high standard characteristic of the old Danish publishing house of Gyldendalske Boghandel. Looking through the lavishly illustrated volume, one would never guess at the economic struggle going on in the land in which it has been published.

The book is paper-bound and carries on the back a severe black-and-grey medallion showing the classical features of Johannes Schmidt; the front displays a colorful cartoon, worthy of the *New Yorker,* with a white boat (presumably the *Dana*) flying the Danish colors, sailing over an emerald-green sea and towing a series of plankton nets astern. In the sea a fantastic deep-water fish treads a dance with a huge, luminous crustacean. It is probably only in Denmark that a dignified firm would send a book of this kind out into the world with such a cover. Actually, the contents of the book live up to its apparently contradictory cover. It contains a perfectly sober and detailed account, written by his friends and colleagues, of Johannes Schmidt's life and accomplishments, but told with such a feeling for the dramatic that one keeps on reading as if it were a Dumas thriller.

After a brief account of Schmidt's family background, the book describes Schmidt's first expedition (to Siam), when he was twenty-four years old, and continues with his activities in the Danish fisheries and with his ever-expanding expeditions in search of the solution of the two-thousand-year-old problem of where the eel breeds. Schmidt's work on the eel runs, of course, as a red thread through the book, but on the side one learns here for the first time how much Schmidt accomplished along other lines which did not capture the public's fancy as did the life history of the eel. Besides being a man of unusual native ability as a biologist, Schmidt had a rare faculty for organizing the most diverse problems, from detailed studies of heredity in plants and fishes to extensive field projects dealing with the development of the food fishes in the North Atlantic and the factors influencing the migrations of these animals. His work extends, therefore, over an unusually large field and is of both a practical and a theoretical nature.

Schmidt seemed born under a lucky star: getting a well-organized expedition on its feet at an age when most men are still working for their degrees; afterward being trained by such an old fox as C. G. J. Petersen, who had the courage to hand an enormous responsibility over to a young man in his twenties (but who also had the satisfaction of watching this same young man find and solve more problems than anyone had expected). On top of this, Schmidt became at an early age the leader of the biological division of the Carlsberg Laboratory in Denmark, which gave him use of the best-equipped laboratories in the country, while at the same time he was free to continue his work on active marine expeditions. His scientific exploits were backed by the wealth of the Carlsberg Foundation, and an always sympathetic and understanding government was ready at any time to supply ships and gear for his expeditions. Last, but not least, he had the full support of numerous Danish scientists—physicists as well as biologists—who worked up the large array of data he accumulated. Without realizing it, the authors have written the story of Denmark's exploration of the sea from the mid-nineties to Schmidt's death in 1933, with its unique cooperation between the government and private institutions and individuals, and the resultant advance in practical and scientific research.

The individual chapters in the book are concise and packed with information; it is a relief at last to find a book entirely free from padding. Each contributor has given his best in the shortest space, and his own enjoyment of his work, whether in the field or in the laboratory, permeates each account. This holds true for the first chapter—about the Siam trip—written by the now eighty-year-old Dr. Mortensen, as well as for the youngest biologist's wide-eyed account of Schmidt's final, triumphant cruise around the world, during which he "bagged" practically all the eels then known to science, thereby giving the taxonomist occasion to write an inspired paean on the classification of the eels.

One understands why Denmark wanted this tribute to be written in Danish, so that Schmidt's lifework could be more fully known to his own people. But one wishes that the authors of the various chapters could be persuaded to rewrite their accounts in English, for "to translate them" is hardly the phrase to use in connection with these men, who publish most of their papers in English. The book is one that should appeal both to the experienced fishery expert and to the beginner, and it should also find many readers outside the scientific world.

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UNIVERSAL LANGUAGE

In Search of Beauty in Music. Carl E. Seashore. xvi + 389 pp. $4.50. Ronald. New York. 1947.

PROFESSOR SEASHORE, an outstanding scholar in applying science to music, attempts to integrate his popular articles and research work at the University of Iowa in this book.

Seashore's approach is configurationistic. Music draws heavily on a number of basic sciences, and it has of necessity been required to await developments in fields such as physics, physiology, anatomy, anthropology, genetics, and general and experimental psychology. The remarkable progress made in these disciplines of recent years has aided immeasurably in placing musical science on a firm foundation.

Although research is still in its infancy, the growing intimate relationship between music and other sciences has been of mutual advantage and benefit. As a result, many superstitions and much fallacious thinking about music have been thrown overboard.

Applications of the sciences, especially of psychology, to music are innumerable. Methods of evaluating musical talent; methods of guidance and of measuring musical achievement; organizing a growing body of principles in educational psychology toward musical training; paving the way at all levels for principles of musical criticism; organizing the scientific description of musical tones and the means of producing them; understanding into the nature, scope, and limitations of musical hearing and appreciation; and use of performance scores for the detailed analysis and the quantifying of artistic elements in musical performance are merely a few results of the contact between music and psychology.

There is some unevenness in the material of the thirty-five chapters. This should be expected since each chapter has previously been presented in whole or in part in a variety of journals, and since the book is designed for such a heterogeneous group as advanced students of music, psychologists, music teachers, educators, professional musicians, and general readers interested in the scientific approach.

Isolationism is no longer valid even in music. The artist, teacher, critic, and listener must comprehend the gestalt—must appreciate the interrelationship of art with the physical and social sciences as a prerequisite in the search for beauty in music.

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THE U.S.D.A.

Two Blades of Grass. T. Swann Harding. xv + 352 pp. Illus. $3.50. Univ. of Oklahoma Press. Norman. 1947.

THE purpose of Harding's book is to show the money (economic and social) value of agricultural research. The four parts (Preliminaries, Achievements, Departures, and Values, with Appendix) are crammed with a profusion of information (historical, scientific, and statistical) on the forerunners, creation, development, organization, and achievements of the numerous successive units of the U. S. Department of Agriculture.

Part I (four chapters) covers How It All Started, Agricultural Science in the Patent Office, Early Research in the Department of Agriculture, and A Few Connecting Links, the last a survey of successive secretaries, of certain laws and their effects, and of a few early problems, such as Hessian fly, silk production, sugar from maize and sorgo, microscopy, etc.

Part II contains ten chapters, nine dealing with different subjects, under fanciful names: Test Tube Magic (chemistry), The Bugs Crawl Out (entomology), All Flesh is Grass (plant industry), Man Can Help to Make a Tree (forestry), A Very Grevious Murrain (animal industry), A Good Soil (soils, fertilizers, erosion), Food and Raiment (home economics), Science at the Milkpail (dairy industry), and Agricultural Engineering Triumphs (drainage, irrigation, machinery, structures). Sandwiched in the middle (chap. 10) is Nation-wide Research Subsidy, which discusses the Land-Grant College Act, the Agricultural Experiment Station Act, and the Office of Experiment Stations, and then reviews a mass of research cooperation between the Department and various state experiment stations in most of the subjects to which the preceding and following chapters are devoted.

Part III (chaps. 15–18) recites the achievements of four Bureaus formerly but not now in the Department: Something Could Be Done About It (Weather), Men on Wheels (Public Roads), Birds and Beasts (Biological Survey), and Fraud in Foods and Drugs (Food and Drug Administration). It is stated that data for the first two were furnished in manuscript form. Much of the early work on foods and drugs had been reviewed in Chapter 5, Test Tube Magic.

Part IV contains Value of Pure Research (chap. 19) and an Appendix on The Publication of Research.

The volume contains an immense quantity of valuable information, but a casual inspection shows that the subject treatments are not uniform, sequential, complete, or always accurate. Arrangement and emphasis seem to reflect writer preference rather than reader need, in spite of the commandment: "All writing is for the reader."

The peculiar lack of sequence is maddening. A chapter usually opens with several unrelated and nonchronological items and then, with little warning, drops back and starts a more detailed review. For example, first Commissioner Isaac Newton dies on page 23 and, on page 28, we learn what he did. Plant Industry opens with a casual observation by a minor scientist on retiring in 1945.

Unexplained omissions occur. The Bureau of Agricultural Economics is omitted from the chronological list of Bureaus created (p. 36), and nowhere is its valuable research discussed. Plant Industry, the largest Bureau, with next-to-longest chapter, apparently has the least complete and accurate treatment. Ignored are the basic Western forage surveys, the international economic survey of the Great Plains area, early soybean studies, the alfalfa program, weed-control research, the lifework of Dewey on fibers, of Stuart on potatoes, of Gould on peaches, etc., etc. Self-advertisers are lauded repeatedly.

The Bureau's largest Division, Cereal Crops and Diseases, is not named. No mention is made of its early and continued advocacy of Federal-state cooperation in research to save money, speed results, and ensure harmony, or its putting its entire nation-wide research program into cooperation with state and Canadian experiment stations, a policy publicly commended by two secretaries and the basis for recent Federal legislation. Omitted are its extensive cooperative breeding and production programs on barley, corn, flax, grain sorghums, rice, and wheat (except durum), including control of destructive diseases. Outstanding leaders like Clark, Harlan, McCall, Richey, Stakman, and Warburton are not mentioned. A single wheat item (rust-resistant Thatcher) is inserted twice, once inaccurately. The international classification of wheat and barley varieties and successive five-year varietal surveys are ignored.

The Division's largest project, the gigantic cooperative barberry-eradication campaign, is not mentioned, although it destroyed more than 30,000,000 plants, discovered the production of rust hybrids in nature (200 physiologic forms), and saves the corn belt and northern Great Plains wheat crops from increasingly destructive rust epidemics, a value in the hundreds of millions of dollars.

Errors abound: in names ("Barry" for Parry, "Clerk" for Clark, "Lowe" for Loew, etc.); in dates (of two international expositions, photograph of Department staff, etc.); and in facts (statement that no Bureaus were created before 1897, etc.). The 31 pictures (16 plates) are excellent, but mostly paired and inserted without regard for subject. In conclusion, any reader who does not check and doublecheck may suffer "a very grevious" misleading.

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SURVEY PROBLEMS

Say It with Figures. Hans Zeisel. xvii + 250 pp. Illus. $3.00. Harper. New York. 1947.

THE subject of this interesting book is inadequately suggested by the title, and the subtitle, "A Manual for Statisticians in the Field of Consumer and Opinion Research," is definitely misleading. The author is concerned largely with the conceptual analysis and the problems of meaning in the treatment of survey findings, rather than with the assessment of reliability and the use of the common statistics that have been the main concern of the statistician.

As a treatment of problems of classification, summarization, and interpretation (as the three parts of the book are characterized by Paul F. Lazarsfeld in his introduction), the book is almost unique and of considerable value. Methods of presenting reasons for behavior, useful procedures for handling other types of multiple answers, the interpretation of "Don't know" answers, the use of percentages and other derivative indices, the value of cross-tabulations, and the panel technique are discussed with examples of how their application clarified some survey problem. A particularly good discussion is presented concerning how to judge indices, exemplified by a subject of high general interest, the baseball batting average, which is criticized, and a new index derived.

The book is a publication of the Bureau of Applied Social Research of Columbia University, and much of the data come from the Bureau's files. More frequent references to other surveys would increase the scope and value of the work. For example, the rotation procedure whereby the Bureau of the Census spreads its sampling variation over a number of months in a continuing panel rather than abruptly absorbing it between panels, and the coding of intensive interviews practiced by the University of Michigan Survey Research Center (and used also by the Lazarsfeld group) would be worth-while additions to the text.

In his attempts to derive meanings from data, the author is often incautious. References to sampling error are meager, and tests of significance completely neglected. Although this is recognized in the book (footnote, p. 145), conclusions are drawn without regard to reliability. For instance, a set of correlation coefficients ranging from +.35 to −.43 is referred to (p. 28) as indicating "that there is indeed a very strong affinity between certain types of [radio] programs." The author here applies a Pearsonian coefficient to fourfold tables, without even stating the number of observations, or assessing the underlying unequal frequencies as a possible spurious factor in relationship. Similarly, in the treatment of cross-tabulation, the following questions are stated (p. 170) to be "the same question in different words": "What are the factors which *determine* the size of this proportion or its complement, the people intending to vote for a Democrat?" and "What factors *characterise* the group which intends to vote Democratic as compared with the group which intends to vote Republican?" (Italics by the reviewer.)

Such treatment makes the book dangerous for those not grounded in statistical caution, and a statistician will find a lifetime of work in determining the variances of statistics used. There are, however, techniques useful for the imaginative analysis of social data, and anyone not solely interested in the manipulation of numbers will find the discussion stimulating, and the book a valuable adjunct to the ordinary text in statistics, which is weak on this creative aspect of analysis.

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BOTH GREAT AND SMALL

A Multitude of Living Things. Lorus J. Milne and Margery J. Milne. 278 pp. Illus. $4.00. Dodd, Mead. New York. 1947.

CHARLES LAMB was wrong. He thought that a mussel, once anchored, was fixed for life, but that was disproved by Reaumur, who put one in a dish of sea water and watched it travel. Some flies make webs that capture spiders. One salamander has no lungs, breathes through its skin.

Facts like these are contained in a book whose title is almost a review of it: details of "a multitude of living things" are packed—I might almost say jammed—into it, but it is easy reading.

The king crab has his chapter, and so does the speleologist. Somewhere I have read that there are a hundred ways in which to cook an egg, but there is much more to an egg than cooking it. The writers have done a chapter on Consider the Egg, which treats of this defenseless organism, food for hungry animals, but somehow able to carry on for itself.

Rivers that no longer exist carried hardened amber from pine forests of the Baltic into the sea. Now it is mined and made into pipestems and jewelry. Don't buy an amber necklace until you have consulted this book, because there have been many imitations of this substance.

Do you know how the blind fish of Mammoth Cave became blind? The book will tell you.

There is some discussion of eyes—the eagle eye, the eyes of squirrels and of the praying mantis.

The authors tell also of hay fever caused by the microscopic scales from the wings of caddis flies, which gives an idea of the scope of the book.

And if you want gruesome romance, the exceedingly good account of the sexton beetles should suffice.

It is difficult to make comparisons, but this book reminded me of some of the works of Fabre. Read it a chapter at a time and enjoy it. There are a few—too few—photographs; those that are presented are unusually good.

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PAMPHLET

John Couch Adams and the discovery of Neptune. Sir Harold Spencer Jones, The Astronomer Royal. 43 pp., with a portrait of Adams. 75 cents. Cambridge Univ. Press. Cambridge, Eng. Macmillan. New York. 1947.

THIS astronomical *Whodunit* explains clearly the complex circumstances surrounding the discovery of the planet Neptune and gives a brief account of the controversies that followed. Sir Harold puts the reader in a position to judge for himself the parts played by Adams, Airy, Challis, Herschel, Le Verrier, and Arago. Half an hour with this delightful pamphlet will give you the inside story of a famous episode—a story that sheds vivid side lights on the human element in research.

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THROUGH PATHLESS REALMS

Rockets and Space Travel. (Rev. ed.) Willy Ley. 384 pp. Illus. $3.75. Viking. New York. 1947.

THIS book is a revised and enlarged edition of Ley's *Rockets,* 1944. Two complete chapters have been added and the original chapters revised and brought up to date. Ley's book describes the early history of rocketry in the greatest detail and interest in English. The treatment of the growth of the concept of free flight in space is treated in a scholarly manner. One, however, should not think that *Rockets and Space Travel* is just another popular book on the subject. The appendix consists of over fifty pages of straight technical information in fine print and is an excellent introduction to rocket theory.

If we go from the general to the specific, we find that Ley has grouped the illustrations at the beginning of the book, which captures interest in an excellent manner. Then follow twelve chapters on the history of rocketry, the development of rocket theory, the use of rockets in the second world war, and finally a popularized account of the application of rocketry to the space ship. The rocket enthusiasts have been talking about space ships for years, but most scientists took their enthusiasm very lightly. Now that it is evident that space travel is only a few years away and that the first nation to establish a military base on the moon will dominate the earth, the development of space flight by engineers and scientists is one of the primary objectives of research sponsored by various governments.

The thesis of space travel, and especially the construction of a terminal in space as a take-off point, is described. The discussion of the application of atomic energy to space flight (pp. 282–3) is not very good in an otherwise excellent treatment. The methods described, such as the use of soluble U-235 salts in water to produce superheated steam, or the use of U-235 on a feed wire into a combustion chamber, are not the most probable devices for a thrust drive. An enriched, high-temperature pile using a secondary fuel for the mass effects of the blast stream appears to be the best way of utilizing atomic energy in a space ship. It should be observed that atomic energy is not a prerequisite of space flight. Chemical fuels will put a ship on the moon and allow construction of the space terminal. Ley emphasizes this point.

If the reader wishes to get a modern, up-to-date, declassified survey of the position of rocketry, Ley's book should be read in conjunction with G. Edward Pendray's *The Coming Age of Rocket Power* (New York: Harper, 1945). The two books supplement each other. Most of the engineering and scientific data on rocketry is being published in the *Journal of the American Rocket Society,* with a scattering of articles in other journals, such as the *American Journal of Physics* (Seifert, 1947). The development of rocketry on a practical basis, as described by Ley and Pendray, will have as much influence on civilization as the development of atomic energy.

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EVOLUTION OF CIVILIZATION

Genetics, Medicine, and Man. H. J. Muller, C. C. Little, and Laurence H. Snyder. vii + 158 pp. Illus. $2.25. Cornell Univ. Press. Ithaca, New York. 1947.

THIS volume of 158 pages comprises the six 1945 Messenger Lectures which are given annually to provide "a course of lectures on the evolution of civilization, for the special purpose of raising the moral standard of our political, business, and social life." Dr. Muller's chapters (The Work of the Genes, The Dance of the Genes) survey in a sprightly fashion the fundamental properties and actions of the genes as they play their role in the biochemistry of life and sex. In nontechnical language, he boldly pictures the great cooperative enterprise of the germ plasm as it provides a short-term as well as a long-term adaptation of the species to its environment.

Dr. Little stresses the relation of genetics to medicine by a masterful review of research on laboratory mammals. Parental influences at the morphogenetic, emotional-endocrine, and psychosocial levels of environmental activity are summarized, with the hope that medicine will be provided with a sound biological foundation of experimental facts. His second chapter deals with integrated growth and individuality of the organism as these are related to the cancer problem.

Dr. Snyder summarizes many of the known data on human heredity with special stress on medical applications (prognosis, diagnosis, prevention of human defects). A simple exposition on the genetics of the blood antigens A, B, M, N, P, and Rh is included. His final chapter on the mutant gene in man provides a particularly clear statement of the genetic basis for human heredity, including the most recent advances on sex-linked genes.

There is an agreeable unity in the book despite its triple authorship. It must be considered as an accurate and modern presentation by these three authorities in their respective fields of genetics.

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WATER

The Story of Water Supply. F. W. Robins. x + 207 pp. Illus. $4.50. Oxford Univ. Press. London, New York, Toronto. 1946.

THE story of the human struggle and success in making water available for the almost innumerable purposes to which it is vital began with man's advent on the scene, has added chapter upon chapter through the ages, and is still being written. F. W. Robins, within the compass of this small volume, has traced the stream through the history of man's economic, political, and cultural development from the earliest times down to recent engineering projects.

Primitive man lived along rivers and other natural water sources. In order to settle in areas distant from natural supplies, he had to devise means of bringing the water where he wanted it. Mr. Robins has selected examples from early records in every corner of the earth to illustrate the mother wit and ingenuity of man in making water come to his bidding. Not only in ancient India, Mesopotamia, Egypt, Greece, and Rome were there outstanding waterworks, but also in thirty or more other countries from Abyssinia and Zanzibar to Peru and Mexico.

Little less than astounding were the variations and effectiveness of canals lined with stone or concrete; aqueducts on arches, some still in use after all these centuries; underground conduits; and pipes of lead, copper, bronze, wood, terra cotta, bamboo, etc. The rivers had been nature's water mains, and springs her pumping stations. Man had to create his own water mains and pumps of various kinds. The size and complexity of such installations varied according to the number of people to be served. In ancient Rome, with an estimated population of 1,000,000, eleven principal aqueducts brought a daily supply of 40,000,000 gallons of water to the city from distances varying up to forty miles. Canterbury Cathedral was supplied with water from springs only a mile distant. In A.D. 1160 that was quite adequate for the small community.

Many medieval towns, monasteries, friaries, and private palaces possessed elaborate indoor equipment, with spigots in kitchens and sprinkling devices for shower baths. Robins pictures graphically the various kinds of water dippers used by early man, made, as they were, of gourds, calabashes, bamboos, animal skulls, bark, leather, coconut shells, and, of course, the cupped hand, which preceded all others.

There are special chapters on "wishing wells," water divining, water sellers, and parish pumps. England is treated in such intimate detail as to make the book a Baedeker on the Roman, Anglo-Saxon, and medieval water systems in that country. We are told that there are more than 400 wishing wells in England, around many of which superstitions or actual beliefs still linger. Although Robins was skeptical of the claims made of locating water sources by divining sticks, he was convinced of the authenticity of the method in one case.

The wide range of data garnered from his global ramblings through the literature on ancient man is documented by footnote references on nearly every page.

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THROUGH A GLASS, DARKLY

From Caligari to Hitler. Siegfried Kracauer. xii + 361 pp. Illus. $5.00. Princeton Univ. Press. Princeton, N. J. 1947.

DO THE films of a nation reflect its mentality in a more direct way than any other artistic medium? Is the motion-picture screen a reflector, a mirror, a recorder of the existing mass desires of the people? Is it true that the technique, the story content, and the evolution of the films of a nation are fully understandable only in relation to the actual psychological pattern of a nation? Will a story of films made in Germany during and after World War I help one understand the ascent and ascendancy of Hitler?

Siegfried Kracauer would reply yes to all these questions. Some interesting hypotheses have been stated by Mr. Kracaucer, but I think that an affirmative answer needs more qualification than he presents.

Here are some of the characteristics of the German mind that Kracauer thinks were reflected in German films during the interwar period:

1. A deep-seated fear of social change and thus a welcoming of films which defamed not only bad rulers but also good revolutionary causes.
2. A strong sadism and an appetite for destruction. The sadism appears to stem from a basic inferiority complex.
3. Life represents a choice between a regime of tyranny or a cataclysm of anarchy. Authority will protect society from decomposition.
4. An inner change of the individual counts more than any change of the outer world—an implication justifying the middle-class fear of social change.

My reader can plainly see the problem involved in presenting such a thesis as Kracauer's. According to this theory, the writer, the director, and his staff are consciously (or unconsciously) reflecting certain values. The public who sees a film consciously or unconsciously is appealed to by these values. But a motion picture does not even need to be popular ("Caligari" is an example) to reflect these desires and longings.

To check on the validity of Kracauer's hypothesis, we might apply it to the films made in the United States today. What mass desires are being appealed to by the five hundred or so motion pictures we produce annually? What does the heavy emphasis on crime and violence in our movies mean? Is our free and easy emphasis on "rubbing out" people, kicking, slugging, shooting, or torturing them a reflection of a mass desire to hurt, to wound, to get even? Simply to ask questions such as these is to suggest the difficulty of answering them.

Kracauer's hypothesis is a highly suggestive one for film analysis even though he did not prove his hypothesis to the satisfaction of this reviewer. A film, it seems to me, is not a mirror of life except in a highly metaphorical sense. It is, rather, life seen through the eye of a writer, then through the eye of a producer, then through the eye of a director, then through the eye of a camera, and then through the eye of a cutter. What usually comes out is a reflection not only of society, but of what the producer thinks will make money. This final product is inevitably related in some degree to the mass desires, wishes, and longings of the movie-going publics in the United States and the rest of the world.

The discovery, however, of the factors finally responsible for what appears in the film is a huge psychological and sociological task, which Kracauer's volume will help to solve. His volume also contains a great deal of useful information about the history of the German film. It is fruitful reading for any serious student of motion pictures.

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COMMENTS AND CRITICISMS

RACIAL INTELLIGENCE

In an article concerning racial intelligence in the October issue of THE SCIENTIFIC MONTHLY, Professor Henry E. Garrett begins with the statement that "the question of the existence of Negro-white differences in mental ability within the United States has of late been sadly confused with social and political issues of racial superiority, discrimination, and the like." This is a very generous admission on the part of Professor Garrett, but perhaps gratuitous, for it would be hard to find a more excessive example of this kind of confusion than his article presents.

Although Professor Garrett claims to be interested in the *facts,* what he deals with primarily are interpretations. The facts are sound enough; the interpretations, I believe, can be shown to be more than questionable.

Let us, then, take Professor Garrett's statements passage by passage and see how they stand up under a critical examination.

"A study of Negro-white differences in the United States offers certain advantages—as well as disadvantages—to the student of racial psychology. A decided advantage is the fact that Negroes and whites have lived side by side in our country for more than three hundred years."

For a native of Virginia (if the biographical account appended to the article is correct) to have written the words "Negroes and whites have lived side by side" betrays either a singular incapacity to perceive the obvious or else a disingenuousness of the most inhuman kind. If there is one thing that Negroes have *not* done anywhere in the United States it has been to live "side by side." And it is the whites who have prevented this by erecting barriers of every possible sort calculated to keep Negro and white apart. When, then, Professor Garrett writes that "the American Negro's native language is English, and he has been exposed to the same manners, customs, and environmental influences (schools, churches, movies, etc.) as the American white," I think he is not only misinterpreting but, what is worse, that he is also misstating the facts. In the land in which Jim Crowism and segregation in "schools, churches, movies," and particularly in "etc." is the rule, to say that the American Negro is exposed to the *same* "manners, customs, and environmental influences," represents a misstatement of the facts so revealing as to beggar description. If the American Negro participates in white culture at all (and that he does so is obvious), it is only more or less peripherally. The Negro's "place" has been on the outskirts of the periphery of American culture. He has rarely been allowed to penetrate beyond. If Professor Garrett is really anxious to know the facts let him read the collection of them made for 1942–43 by Professor Howard W. Odum in *Race and Rumors of Race* (Chapel Hill: Univ. of North Carolina Press, 1943). In this book he will find Professor Odum asking why it was that Southern conduct toward the Negro was so contrary to all principles and preaching; why it was that the tenets of fellowship and Christian religion did not hold with respect to the Negro. He will find Professor Odum (himself a Southerner) returning the considered judgment that the only answer is "that the Negro did not come within the framework of human brotherhood" (p. 23). Let Professor Garrett read Gunnar Myrdal's *The American Dilemma,* a book that presents the facts *dispassionately* analyzed by a Swedish scientist. Possibly these two works might show him that it is completely and, I may add, viciously, untrue to say that the American Negro has been exposed to the same influences as American whites. But does one have to read books to know this? I think not, and I also think that no amount of reading of books or acquaintance with the *facts* is necessarily capable of modifying beliefs that a person *wishes* to believe. But this is a commonplace of psychology.

"We may begin with McGraw's study in 1931 of the comparative abilities shown by 68 white and 60 Negro babies, two to eleven months old, all living in a Southern community. This study is valuable in spite of the small samples because social influences are minimal if not completely absent at these early age levels."

To students of the socialization of the child it will come as a surprise to learn that there is still a psychologist in existence who believes that "social influences are minimal if not completely absent at these early age levels," specifically, from two to eleven months. Most students of the social development of the child would agree that the most fundamental steps in the process of becoming a social human being are taken well within the first year of life; that the socialization of the basic urges or needs of the child commences almost from the moment of birth; and that by the end of the first year different habits of behavior usually associated with different social classes are already easily perceptible. Babies in different cultures are already during the first year perceptibly different in the nature of their responses. Professor Garrett would doubtless put this down to their genes. The anthropologists and the child-study groups, the pediatricians, and most psychologists think differently. Margaret Mead, Bateson, Benedict, Dennis, Erickson, Kluckhohn, Mowrer, Dollard, Gesell and Ilg, Helen Thompson, Margaret Ribble, Goldfarb, and numerous others find that the first year is perhaps the most important in the whole social developmental life of the person. But not so Professor Garrett.

The developmental quotient of the small sample of Negro infants to which Professor Garrett refers was 92, some 13 points lower than that of the white infants. Differences in the socialization process might very well explain such a finding. In any event, the sample is heavily prejudiced by a difference in socialization for which no allowance whatever is made. On the other hand, such differences in social status as were reported are minimized by Professor Garrett, and the very significant fact that the white infants were taller and heavier than the Negro infants is dismissed as irrelevant. The fact that height and weight are almost universally regarded as the best measures of the developmental status of infants may not be of relevance for Professor Garrett's argument, but so far as the interpretation of the facts is concerned, I submit that it is of some importance. Taller and heavier children would be expected to show a higher developmental quotient.

With respect to the I.Q.s of Negro as compared with white children, I do not think that it is possible to draw any conclusions from the tests as they are at present set up other than that what they measure is socioeconomic experience and schooling. Where these variables have appreciably varied, the results obtained should be expected to vary. And this is, in fact, what we find. Income and per capita expenditure on education is highly correlated with I.Q. score. This has been recently demonstrated by Drs. F. L. Marcuse and M. E. Bitterman, of the Department of Psychology of Cornell University, with respect to the scores obtained on the Alpha and Beta tests administered to draftees during World War I ("Notes on the Results of Army Intelligence Testing in World War I," *Science*, 104, 1946, 231–32). From their results the authors write that "it is probably most warranted to conclude . . . that the Beta scores, like the Alpha scores, are strongly influenced by cultural factors concomitant with the socioeconomic levels of the states" (p. 231).

In an analysis of "Intelligence Scores of Northern and Southern White and Negro Recruits in 1918" (*J. Abnorm. Soc. Psychol.*, 39, 1944, 471–74), Professors Thelma G. Alper and Edwin G. Boring, of the Department of Psychology of Harvard University, conclude their findings with the words: "It is the Negro's educational disadvantages and not actually his color which handicap him in these tests" (p. 474).

My own findings are in complete agreement with this verdict ("Intelligence of Northern Negroes and Southern Whites in the First World War," *Amer. J. Psychol.* 58, 1945, 161–88).

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Dr. Garrett has participated in a number of polemics on the subject of Negro intelligence, one of the most recent being in your sister-journal, *Science* (1944–45). At that time his remarks were adequately refuted by others more competent than myself in the field of racial psychology.

In his latest article, however ("Negro-White Differences in Mental Ability in the United States," SM, October 1947), he bases a large part of his argument upon McGraw's study of Negro and white infants in which she found the former to be significantly inferior. This was until recently the only study of its kind in the United States. Dr. Garrett states incorrectly of that study that ". . . the standard height-weight measurements . . . show the Negro children to be as typical of their age levels as were the white children." Not only were the height-weight measurements below those of national Negro norms (which are far below white norms), but the white female figures were above the norms quoted by Dr. McGraw.

In a paper published last year (*J. Genetic Psych.*, 1946, 69, 3–44), I describe a group of New Haven Negro infants whose development was fully equal to that of the white babies upon which the *Gesell Developmental Schedules* are based. Significantly, the birth weights and physical development are equal to local and national white norms. This study was done during the war years when despite (or perhaps because of) rationing Negro mothers for the first time in American history secured a diet approaching that of white standards. Within the past decade the great importance of prenatal nutrition upon infant welfare has been indicated by a number of workers in the field.

The infants of our study have now been examined for a third time (ages nineteen to thirty-two months) by Dr. Hilda Knobloch and myself (report not yet published), and we find that their development continues at the initial rate.

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MAN, THE MYTH-MAKER

It is not my habit to indulge in *pro bono publico* letters, and I certainly am not the friend of censorship or anything which savors of curbing free speech; but the article on "Man, the Myth-maker" in your issue of July 1947, does provoke the observation that freedom of speech implies a responsibility to use canons of good sportmanship.

In the article referred to, on page 62, the writer is speaking of what he calls "crude anthropomorphic myths" which have recently merely suffered a word change even though utilized by reputable thinkers. Says the writer: "Witness the Pythagorean assertion of a great modern scientist that God is a Mathematician (maybe it was a joke or whimsey) and the feminized Platonic Realism of Mrs. Eddy (certainly not a joke)." The writer continues his apparently amused contempt for the millions who still believe in such "crude sacred myths" as personal immortality. From that he goes on to pillory "millions of believers in various more or less mythical concepts including fifty-seven other varieties of semi-sacred myths that deny the evidences of the senses and the findings of science."

Permit me to observe for the benefit of this writer, and others of similar temper, that sadism is not science, nor is sarcasm the height of wisdom or the scientific spirit. It apparently never occurred to this writer that the most fundamental principles in physical science must and do contradict and deny the "evidences of the senses." Such contradictions are indeed the very foundations of science. He evidently still labors under the naive idea that physical science, with its apparatus and methodology, has attained firsthand contact with "reality." The genuine scientist knows, and frankly states, that the best he is able to do is to give us a symbolic representation at secondhand of what the universe really is. Indeed, the "findings of science" themselves have not infrequently been myths with emotional response, persecution, invective, and all the other characteristic stigmata of handling the unbeliever. Does this writer really think that scientists like Steinmetz, Carrel, Eddington, Jeans, Dampier, or Lecomte de Noüy are below par mentally because they recognized the reality of psychic and spiritual forces above and beyond mere sense-appearances? I wish we had more of their equals in the field of sociology!

The whole history of mankind insofar as it records progress has consisted in the stripping off of layer after layer of false belief and outworn concepts. We welcome every new advance of truth which enables us to discard the old beliefs however precious they may have seemed to be. At the same time, scientific progress is never made by rudeness or overstatement or deliberate affront. The appeal that convinces men is not oratorical, harsh, but the winsome, straightforward, humble, and modest presentation of fact. The gates of the kingdom of science and of truth are never opened by name-calling or *argumentum ad hominem.*

ARTHUR J. TODD

Washington, D. C.

I am writing to congratulate you on publishing Read Bain's article on "Man, the Myth-Maker" in the July number of SM. The article is very timely and highly important. If we are entering a new scientific era, we must put off the old traditional myths we have been brought up on and make all things new. Myths developed during man's most ignorant and degraded period, namely, the half million years or so during which he was slowly rising from the animal level to the somewhat higher level he is now on. As we enter the new era of knowledge and understanding we must free ourselves more and more from these old myths. This is almost as important for the scientist as for the layman. Have you read the article by Dingle in *Nature* (London) for July 26, page 108? He points out some of the shortcomings of even our top scientists and contends that the greatest lack and corresponding need of science is for a constructive self-criticism. He attributes this lack to the fact that scientists are digging industriously, each in his own little dark corner of the great unknown, and seldom come out to look around (in Bain's terms, to scrape off some of the myths).

It may be the most important function of the SM to expound this new point of view and to prepare our people for the new world we are now entering.

An incidental reason for this letter is to help counterbalance the letters you may receive against having published Bain's article.

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BRIDGMAN AND COHEN

In the August 1947 SM the semitechnical articles by Janssen, on rock-weathering, Dellinger, on the ionosphere, and Lewis, on the distribution of wealth in a Mexican village, are all very interesting. The last of these may be of some value as indicating the natural equilibrium of wealth distribution in communities, a distribution the New Deal has been trying to upset by stultifying the efforts of the able as far as possible, unless they were politicians or in the good graces of the politicians.

The articles by Bridgman and by Cohen are likewise interesting, but for a totally different reason. Whereas the three articles above-mentioned enable us to enjoy the great outdoors in a detached fashion, these last two are highly charged with personal feeling. Cohen, and the collectivists who think like him, are a threat to scientific detachment, which Bridgman, and those who think like him, regard as essential to a scientific frame of mind.

I did not notice, at first, that Cohen was a lawyer, according to the small type under the caption, but I became aware as I read on that his work was a piece of special pleading, a lawyer using his brilliance, rather than his conviction, to plead a bad case; or the performance of a magician, dazzling the audience with a convincing demonstration of things that we know perfectly well are not so.

Bridgman's article gives the impression of sincerity, a rather simple sincerity, though in some of his scientific writings I have suspected him of taking a Puckish pleasure in teasing his readers with conundrums. But here and there is a note, not insincere, of bitterness and alarm, as when he defines the "present philosophy of the relation of the individual to society" as the "right of the stupid to exploit the bright."

The bitterness and alarm are justified, but the phenomenon is not quite as stated. The "stupid," even if they should turn out to be more numerous than the "bright," are incapable of exploiting them. They are too stupid to do so, even if they wished; and being stupid does not necessarily involve a wish to exploit others. The people of less than average intelligence are often simple, sincere, and reasonable, though they may be sometimes a little tiresome to the "bright."

The real trouble is that the "stupid" are egged on by those who think, like Cohen, judging from his reference #9, that the preferential exploitation of the bright, and of the successful or wealthy, is desirable. The trouble is not with the stupid, but with the bright demagogues who exploit their stupidity and, through their votes, hope to exploit the bright ones who mind their own business.

As a matter of fact, the dissociation of the scientific investigator from worries as to how his discoveries may be misexploited by lawyer and politician is important to his success as a discoverer. To this extent Bridgman is absolutely right. Therefore, as a matter of ethics, society has no right to place upon him a burden that will destroy him as a scientist. But, as a matter of fact also, the scientist has something the politican wants: he has knowledge, which is power. The politician wants that power, power over other men, which that knowledge represents: to him this power is wealth. He does not possess the ability to create the wealth, so he must create a situation in which he is dominant over the scientist. This is where the votes of the "stupid" come to his aid. He can create the impression that the stupid are exploiting the bright, but it is really the power-hungry "bright" ones, who understand the "stupids," exploiting those "bright" ones who are not power-hungry and care for the knowledge that is power, rather than for power itself.

Psychologically, I suppose the most characteristic thing about scientists is that they have no ambition to lord it over other men. Some are sociable, and some are shy: some are easy to get along with, and some are definitely cantankerous. Some make good executives, as was strikingly demonstrated in the OPM and WPB, but very few really want to be executives, and a large proportion of them ask little of other men, except that they keep out of their way.

This is where the danger lies. They have power, which is untold wealth to another type of mind, and they have no use for it. What a group to plunder! It is like taking candy from children.

But here enters another phenomenon, utterly perplexing to the politician. The scientists are perfectly willing to give their knowledge to all the world: they do so in "scientific papers." The politician would rather have the knowledge donated to himself and kept from others, for that would increase his power. The original Kilgore Bill seems to have had this viewpoint.

But a still more exasperating situation follows. When the scientists have turned over to the politicians their knowledge, their wealth of power, it evaporates in their hands. It ceases to be power, because the knowledge, which is freely given, is beyond the ability of the politician to accept. Present him with all the scientific papers that were ever written, and kill off all the scientists, and he has nothing but waste paper. Rome in the hands of the Vandals is no longer Rome, save in name.

Hence arises the demand for control of the scientist. It is not the power of the scientist that must be snatched from him: it is not the knowledge of the scientist that must be exploited: it is the scientist himself.

The situation is bad; but desperate situations often produce great men to handle them. Although the mind of the typical scientist and of the typical politician are far apart, it is not inconceivable that from among the ranks of scientists there may arise a consummate statesman. If so, though he will in the end be sacrificed to the demagogues and the mob, he may be able to accomplish something of the synthesis that the arguments of both Bridgman and Cohen demand.

As things stand, the dilemma continues. The scientist wants to be left alone to be a scientist: the politician wants him chained to his chariot on his triumphal processions. He wants to be able to show the mob how marvelous are the results when the politician makes the scientist do his bidding, and what a benefactor of the nation the politician is when the scientist is compelled to keep in mind continually the social possibilities of his discoveries. This demonstration will be false, but it will be effective if we are not alert and careful.

The politician, in trying to throw the odium of the misuse of science upon the scientist, is confessing his own incompetence and dodging his own responsibilities. But to the extent that the politician is incompetent, the scientist must help. If we have some statesmen with even a modicum of competence, and of a fair standard of honesty, it should not be difficult for at least some scientists to collaborate with them, giving them help and advice from time to time, without feeling exploited or exasperated at the loss of their time from research proper. This is the only intelligent compromise. But it should not be forgotten that power is no concern of the scientist, and that the statesmen must accept the sole responsibility for its exercise. Its misuse, its unintelligent use, will become increasingly dangerous as science progresses. A higher standard of statemanship will be demanded. Less preoccupation with the exploitation of the votes of the "stupid," the uninformed, and misinformed will be demanded, even if it means that our present crude concept of "democracy" as mob rule by an uninformed majority has to be amended.

At present the scientist is afraid of the politician: he may well be, after the violent and illiberal and dishonest career of the New Deal. The politician is also afraid of the scientist: he may well be, after the atomic bomb and the supersonic rocket. But not all statesmen are knaves, and not all scientific discoveries are harmful.

The scientist may regret, or even resent, every moment's diversion from his researches: he may be convinced, and rightly convinced, that every such diversion is a net loss. But we have to be practical. The injunction, addressed to men of otherwordly ambition, still stands, to be as wise as serpents and not simply as harmless as doves. And wisdom consists in recognizing what has happened to scientists, what power has been committed into their hands, what dominion over all the earth, and that the fear of them is upon every living creature.

Into their hands the future of the nation and of the world is committed. With that power necessarily goes responsibility. Great power was for a decade or more granted to the trade unions, but responsibility they refused. Are scientists going to imitate them? The Congress and its committees sought the advice and suggestions of the unions, to frame an equitable law, and met with no cooperation whatever: now the unions are howling about a slave-labor act. A measure of collaboration from scientists is essential, even at the loss of some of the time we should like to devote to research; otherwise we also shall soon be complaining about the enslavement of the scientist, and it will be more than an exploitation of the bright by the stupid—and far worse.

The detestation in which we hold some lawyers, some politicians, some legislators, their stupidity and their cupidity, the shrewdness that substitutes for intelligence with some of them, must not prevent our recognizing that this country is entitled to good government, the best government we can provide, and the best will not be provided without the help, very substantial help, from scientists. Government by statesmen advised by scientists would appear to me to come near to being ideal, on an intellectual basis, provided that fundamental honesty (on which scientists are long) and on decent respect for the limitations of human nature (on which many scientists are short) are also part of the set-up.

We do not want another doctrinaire brain-trust; we need wisdom as well as "brightness." And let us hope that scientists can show such a degree of human sympathy and understanding that we do not live to see them discredited and detested like the irresponsible leaders of the trade unions.

Statesmen must not demand the souls of scientists, nor too much of their time. That is not the way to make a great nation. But they are entitled to the support of scientists, to their advice, to some at least of their time. Scientists cannot be excused this contribution, as a matter of practical politics, any more than they can be excused their share of taxes. Nor should they ask it. They are entitled to press vigorously for a reduction of everyone's taxes, for a change in that vicious system (which Cohen clearly approves) of demanding from each, not according to his ability, but according to the square or cube or some higher power of it; and they are entitled to see that scientists shall not be put under excessive contributions of their time, merely because they are scientists.

Bridgman is right in insisting that the responsibility for the social or unsocial use of science is a responsibility that lies upon the whole society. Cohen is not right in insisting (as I think he does) that a special part of it lies upon the scientists. The use made of science is not a scientific phenomenon, it is a social one. All the citizens share the responsibility. The scientist takes his share only because he is a citizen; and scientists as a whole take a very minor share, because they are few. The crime of misapplying science is squarely on the shoulders of the great majority who are not scientists. The sin of omission in not applying every scientific discovery to some socially desirable end also rests upon society as a whole. Scientists are not now, and never will be, numerous enough or clever enough to see that no useful application goes unexploited, no unsocial one ever gets practiced. They are not policemen: they are not mind-readers: they are not magicians: they are scientists—seekers after truth.

Somewhere close to Bridgman, and a long way from Cohen, is the ideal society; for science cannot flourish in Cohen's society, and society cannot flourish without science.

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EINSTEIN'S ERROR?

The following criticism is made on the assumption that Dr. Gould ("The Theory of Relativity and the Atomic Bomb," SM, July 1947) has given an accurate exposition of the relativity theory. It appears to agree with several such outlines that have been studied over the past thirty years and, if anything, appears to be the most lucid of those in recollection. For this reason, it seems also to be the most suitable one as basis for the following criticism. It seems, in fact, to reduce the theory to absurdity, although there is obviously no such intention on Gould's part.

Professor Einstein's theory of relativity has, of course, been questioned before, and, particularly recently, some of his derived formulas have been under concerted attack by physicists and engineers because the same results can be obtained by different, and seemingly more logical, formulas. The purpose of the present discussion is to show that the original, special relativity theory, which he published in 1905, not only assumes conditions that do not exist, but in addition uses a mistaken theory of light to explain the hypothesis. It is supposedly based on a scientific dilemma arising from experiments carried out by Fizeau in 1851 and Michelson in 1881, which are said to disagree with Newton's "principle of additive speeds" (1687).

Fizeau is supposed to have found that, when light was passed through a liquid in rapid flow, "some but not all of the velocity of the liquid was imparted to the light" (quoted from Gould) as compared to passage through the motionless liquid. In seeming contrast to this, Michelson found that when he reflected a beam of light in the direction of the earth's motion (18 miles per second) and another beam for the same distance at right angle to the first, both beams made the return trip in exactly the same time.

These experiments embody several dissimilar elements, for which apparently no comparative data exist, such as different direction substituted for parallel direction in quiescent and flowing medium, and gaseous for liquid medium. Even so, there is probably no mechanism by which Newton's corpuscular theory of light could account entirely for these findings, but the undulatory theory of light does so quite readily and, since modern views tend more in this direction, there is no further cause for assuming that any dilemma is presented by these experiments.

Gould's article employs Einstein's "moving-train-and-platform" simile to explain his hypothesis. This is presumably copied almost verbatim from Einstein. It is stated that the velocity of light through air or vacuum is so great that "nothing whatever can be added" to it, and only part of any superimposed velocity can be added to the lesser speeds of light through various liquids, etc. These conclusions are supposedly drawn from the above-mentioned experiments and from other similar evidence.

The constancy of the speed of light in air or in vacuum is said to abrogate Newton's principle of additive velocities and is supposed to be due to its enormous magnitude, whereas, since all known facts point to a different mechanism of light propagation than Newton assumed, it is the theory of light that is at fault. It is not a question of magnitude, but one of the underlying phenomenon involved in light propagation.

The velocities of undulatory or wave motions are dependent only on the characteristics of the medium in which they travel. In the case of light, the possibility of superimposition of two media must be assumed. Light cannot properly be said to consist alone of vibration of the bodies through which it passes, as sound is entirely accounted for by vibration of the air, liquid, or solid through which it passes. Unlike sound, whose velocity increases in accordance with the elastic properties of the media and in general with their density, the velocity of light is markedly reduced in accordance with the optical density of bodies located in its path.

Light must be dependent for its speed on a combination of the properties of at least two media when traveling within the earth's atmosphere. The situation is somewhat like that of a stream of water permeating, or running through, a log jam, which may also travel at its own speed, dependent on friction against the shore, although the effect of the logs themselves on the speed of sound in this water would in this case be negligible. Their effect would be limited to a slowing up of the stream with attendant slowing of the sound. In the same way, light, passing through transparent bodies, is always impeded, but more or less so depending on the state of motion of these bodies. In no sense could their entire velocity be added to that of light.

Regarding any effect of the motion of a light source on the velocity of light, the Doppler principle (1842) states explicitly for sound and light that such a motion affects the frequency or wave length—*pitch* in the case of *sound*, and *color* in the case of *light*. This principle is amply substantiated by experience, and it is also well known that frequency and wave length are unrelated to the speed of transmission. Were it not so, music would become distorted (refracted) when heard from a considerable distance or when transmitted by FM, because the higher notes would travel faster than the lower ones. It would also presumably be impossible to be hit by a projectile before hearing it. It is also known that all electromagnetic waves travel with the same velocity, even though the frequencies of known waves vary widely.

The entire train-and-platform simile, purporting to show that there is an apparent infinitesimal difference between the time on the train and that on the platform, therefore becomes meaningless. The assumption of a real difference, leading to an interrelationship of space and time, would have to be based upon a superimposition of a timeworn theory of light (Newton's) on an erroneously assumed effect of the speed of a light source on its velocity, and certainly makes a very artificial and questionable basis on which to found the general theory of relativity (1915).

As stated above, any distinction between the behavior or speed of light and Newton's underlying principle of additional speeds is not based on magnitude, but on an understanding of the type of phenomenon involved. Newton's principle obviously does not apply to wave motions in the form used by Einstein. As far as the formula $E=mc^2$ is concerned, would it not be almost as correct and informative to say that $E = much,\ much,\ much$ since we are as yet very far from being able to ascertain how much *more* energy the atom might contain than that expressed by this formula? Certainly, the fact that the speed of light appears to be a universal constant must be based on entirely different reasoning than this.

Exactly what Fizeau thought he discovered is in doubt without extensive research. It does not seem possible that he or anyone else (except perhaps a mathematician gone rampant) could have thought that any of the velocity of a liquid could be added to that of light passing through it by any type of mechanism, corpuscular or undulatory, unless the liquid travels at a greater speed than the light. At any speed below this the liquid must (and does) slow the speed of light, as indicated by the refractive indices. It would be very interesting to make a detailed analysis of his results in accordance with the ideas of Fresnel (1788–1827).

The fact that liquid flowing in the same direction as light slows its velocity less than motionless liquid is simply explained by the fact that it has less liquid to traverse. The difference is proportional to the distance the liquid travels during the time required for the transit of light; or, to put it another way, the apparent optical density of the liquid is correspondingly less.

Such investigations might add considerably to the theory of light, but none of this information could possibly lead to an interrelationship of space and time as long as, in Dr. Gould's words, "we can make a transformation of the one time and place into the other." Since, therefore, no uncertainty exists, it is merely a matter of applying corrections in known and time-honored fashion. The fact that all celestial bodies are in motion relative to one another changes this circumstance in no fundamental. A reference-point-at-rest can always be assumed, and it might actually someday be discovered.

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OBFUSCATING PERSONALITY

The August SM and the current number of the *Journal of Clinical Psychology* contrast a bit too sharply on the subject of personality diagnosis. SM carries an excessively enthusiastic piece by Louis Wekstein on projective techniques for "X-Raying the Personality," particularly by use of the Rorschach Test. (The test consists of ambiguous ink blots, which are interpreted, and then explained, by the subjects, clients, patients.)

In the *Journal*, the most relevant publication in this country, edited at the Medical College of the University of Vermont by Frederick C. Thorne, psychologist and psychiatrist, I have occasion in a piece on the psychotherapeutic use of logic to mention with satisfaction the passing "of the vogue of ink spots;" and the editor comments with some alarm on the "opportunities for the growth of quackery which currently exist in clinical psychology particularly in the fields of diagnostic testing and counseling. The development of projective methods . . . has opened a fertile field for those who purport to be specialists. . . . Riding on the band wagon of the current popularity of the Rorschach test, these self-appointed experts solemnly give ponderous opinions on personality problems which seem ridiculous to more sophisticated observers."

I regret need for caustic comment on personality X-raying, for I am a devoted reader of SM, but the plain truth of the matter, somewhat overdue, is that the Rorschach test during its decade of popularity has not been proved reliable or significantly valid for useful diagnosis in the opinion of the majority of psychometrists. Much evidence has appeared during the past year, particularly in the *Journal of Applied Psychology* and in *Educational and Psychological Measurements*. On the other hand, by the test the schizoid have been "proved" schizoid, the neurotic have been found neurotic, and the Nazi war criminals have been discovered somewhat paranoid after months of imprisonment. The ink blots are sufficiently phallic to prove entertaining, and the test is clearly superior to extrasensory perception, but it is my observation that clinical psychologists and psychiatrists who use it usually shift from one system of scoring to another and finally administer the test mainly for window-dressing purposes ("scoring" by the inspection method), or else they shift to the use of dominoes. One needs only give an ink spotter his honest "protocol" as if from his maiden aunt and then ponder the personality description obtained.

Hasty popular enthusiasm over cure-all psychotherapies is pitiful and, in our culture, probably excusable; persistent professional promotion of a catch-all scheme for diagnosis may be pitiful, but is hardly excusable in any culture. Handwriting analysis, anthropometric measurement, hormone assays, dream interpretation, free association, picture interpretation, description of ambiguous figures, conversational reaction time and dominance, facial responses (not features any more), polygraphic records of subliminal responses, and so on and on are all significant for the understanding of personality, in a research way, but they are not promising in clinical diagnosis for the selection of therapy unless (*a*) one has infinite time (and the client has lots of money); (*b*) one is dealing with a pathological liar; (*c*) one does not know of, or cannot think of, very many direct and appropriate questions; and/or (*d*) one puts together many clues from such sources. Indeed, the current problem in clinical diagnosis is to detect many clues and to put them together into the most significant pattern rather than to discover one curtain to draw or one button to press.

Experimental evidence in quantitive terms is as imperative in psychology as in any other science and, unfortunately, more elusive. Scientific colleagues in other fields do not help us by proving credulous in ours.

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THE SEA'S PHANTOM BOTTOM

The echo-sounding layer described by Dr. Chapman in the March SCIENTIFIC MONTHLY was discovered off the coast of California by scientists of the Navy and University of California. It has since been observed elsewhere in the Pacific, and in the Atlantic. Dr. Robert S. Dietz, who accompanied the Navy 1947 Antarctic Expedition observed it regularly in the daytime as far south in the Pacific as the Antarctic Convergence.

Originally described as the "deep scattering layer," the phenomenon was recently rechristened the "phantom bottom" by the Hydrographic Office, owing to the erroneous reports of shoals that have resulted from misinterpreted fathograms. A significant feature of the phantom bottom is that it is observable only in the daytime. Within a half hour after sunrise the layer, if present, is recognizable at depths of 125–375 fathoms, and it vanishes after sunset in as short a time. It disappears at night, presumably because whatever is causing it has moved up so near the surface that the return echoes are merged with the outgoing "ping" of the sound device.

The depth at which the phantom bottom occurs is too great for it to be associated with photosynthesis, and the diurnal variation rules out any other physical or chemical process in the sea as the cause. Of biological agents, only zooplankton or nekton can be responsible.

An organism moving vertically 200 fathoms in a half hour is making 40 feet per minute. While zooplankton are known to migrate vertically, it is doubtful whether any of the common forms are capable of such speeds, and Dr. Dietz moreover doubts that they are large enough to scatter sound of fathometer frequencies. As for fish, neither fishermen nor ichthyologists are aware of any forms occurring in abundance sufficient to account for echoes from the equator to polar waters. Furthermore, although fish are capable of swimming at speeds much faster than 40 feet per minute, it is questionable whether they actually do migrate vertically at such rates near the top layers of the sea over distances of several hundred fathoms, owing to the effect of the large changes in hydrostatic pressure on their delicately adjusted swim bladders.

Besides fish, the only other widely distributed component of nekton is the cephalopoda. Squid do not have swim bladders, and their morphology and mode of propulsion render them practically indifferent to hydrostatic pressure. They are known to migrate vertically, rising to the surface at night. Are they present in sufficient numbers to provide echoes over most of the waters of the earth? Judging by the distribution of their predators, they are. Cuttlefish form the sole food of sperm whales, which are encountered, well-fed and fat, everywhere from high northern latitudes to the Antarctic Convergence. One would expect sperm whales to require a concentrated food supply of the order of magnitude of that of their cousins, the whalebone whales, which thrive only in the regions of richest production of zooplankton.

Squid are likewise the sole food of bottle-nosed whales, which are abundant enough to form the object of fisheries in various parts of the world. Squid are the chief food of all the other toothed Cetacea, except killer whales; they are a major portion of the food of the fur seals both of the Bering Sea and of antarctic waters; and they are a large portion of the food of many sea birds. All these warm-blooded animals require proportionately more food than comparable fish or invertebrates, and their abundance implies the existence of a vast cephalopod population.

It is therefore suggested that the "phantom bottom" is made up of the echoes from a concentration of squid, hovering below the illuminated zone of the sea and awaiting the arrival of darkness in which to resume their raids into the plankton-rich surface layers.

JOHN LYMAN

Washington, D. C.

CRUMBLING ROCKS

Dr. Raymond E. Janssen's article, "Crumbling Rocks," in the August SM is a very instructive survey of weathering and erosion, but he makes some statements as matters of fact which might be questioned. He says: ". . . the surface of our moon . . . [where] weathering and erosion do not occur. Because there is no atmosphere and no water on the moon, its surface remains unchanged except for the impact of falling meteors."

Apparently, talus slopes occur on the moon where the temperature changes in a short time from about 250° F. above zero to possibly 150° F. below. Such a change in temperature must disrupt the rock to some extent and be classed as weathering. The falling fragments chip the mountainsides as they slide and roll down. Their effect may be slight, owing to lesser gravity and kinetic energy, but such abrasion as they make is true erosion. There are advocates of the theory that a lunar atmosphere exists. Finally, a "meteor" is the light caused by the ablation of the surface of a meteorite. It is the meteorite that causes the impact.

Dr. Janssen continues: "The profound effects resulting from the combined action of weathering and erosion account for nearly all the geologic changes that occur on the surface of the earth."

The effects are profound, but I believe that they are all the direct or indirect result of impacts of huge meteorites, of which the badly eroded and jumbled crater rims would be more easily interpreted from the moon. Certainly the moon has not had a monopoly on meteorites.

CHAPMAN GRANT

San Diego, California

I have just finished reading the article "Crumbling Rocks," by Professor Janssen, in the August number of SM. It is well written and illustrated but hardly a step in the advancement of science.

I have no quarrel with any statement made in this article. It is all true, but "old stuff" that has been well known for at least fifty years and can be copied from almost any geology textbook. It *is* illustrated with a few new photographs.

It reminds me of what *Time* magazine used to call "Brisbanalities," referring to Arthur Brisbane's talent for telling the public what they already knew.

Yes, Professor Janssen is dead right! Water runs downhill! Rocks disintegrate! Mountains are worn down to plains! Even Isaiah knew these facts, as the Professor points out in his text: "For the mountains shall depart, and the hills be removed."—Isa. 54:10.

He might have used also that other biblical quotation: "There is nothing new under the sun;" and added, "or in this article."

ALLAN O. KELLY

Carlsbad, California

TECHNOLOGICAL NOTES

Photonotes. Because of this issue's attention to photography, releases in that field receive attention. News of this kind gets around rapidly among camera devotees, but some of the items may be novel.

"Needle-sharp" pictures, if not actually guaranteed, are at least made possible by the "Measure-rite" range finder of the Brownie Manufacturing Co. The claim that this instrument "accurately measures all distances from 2½ feet to infinity" has special interest; I shall use it the next time I need to measure infinity.

A light for the microscope and for photography, both micro and macro, said to be "radically different," is made by Fish-Schurman Corporation. It appears to be an arc sealed in a tube of inert gas and almost pinpoint in sharpness—the bright spot is less than a millimeter in diameter.

A camera especially designed for recording oscilloscope traces would appear to have a good deal of usefulness. Fairchild's "Oscillo-Record Camera," said to be the first for such specific purpose, will take still pictures or catch the traces on film moving from 1 inch to 3,600 inches per minute.

Color photographs of the reflections of tiny particles, as revealed by the ultramicroscope, may be better in furnishing information on some substances, such as plastics and rubbers, than the higher magnifications possible with the electron microscope, according to Professor Ernst Hauser, of M.I.T., before an American Chemical Society section. The technique to use, of course, depends on the material and the purpose. Some delicate substances may be "burned" by the penetration of a stream of electrons.

Professional-size pictures are generally 8″ x 10″, the Eastman Company tells us, but the improvements in equipment make it possible to get the necessary detail and accuracy in smaller size, so Kodak's new view camera takes them 4″ x 5″. The quarter-size negative is economical and still can be enlarged and touched up professionally.

Still other Eastman offerings: photographic papers so sensitive and so contrasty that in reproducing business documents clear, sharp prints may be made from worn and faded drawings, and copies may be more legible than originals.

Amateurs can select the proper combination of f-stops and shutter speeds easily with the aid of General Electric's exposure meter Type PR-1. The announcement interests, but doesn't enlighten, by saying that the meter "remembers" conditions and "reminds" the photographer to check the range of light on the subject.

Announcement of 16-mm. movie-camera lenses, fluoride coated to cut down reflection, is a reminder of the practical extent of this recent development. A 30 percent increase in light transmission is higher than I would have expected, but that's what Bausch & Lomb says it is.

Where there is exposure to rays from radioactive material, employers and workers can be put at ease by a service of Tracerlab. A "badge" of film, fresh every week and worn over the heart (as the picture of a pretty girl in the release shows), is developed and noted. Anyone getting excessive doses of radiation can be detected and protected in time.

Stereographic photography with satisfying solidness, according to an article in Bausch & Lomb's periodical, *Educational Focus,* is the "Trivision" method attributed to a Mr. Douglas F. Winnek. It is stated to be much more than a fad, and promising for future development. Instead of being a mingling of views taken with two cameras, this picture is caught in a whopping B & L lens—looking like a locomotive headlight, the account says—and separated in the film, which is faced with tiny plastic buttons, 300 of them to the inch. When a transparency of such a film is viewed, one eye sees part of the picture coming through the buttons—each a tiny lens—and the other sees a composite of slightly shifted images. No stereoscope is necessary.

M. W.

THE BROWNSTONE TOWER

The American Association for the Advancement of Science assumes no responsibility for the statements and opinions advanced by contributors to its publications. Views expressed in editorials are those of the editors and do not necessarily represent the official position of the American Association for the Advancement of Science.

The foregoing statement appears for the first time under the table of contents of this issue and will be repeated in each succeeding issue of the SM. To a thoughtful person it must seem unnecessary to publish such an obvious fact. By its title the A.A.A.S. is committed to the advancement of science. But how science can best be advanced is a matter of opinion that varies among our members. These various opinions are expressed in the SM by its contributors and editor. A published opinion may or may not coincide with majority opinion of the readers on a particular issue. Even if majority opinion on each issue could be determined, it would certainly be unwise to restrict expression of opinion to the views of the majority. Nevertheless, an occasional reader, deeply offended by the views of a contributor, regards his obnoxious ideas as having the tacit approval of the A.A.A.S. and blames the Association, and particularly the editor, for having admitted such "dangerous nonsense" into an Association journal. Therefore the statement quoted above, similar to those in other national magazines, is being added to the SM.

It would be easy to eliminate all contributions to the SM that might irritate some readers. All articles touching on religious, racial, or political questions might be rejected and the contents of the SM restricted largely to the natural sciences. Then no one would clamor for the editor's scalp, but the SM, I think, would gradually and quietly become moribund instead of increasing in circulation. The editor of a general magazine must consider reader interest first, and our readers are certainly most interested in controversial social questions touching science. Both praise and blame following the publication of such articles indicate interest in them.

Many of the unsolicited manuscripts that are sent to the editor are essays presenting the views of their writers on controversial questions. As a rule such manuscripts are not submitted to reviewers because it is immaterial whether the reviewer agrees or disagrees with the opinions expressed. The editor publishes only a few of them and is guided in his selection by the following considerations: The manuscript must hold his attention throughout, it must have literary merit, and must deal with an important subject in a novel way. It should be written by a person of reputation in his field. It is possible that the editor's choice is influenced somewhat by his own views, but it is a fact that he has published many essays containing opinions with which he does not agree. The section of the SM called Comments and Criticisms provides opportunity for readers to point out errors of fact that have escaped the editor or to express their own dissenting opinions. As many critical letters are published as space permits. Thus any bias of the editor can be counteracted and all points of view brought out.

It is unfortunate that at least two months must elapse between the publication of a controversial essay and that of letters pertaining to it. Our critics can help us by writing as promptly as possible after an issue appears. Comments on a particular article or articles should be written for Comments and Criticisms and not for publication as a principal article. If a contributor wishes to write a long essay presenting views opposite to those of a previously published article, he should do so without belaboring the previous article.

Comments and Criticisms in the present issue should be of unusual interest. Two articles that the editor thought would draw fire have done so. These and other opinions are expressed vigorously and at length. New facts and interpretations are presented, and there are more to come. The SM lives and grows.

F. L. CAMPBELL

THE
SCIENTIFIC MONTHLY

FEBRUARY 1948

AGRICULTURAL EDUCATION IN COLOMBIA

W. H. HODGE

Dr. Hodge (Ph.D., Harvard, 1941), associate professor of botany at the University of Massachusetts, has spent much of the past ten years in Latin America. In 1945–47 he served as visiting professor and head of the Department of Botany at the Facultad Nacional de Agronomía, Medellín, Colombia. From 1938 to 1941 he was a teaching fellow at Harvard's Gray Herbarium; during this time he made floristic studies in Cuba and in Dominica, British West Indies. He spent the years 1943–45 in Peru for the Office of Economic Warfare looking for new stands of Cinchona.

HIGHER agricultural education in Colombia is provided by two agricultural colleges, one located at Medellín in the Department (i.e., state) of Antioquia, and the other at Cali, in the Department of El Valle. The school at Cali, originally a state agricultural institution, has been nationalized only recently. Since its current enrollment is small, the school cannot compare as yet in importance with the older Medellín institution, the Facultad Nacional de Agronomía.

The latter school was founded in 1914 as the national agricultural college. In 1937, however, it was incorporated as a *facultad*, or "college," of the National University of Colombia (Universidad Nacional de Colombia), from which it receives its annual budget, derived from the Ministry of Education. The National University, like most Latin-American universities, has scattered branches, or faculties, not only throughout various sections of the capital city of Bogotá, but also as far away as Medellín, Colombia's second city. Recently, in a new ultramodern development, the National University has brought all its Bogotá branches together onto a beautifully landscaped, U. S.-style campus, the Ciudad Universitaria (University City), located on the outskirts of the capital. Two of its branches, the School of Mines and the School of Agriculture, cannot feasibly be moved; these are located in Medellín. There is logic behind this seeming isolation of the University's branches at Medellín, for the State of Antioquia, among other things, can boast of being Colombia's gold-mining center and one of the most important cattle-selling districts; besides, it is the heartland of the republic's best-known export crop, fine-grade coffee. Moreover, Medellín offers other advantages for an agricultural school, for, situated only one mile above sea level—but with cool mountain heights at her doorstep—she can offer better conditions for growing most types of demonstration crops, from temperate potatoes to tropical cacao. Bogotá and her much colder environs can scarcely support anything but a temperate type of agriculture.

The principal aim of the Facultad de Agronomía is to prepare men for a career in Colombian agriculture, but, in addition, re-

FACULTAD DE AGRONOMIA

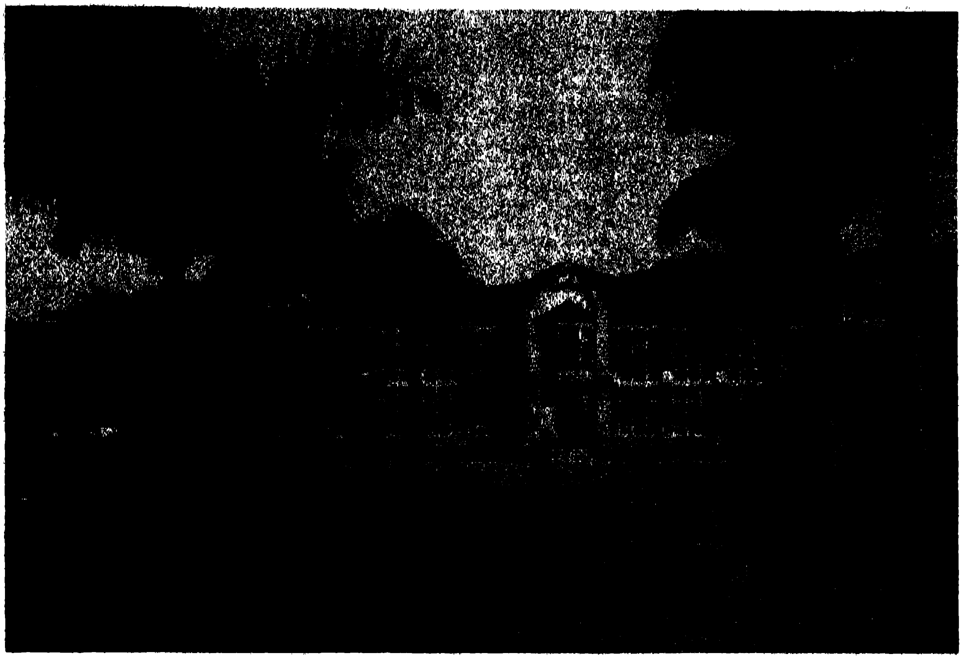
search is carried on by members of the institution. Extension work such as we have in the United States, although initiated, has not been extensively developed as yet; of course requests for aid from farmers or other interested individuals are honored.

Administrative work at the Facultad is in the hands of a dean. General rules and regulations of the agricultural school naturally follow those of the mother-University and in fact are largely drawn up by the directive council of that institution, of which the deans of the various faculties are members. Any modifications of those rules judged necessary by the school at Medellín must eventually be voted upon by the council in Bogotá. The Facultad, in turn, has its own directive council made up of six faculty members composed of a president (the dean), a secretary, two professors elected as representatives for the faculty, and two professors elected for and by the student body.

At the present time the faculty is composed of approximately twenty-five members—not a large number, but in proportion to the small student body of approximately two hundred it is ample. Of the faculty group, only about half can be considered full-time professors, or research men, stationed at the college. The remainder are professors who, though full-time teachers, are attached to, or share their time with one of the other local educational institutions such as the National School of Mines, the University of Antioquia, or its School of Chemistry, and handle certain lectures only at the Facultad de Agronomía. In such positions are the men who give course work in mathematics and related subjects, geology, agricultural economics, and the like. The majority of the professional men are themselves graduates of the Facultad or of one of the other branches of the National University and, as such, usually hold the equivalent of a college undergraduate degree. Several have undertaken postgraduate studies in the States. In fact, Colombians have deep respect and admiration for our agricultural teaching and techniques. They look to the United States for guidance and mutual cooperative aid, they use American textbooks in the agricultural sciences, they send promising students to us for further study, and they aid professional men to further their research by profitable visits to the United States. A number of their federal agricultural men have studied in our country as students financed by their own government.

As a further means of improving either local research or teaching practices, Colombia has sought continually to invite visiting professors for a stay at the Facultad in Medellín. Colombians seem to prefer that these visiting men be North Americans, but these wishes are hard to fulfill because of language difficulties—all too few scientists in the States have the necessary background in Spanish to be effective teachers in that Latin tongue. Nevertheless, the Facultad has managed to attract some half-dozen United States scientists in recent years, including specialists in genetics and improvement of corn, plant taxonomy and general botany, tropical agriculture, and animal husbandry. Visiting scientists have come from such institutions as Iowa State and Ohio State; the Universities of California, Massachusetts, Texas, and Notre Dame; and the U. S. Department of Agriculture. In addition to these scientists, Medellín has sought elsewhere in the Americas for agriculturalists, having had an irrigation specialist from Mexico and a professor of pomology from Cuba.

It is somewhat easier to attract fellow-Latins to the Facultad since for these men



FAÇADE OF MAIN BUILDING, FACULTAD DE AGRONOMIA, MEDELLIN

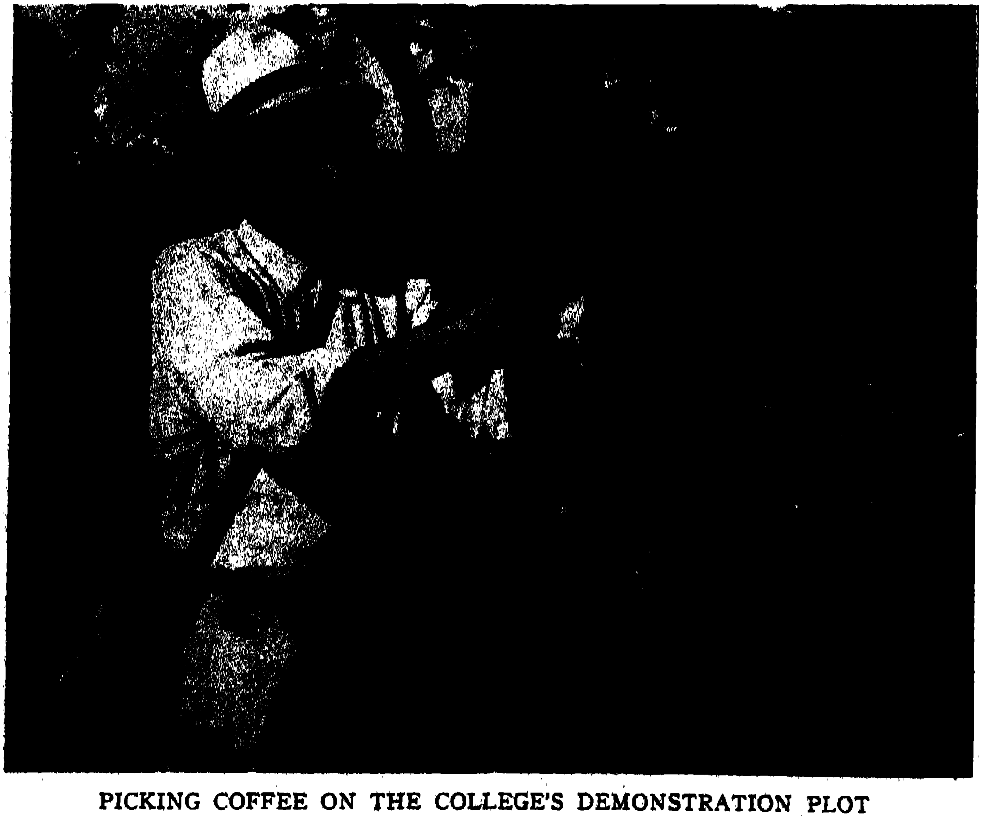
there is naturally no language difficulty and little salary differential. The average U. S. scientific worker can hardly afford to leave the security of his position and higher salary for the pay offered by an educational institution in Latin America. One of the means of accomplishing the interchange of professors has been the funds made available by the Division of Cultural Cooperation of our State Department. Such funds have been utilized by visiting scientists to augment the low salary schedule obtaining in many parts of the other Americas.

In comparison with salaries found in many Latin republics, the top salary of a full professor at Medellin is good, averaging about 8,000 pesos (approximately $5,000) per year. But living costs, if a person is to live "a la U.S.A.," are very high, especially now with postwar inflationary prices. Except for the comparatively poor financial return—when considered over a long period of years—many a North American scientist might be more than pleased to remain permanently in Colombia to pioneer in a scientific field that has boundless frontiers. And it is only by remaining for long periods that a visitor can bring about the greatest mutually beneficial results. Over short periods it is more desirable to send promising Colombian students or professional men to the States for special training.

One of the basic reasons for Colombia's demand for agricultural specialists from North America is the inadequate number of her own agricultural corps. For, despite the fact that she currently graduates up to fifty potential workers a year—some of whom presumably could emerge as professionals—the fact remains that the better men are sooner or later lured, almost without exception, to higher-paid, more attractive commercial posts, often outside the agricultural field. There are jobs in agriculture, but because of the poor pay there are no men to fill them. Colombian government service, which can pay up to 1,000 pesos monthly for an engineer or a specialist in rural medicine, can offer a mere 350 pesos for a plant pathologist, or 280 pesos for an entomologist. (The Colombian peso is equivalent to 57 cents in U. S. currency.) Even the big commercial agricultural interests, such as the wealthy National Federation of Coffee Growers, who control the production of Colombia's most important crop, pay very low salaries to their scientists. Obviously, Colombia must correct this situation, especially since agriculture is her No. 1 industry.

Admission to the Facultad de Agronomía is open to male students sixteen years of age or older who have satisfactorily completed their secondary-school education. These boys hold the Latin-American *bachillerato,* which is the equivalent of our high-school diploma. Although entrance examinations are required by the other schools of the National University, this is not true of the agricultural college, which unfortunately often gets a very poor class of student. In other words, most young men strive to enter one of the other branches of the University, where prestige is higher and the ultimate take-home salary will be greater. Thus the agricultural college often finds itself with an entering freshman class composed largely of students who have been turned down at the Medical School, the Law School, or the School of Mines.

This matter of prestige, which operates in our own schools to a minor extent, is a dominant feature of the Latin student's outlook. His background, like that of all Latins in the Central and South American republics, is one of arts and letters rather than the sciences. This tradition, of course, has its roots in the Iberian Peninsula. Spain's accomplishments have not been in science, and her daughter-republics in this hemisphere have followed in her footsteps. Until science is looked on more favorably and agriculture and other natural sciences are given the prestige they deserve—equal with other fields of endeavor—Colombia and her sister-republics can hardly expect to rear an independent-thinking and -acting corps of agricultural men who have faith in, and love and a deep-seated respect for, their work. Instead she will find her cities disproportionately filled, as at pres-



PICKING COFFEE ON THE COLLEGE'S DEMONSTRATION PLOT

VIEW FROM MAIN BUILDING, FACULTAD DE AGRONOMIA

THE BUILDING IN THE CENTER BACKGROUND IS A DORMITORY.

ent, with lawyers and physicians and her mountains and valleys crying for trained agriculturalists.

Students at the Facultad de Agronomía number about two hundred, and in recent years the enrollment has been gradually increasing. The undergraduate body is representative of all the agricultural regions of Colombia—tropical lowlands to temperate highlands—but the greater proportion of students come from the Caribbean coastal plain centering around Barranquilla and Cartagena. Tuition, ranging from 30 to 400 pesos annually, is logically graduated according to the taxable income of the father, thus permitting an educational opportunity for all classes. Further, certain worthy students lacking even the lowest financial wherewithal are enabled to attend school through funds supplied by their local home town. Colombia is a true melting pot of races, with all degrees of admixture between Spaniard, Indian, and Negro. Students in agriculture are a representative cross section of these many racial combinations.

As might be expected of a student body that is primarily studying agriculture because it can't be studying something else, study is listless and classes are dull, at least to a visiting professor accustomed to the alert and inquisitive U. S. student. While I was teaching at Medellín I failed to have a single student make a request for aid or for suggestions for additional readings in his chosen field. The average Colombian agricultural student will do only enough to just get by, and the diploma rather than what it represents seems to be the thing most desired. To some degree this lack of pep and vim may be laid to the absence of proper facilities for study and true campus life. Dormitories at the Facultad are inadequate, and so most students live in town, usually boarding and renting rooms in rather barren surroundings, where a study desk and quiet are unknown. At present, library study facilities are also

TYPICAL GROUP OF SECOND-YEAR AGRICULTURAL STUDENTS

inadequate, and most students resort to the Latin-American custom of pacing school corridors or patios, textbook in hand, all the while vigorously lip-reading the assignment. Such arrangements are not conducive to interest in any subject, and it may properly be said that the art of real study is foreign to most agricultural students in Colombia. Plans for a new building program have been recently made, and, when the funds have been allocated, the Facultad can look forward to an alleviation of present congested conditions.

Faculty-student relations are for the most part excellent. The visiting professor will find, however, at least one source of irritation—the *huelga estudiantil,* or "student strike." Into this weapon the students pour all the fire and energy that they refuse to give to the improvement of their education. In my opinion, the student strike is doing more to keep down high standards of teaching in Colombia—in secondary schools as well as in the universities—than any other one thing. The *huelga estudiantil* is apparently universal in Latin America; it is a sort of tradition. As used in Medellín, where I have seen strikes at several of the local colleges, it is inexcusable. One student strike at the Facultad de Agronomía lasted several weeks. Its immediate effect on my courses, as on everyone else's, was to necessitate the elimination of practically two weeks of important work, which could not be replaced in the school year. Laboratory preparations requiring several weeks' anticipation were thrown entirely off schedule. These losses were insignificant, however, compared with one of the principal causes of the strike—protest over the appointment and qualifications of a new professor whom the students did not like. Through their protest the students eventually caused the man's dismissal from teaching. If a strike of this sort were directed against poorly prepared professional men who should not be teaching, then such a weapon might be condoned. Unfortunately, this is not always the case, and strikes are sometimes called against competent teachers whose personalities do not appeal to the students, or against men who are giving too "tough" a course, that is, the very teachers who are actually trying to lift educational standards and practices to a higher level. Thus it can come about that as fast as a Colombian institution improves its faculty a student strike can cancel the gain. For this reason the faculty unjustifiably has to "play ball" with the students, upon whom, in the final analysis, their tenure often depends. Until the National University, as well as other Colombian institutions, outlaws the *huelga estudiantil*, it will continue to lose its teachers, who are thus forced unwillingly into other fields by student whim.

THE school year, composed of two semesters, begins in February and ends in late November, with a between-semester vacation of several weeks in July. The curriculum at Medellin has been changed recently from a four-year to a five-year one. The principal reason for this extension undoubtedly lies in the fact that the school receives in its entering class such a mixed assortment of men—so far as their scholastic background is concerned—that it must fill in the necessary gaps caused by faulty secondary-school education. Thus, the first year is weighted heavily with mathematics and includes algebra, plane and solid geometry, arithmetic, trigonometry, physics, chemistry, freehand and industrial drawing, general biology, and introductory botany. Granted that mathematics is important, it is still overemphasized in a school that should stress agricultural subjects. This overemphasis is upheld with the argument that the degree offered (*ingeniero agrónomo*) is not simply one in general agriculture, but is more inclusive, with agricultural engineering entering prominently. The overemphasis of mathematics is an excellent example of a tendency in Latin-American institutions to offer all the subjects under the sun merely for institutional prestige and without carefully limiting the curriculum to the essentials

LIBRARY, FACULTAD DE AGRONOMIA, MEDELLIN or to courses that can be ably and creditably taught.

The curriculum throughout all five years at Medellín is a more or less inflexible one, and students get little freedom of choice so far as subjects are concerned. The courses given after the first year may also be of interest: In the second year the student takes differential and integral calculus, analytical geometry, analytical and organic chemistry, surveying, topography, mineralogy, geology, economic botany, plant propagation, and general zootechny; in the third year courses are agricultural machinery and repairs, agricultural economics, meteorology, agricultural chemistry, mycology, entomology, plant physiology, agronomy, and dairying; fourth-year courses include genetics, phytopathology, hydraulics, statistics, marketing, animal hygiene, horticulture, hybridization, pomology, principal farm and forage crops, temperate crops, and principles of irrigation and drainage; courses in the fifth and final year are tropical crops, control of disease, farm management, silviculture, cultivation of coffee, agricultural propaganda, production of Colombian agriculture, rural construction, agricultural technology, and two elective courses. Postgraduate studies are not offered, but may be given in the future, in which case specialization will be tentatively in fields such as phytopathology, entomology, genetics, taxonomic or physiological botany, chemistry, agronomy, agricultural engineering, agricultural economics, and zootechny. Although the list of subjects is fairly large, the caliber of courses offered is mediocre when compared with similar courses given in agricultural colleges of the United States.

One of the most pressing needs, not only in Colombian agricultural education but in general science education throughout all Latin America, is for good basic texts in Spanish. These should be written for tropical or warm-temperate Spanish America and should not be mere translations of texts designed for North American students. While I was teaching at Medellín, a local scientific supply dealer visited the school, offering, as teaching aids for Colombians, a full supply of typical North American charts—Audubon bird charts (including one on winter birds!), Riker mounts of life histories of North American plants and animals, and other such preserved materials from temperate regions. Needless to say, I laughed good-naturedly in his face and pointed out that we had initiated the construction of local charts using Colombian subjects whenever possible.

The visiting scientist is much surprised at first to learn that at Medellín a number of standard English texts—for instance, in biology, botany, geology, plant physiology, meteorology, soils, and genetics—are used in various courses. Think of a freshman attacking new and difficult subjects with texts written in a still-strange language! The lot of the embryo agricultural student is initially hard. There is a definite field for competent texts in the Spanish language. One big need is a Spanish-English technical dictionary, for many common scientific terms are as yet nonexistent in the Spanish language. There are, perhaps, a few English texts whose contents are sufficiently universal in application to serve Latin America in translated form, but the great majority would have to be either extensively revised or entirely rewritten. The factors in favor of translations from the English are the general high quality and modern presentation of the subject matter and the superior printing methods, for book quality is, as a rule, poor in Latin America—Mexico and Argentina excepted. Of course, preferably, Latin Americans should write their own texts, but this will be difficult until there is a larger corps of true Latin-American scientists. To be sure, a few Spanish agricultural texts are available, but many of these are quite out of date. Only a very few can be recommended for use in institutions of higher learning.

AMERICAN AND EURASIAN GLACIERS OF THE PAST: A PICTURE BASED ON EXISTING ONES

WILLIAM HERBERT HOBBS

Dr. Hobbs (Ph.D., Hopkins, 1888) is emeritus professor of geology and geography at the University of Michigan. He has been assistant geologist, U. S. Geological Survey, associate editor of the Journal of Geology, *director, University of Michigan Expeditions to Greenland (1926–29, '30), and vice-president of the International Glacier Commission.*

A CENTURY ago, when the theory was first promulgated that glaciers of continental dimensions once blanketed northern Europe and northern North America, the only glaciers of which much was known were those high up in the Swiss Alps. The existing continental glacier of the antarctic was not to become known for three-quarters of a century, though the margins of its vast expanse were being approached by three great national exploring expeditions while Agassiz was announcing his theory. The Greenland glacier was first to be explored during the last quarter of the nineteenth century, nearly a half century after Agassiz' discoveries.

In a measure, this explains why the geologists who surveyed and studied the deposits laid down by the Pleistocene glaciers did so with the picture of the little alpine glacier always in their minds. Their assumption that except for size and outline alpine and continental glaciers must be identical was quite without warrant. Even after the opportunity had arrived to learn something of the nature and the behavior of the earth's continental glaciers, glacialists have rather generally evinced but little interest in them.

The first studies of the periglacial area of an existing continental glacier were carried out by the University of Michigan Greenland Expeditions—five expeditions in the years 1926–32. It should not be a matter for much surprise that continental glaciers have now been found to be in their nature and in their behavior essentially different from the diminutive alpine ones, which had so long served as models in interpreting the great glaciers of the past.

The more important of these differences relate to—

1. Their form or model.
2. The slope of their underlying bed.
3. The generally imperceptible slope of their upper surface.
4. Their general lack of internal movement.
5. Their overlying centrifugal wind system.
6. The place and manner of their nourishment.
7. The nature of the bed they override.
8. The place and manner of their deglaciation.
9. The agents and the nature of the deposits they lay down.

The model of a continental glacier is a very flat dome, or shield, though it may be likened to the cover of a hunting-case watch or a cake of very thin batter, the surfaces of which are, with the exception of the relatively narrow intramarginal zone, flat, though everywhere with a hardly perceptible outward slope. Unlike the alpine glacier, there is beneath the continental glacier no definite forward slope of the bed that could bring about a gravitational flowlike internal movement in the glacier after it has attained its maximum size. The radial profile through a continental glacier, such as that of Greenland, could be represented in correct proportions (with vertical and horizontal scales the same) by a thin horizontal pencil line about six inches in length.

The upper surface of an alpine glacier slopes steeply forward by an amount approximately that of its bed, but the surface of a continental glacier, except within a relatively narrow intramarginal zone, is so flat that its gradient is imperceptible to the unaided eye—one or two degrees only of arc—though the theodolite reveals that this slope is always outward from a central divide. Internal ex-

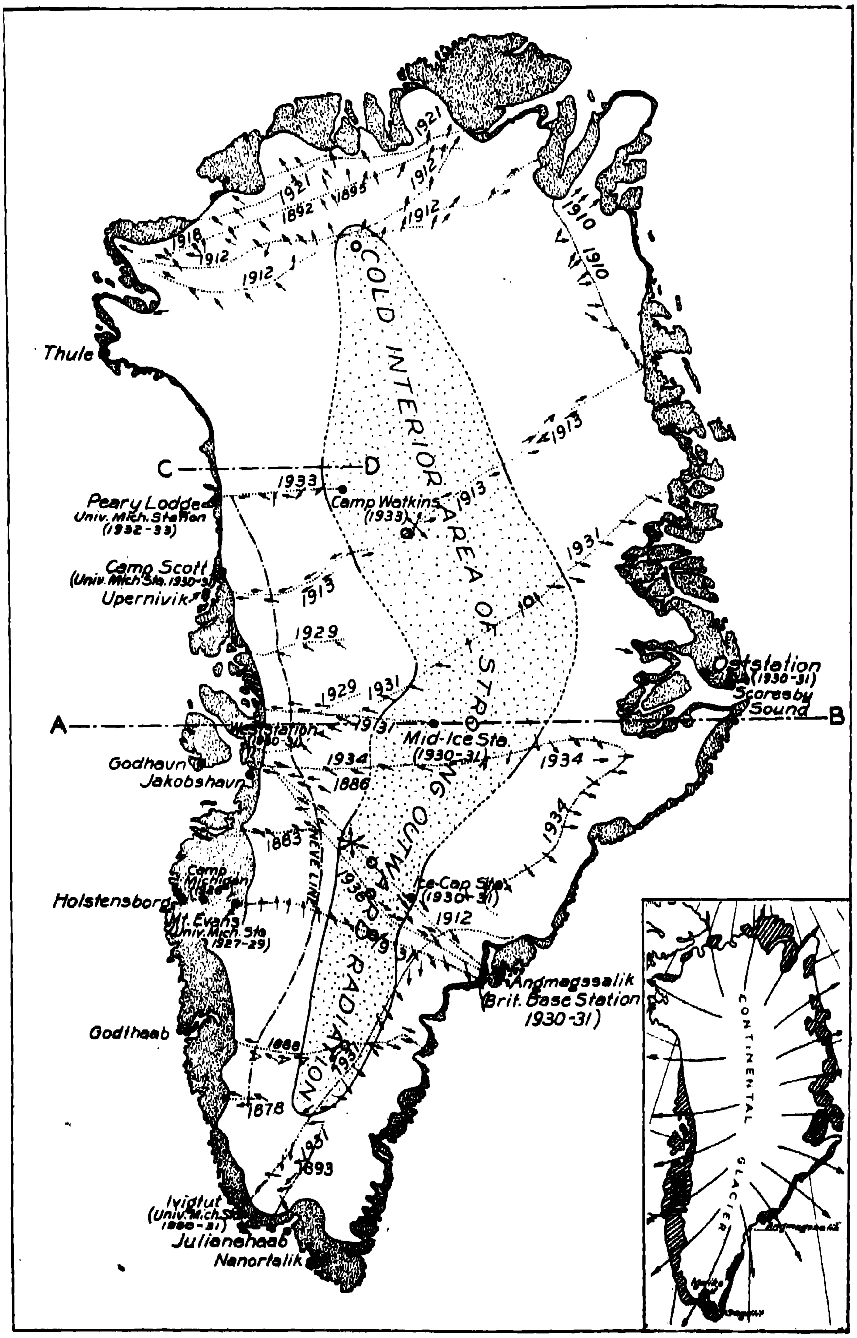


FIG. 1. THE GREENLAND CONTINENTAL GLACIER

THE SMALL MAP (*lower right*) SHOWS THE CENTRIFUGAL WIND SYSTEM (GLACIAL ANTICYCLONE) THAT OVERLIES AND SURROUNDS THE GLACIER. THE DOTTED LINES ON THE LARGE MAP ARE THE TRACKS OF EXPLORERS, AND THE ARROWS ON THE SAME MAP INDICATE THE DIRECTIONS OF THE STRONG WINDS THEY ENCOUNTERED.

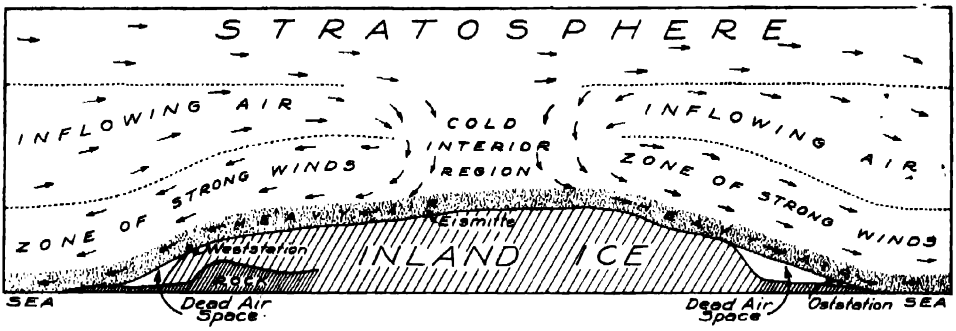


FIG. 2. TRANSVERSE PROFILE OF THE GREENLAND CONTINENTAL GLACIER

SECTION ALONG THE LINE *AB* OF FIGURE 1, BUT WITH THE VERTICAL SCALE ABOUT 300 TIMES THE HORIZONTAL.

trusion-flow after the glacier has attained its full spread must therefore be restricted to the steeper intramarginal zone, within most of which such a flow is betrayed by the cracks (crevasses) on the glacier surface. The surface of the glacier is held to its model, despite its intramarginal extrusion-flow, by the snow that is drifted outward over its surface by the winds of the glacial anticyclone; and its surface layers are, except within the outer zone of summer surface melting, always of snow, never of ice.

A strong centrifugal wind system, the glacial anticyclone, is always present above a continental glacier. These winds extend outward over the periglacial region for at least a hundred miles beyond the glacier margin, and they may extend hundreds of miles beyond it (Figs. 1 and 2). This powerful system, with wind velocities that at times surpass one hundred and fifty miles per hour (Fig. 3), is apparently brought about as a consequence of the refrigeration—abstraction of heat from the air layers close to the glacier surface—and of the consistently outward-directed surface gradient of the glacier.

Though the alpine glacier is nourished by the snow precipitated from ascending currents of moisture-laden air that move in over the glacier from its front, such a movement is not possible for the continental glacier, because of the outblowing winds of the glacial anticyclone. Its nourishment is supplied by the ice needles of the cirri contained in descending air currents from the substratosphere within the downdraft of the central core of the anticyclone (Figs. 1 and 2). These ice needles, adiabatically vaporized during the central downdraft, are, as fine "frost snow," precipitated just above the glacier surface and are swept outward toward the borders by the winds of the glacial anticyclone. At stations set up in this downdraft over Greenland, such snows and driftings occurred on more than half the days that the stations were occupied. The nourishment of a continental glacier is thus supplied by descending air currents, both above the cold central area and, by drifting, over all flanks of the glacier as well.

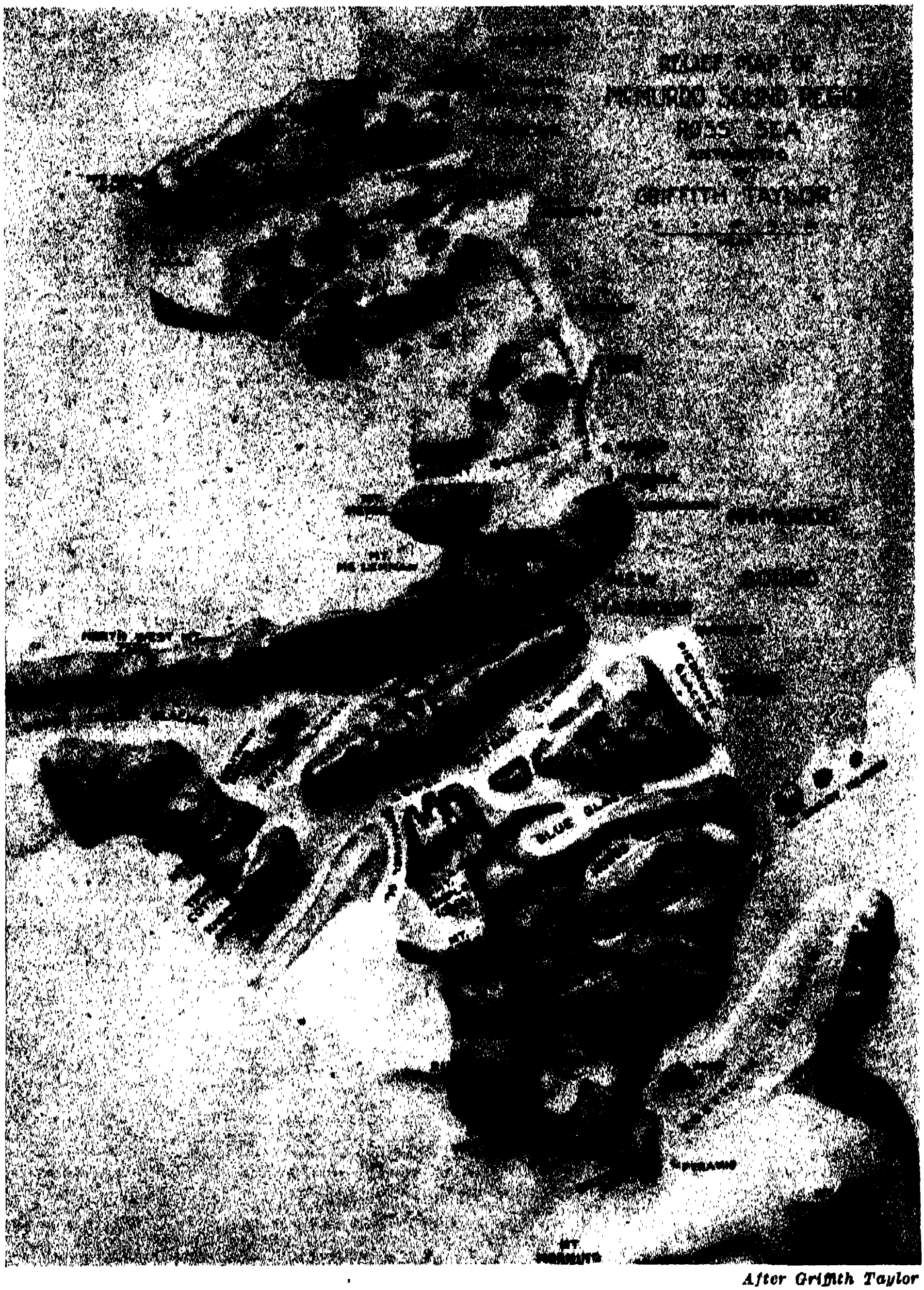
This central area of downdraft of the glacial anticyclone is very sharply delimited on all sides. Explorers, as they enter it, are struck by the much lower temperature of the air, and by the greater daily range of air temperature, though this border is marked by no topographic break (Fig. 1).

The alpine glacier is perched high upon

After Mawson

FIG. 3. LEANING ON THE WIND

FACING THE ANTICYCLONE, THE MAN IS CHOPPING ICE.



After Griffith Taylor

FIG. 4. THE BORDER OF THE ANTARCTIC CONTINENTAL GLACIER

THE ROUNDED SUMMITS OF GLACIATED MOUNTAINS HAVE BEEN EXPOSED BY DOWNMELTING OF THE SURFACE OF THE CONTINENTAL GLACIER. THE MEASURE OF DOWNMELTING HAS HERE BEEN IN EXCESS OF 2,000 FEET.

a bed too steep to hold weathered mantle rock. It has therefore had throughout a bed of indurated rock, which by reason of its forward downslope the glacier has been competent to pry off in blocks. With these as tools the bedrock is ground down and glaciated (planed off, polished, grooved, and striated). In the process the blocks are themselves ground away on the faceted surfaces, so that the deposits of the alpine glacier are powdered rock in which are imbedded faceted boulders and pebbles.

The continental glacier lacks a definitely sloping bed and so advances over weathered mantle rock, or regolith, with its included residual rounded blocks of the harder rocks, such as granite, gneiss, and the crystalline rocks rather generally. This regolith material it ploughs or scrapes up, together with any trees, turf, or soil that had covered it. The rounded residual boulders are for the continental glacier the ready-made tools with which it can glaciate the bedrock. These residual blocks of the crystalline rocks are sometimes of gigantic dimensions, and after transportation by the glacier they are left, except as drilled by flying sand, with their outer surfaces but little changed. They apparently travel close to the front of the glacier, and the larger ones are not overturned during transit, since they are found to be glaciated (planed off and polished) on a single side only, the under one. They are found, as a rule, not on the land the glacier has overridden, but outside on the periglacial areas, over which they have moved in melt-water floods while encased in detached islets of glacial ice. Quite by contrast with the faceted and glaciated surfaces of the boulders of alpine glaciers, they may be described as saxums (from *saxum glacie transportatum*).

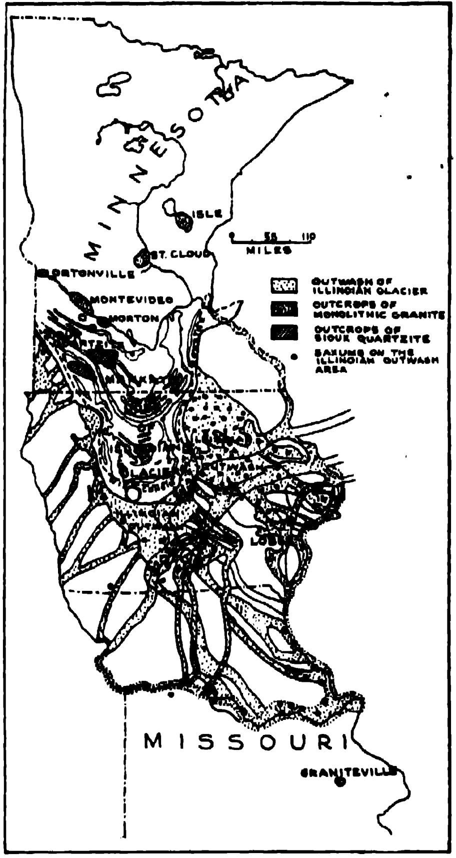

MINNESOTA
ISLE
ST. CLOUD
ORTONVILLE
MONTEVIDEO
MORTON
0 55 110
MILES
OUTWASH OF ILLINOIAN GLACIER
OUTCROPS OF MONOLITHIC GRANITE
OUTCROPS OF SIOUX QUARTZITE
SAXUMS ON THE ILLINOIAN OUTWASH AREA
GLACIER
OUTWASH
LOBE
MISSOURI
GRANITEVILLE

FIG. 5. A FORMER GLACIER

A LOBE OF THE NEXT-TO-THE-LATEST GLACIER IN NORTH AMERICA, HERE DESIGNATED THE MANKATO LOBE OF LATE ILLINOIAN AGE. THIS IS SURROUNDED BY AN OUTWASH PLAIN WHICH ONCE HAD A GREAT NUMBER OF GRANITE SAXUMS SCATTERED OVER IT.

The place and the manner of deglaciation during the receding hemicycle of glaciation are as different as possible for alpine and continental glaciers. The alpine glacier is nourished by snow deposited near its head, far up near the crest of a lofty mountain range, but it is melted far below near its foot, where much higher air temperatures and, far more important, where warm summer rains prevail. Melting of the glacier is therefore concentrated at the glacier foot, and as it proceeds the glacier front retires up the valley. The sediment-filled melt water that issues from the glacier front is on so steep a slope that it has a swift current, and its burden of rock debris is largely carried down to the distant lowland, where it is widely dissipated. Glaciofluvial deposits thus have small importance in the case of alpine glaciers.

With continental glaciers it is quite different. Study of the Greenland and the antarctic glaciers has shown clearly that in the deglaciation that characterizes the present era, when all glaciers are in the receding hemicycle of glaciation, these glaciers are melting on

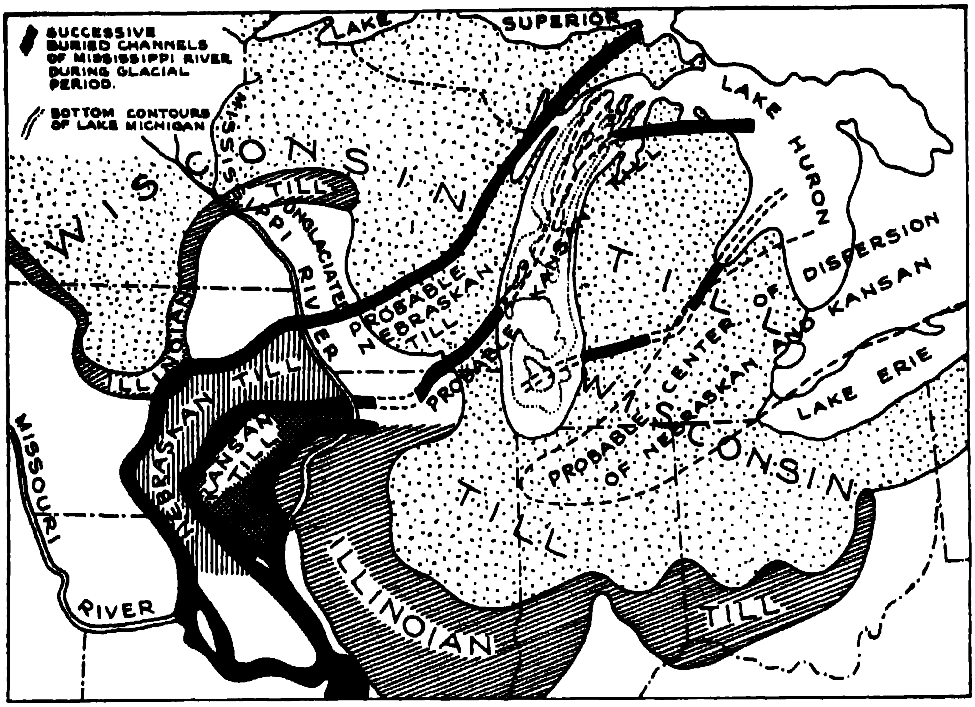


FIG. 6. GLACIAL MAP OF THE UPPER MISSISSIPPI VALLEY REGION

SOMEWHAT EXAGGERATED AS TO WIDTH, THIS MAP SHOWS THE MELT-WATER RIVER CHANNELS MARGINAL TO THE NORTHERN FLANKS OF THE THREE EARLIER GLACIERS—THE NEBRASKAN, KANSAN, AND EARLY ILLINOIAN. TO THE NORTHWEST OF THEM IN IOWA AND MINNESOTA THE MAP SHOWS THE FRONTAL LOBES OF TWO LATER GLACIERS, THE LATE ILLINOIAN AND THE LATE WISCONSIN, BUT FROM THEIR FRONTS THE LAND SLOPED AWAY, AND IN PLACE OF MARGINAL RIVERS THERE WERE OUTWASH PLAINS, WHICH HAVE NOT BEEN REPRESENTED HERE.

their upper surfaces, not back from their fronts as do alpine ones (Fig. 4).

Melting of glaciers takes place only in the summer seasons. By late midsummer the glacier surface, within an outer frontal zone (in West Greenland some tens of miles in width), is always covered with melt-water rivers and lakes. These superglacial rivers flow outward downslope to descend in the intramarginal crevasses, continue through subglacial tunnels, and issue from beneath the glacier front under hydrostatic pressure and with a strong current. During the day the melt water floods the land surface throughout, over the frontal periglacial area, but toward evening the outflow is reduced to streams within channels of braided pattern. The glaciofluvial deposits thus build up an outwash plain whose inner margin is the glacier front and whose outer border is of dendritic pattern. From this front small icebergs become detached through undermining, and these float off loaded with boulders and finer englacial rock debris of all sizes. These bergs either float out to the sea in the outwash channels, or during the floodings of the plain they are left stranded upon its surface, where the ice melts and leaves the boulders behind.

At the end of the summer season the glacier melt water ceases to issue, and surface layers of the outwash plain dry out. The outblowing winds of the glacial anticyclone, throughout the summer powerless to lift even the finest silt, now take over as the transporting agent, and under their action the plain becomes a deflation area. Not only silt, but sand and even pebbles, are lifted and carried in the air across the outwash plain—the pebbles by saltation, the coarser sand a few feet only above the ground, the finer sand and silt horizontally at higher levels. Left behind as

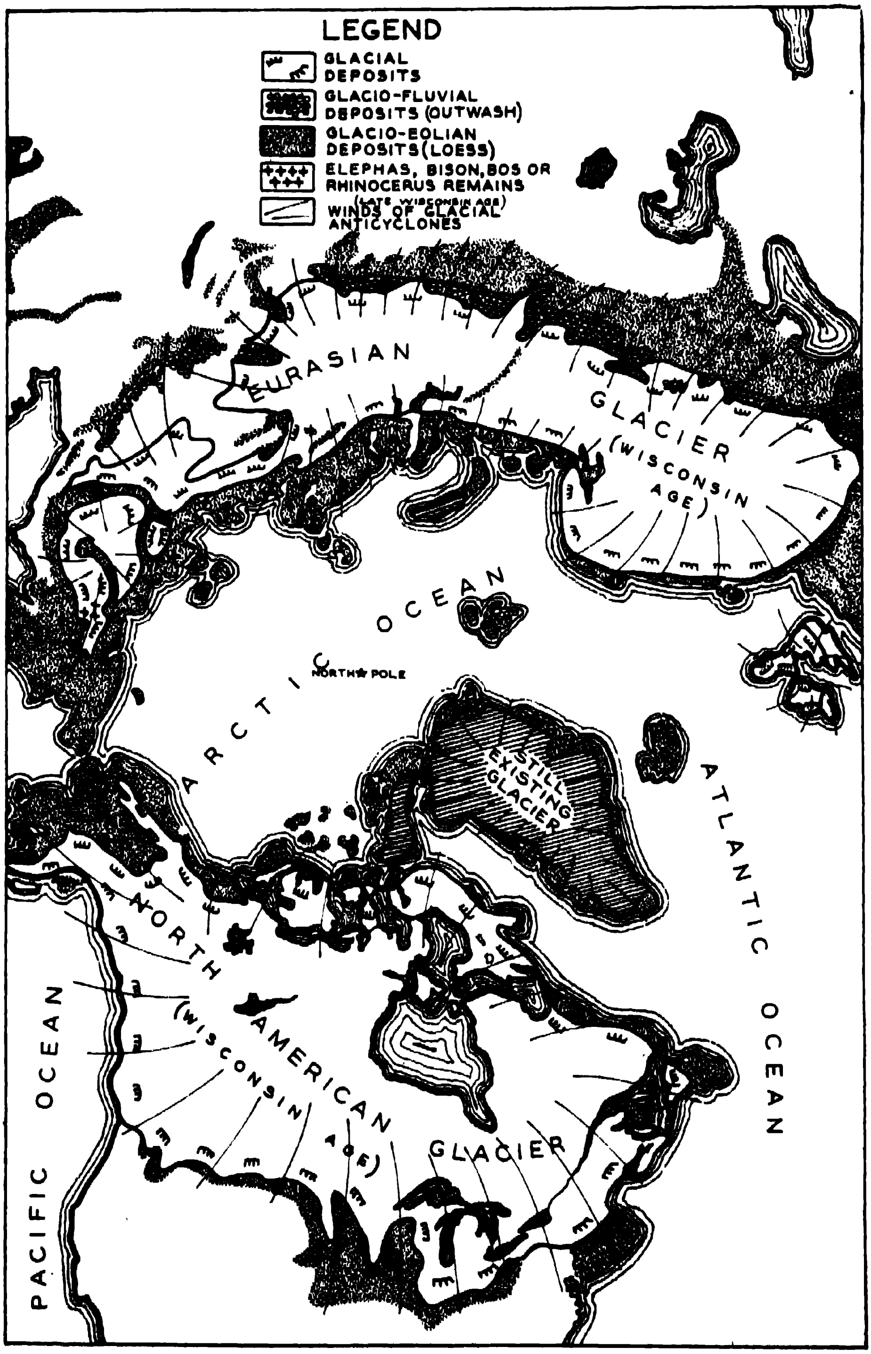

LEGEND
GLACIAL DEPOSITS
GLACIO-FLUVIAL DEPOSITS (OUTWASH)
GLACIO-EOLIAN DEPOSITS (LOESS)
ELEPHAS, BISON, BOS OR RHINOCERUS REMAINS
(LATE WISCONSIN AGE)
WINDS OF GLACIAL ANTICYCLONES
EURASIAN GLACIER (WISCONSIN AGE)
ARCTIC OCEAN
NORTH POLE
STILL EXISTING GLACIER
ATLANTIC OCEAN
NORTH AMERICAN GLACIER (WISCONSIN AGE)
PACIFIC OCEAN

FIG. 7. THE LATEST GLACIERS OF NORTH AMERICA AND EURASIA

THESE GLACIERS WERE SURROUNDED BY APRONS OF GLACIOFLUVIAL AND GLACIOEOLIAN DEPOSITS. THE CORRESPONDENCE IN AGE OF THESE APRONS OF OUTWASH AND LOESS WITH THE GLACIERS THEY ENCLOSE IS ATTESTED BY THEIR VERTEBRATE FOSSILS. THE EURASIAN AREA OF THIS MAP IS BASED ON A RECENT U.S.S.R. ATLAS.

an armor to the plain is a coarse "pebble band" made up of materials too heavy to be lifted, and this band of armor soon halts the deflation process. While in the air the coarser sand bombards the stranded boulders and transforms them into ventifacts; if these are intersected by dikes or veins, into those of ribbed types, deeply recessed.

The floods of the next succeeding summer redistribute the surface pebble band to the plain and whirl the stranded boulders about so as to expose a new face to the winter sandblast, thus ultimately giving the boulders an ellipsoidal form. Repeated movements through successive summers keeps them always on the surface of the plain as it rises. With the retirement of the glacier front the latest pebble band alone survives as a permanent armored surface to the plain, together with the scattered ventifact boulders.

The silt, which has traveled beyond the outwash, has been deposited and held down by the tundra vegetation, for plant growth is inhibited on the outwash itself by the floodings throughout the growing season. Thus, during the deglaciation of a continental glacier, wherever the general land surface has a slope *away* from the ice front, an outwash plain will have formed as an apron to the glacier, and outside it on the periglacial area an apron of glacioeolian deposit (silt or loess), which is thickest next the outwash and which thins out gradually throughout a distance of one hundred miles or more.

The deposits thus laid down over the periglacial region during the receding hemicycle of continental glaciers may be no less important than the glacial ones. Some thousands of cubic miles of melt water issuing on a single glacier flank, and all of it highly charged with suspended sediment, cannot be left unconsidered. It is probably because melt-water deposits are so unimportant in the case of alpine glaciers, and because these have been the models in interpreting the Pleistocene deposits, that the glaciofluvial deposits have been generally overlooked by students of glaciers.

It must be clear that outwash aprons will be formed only where the general slope of the land is *away* from the glacier front. If *toward* it, their place will be taken by a great marginal melt-water river. Such marginal melt-water rivers flowed mainly in the summer seasons, but they acquired proportions large as measured even by our present Mississippi River. For the three earlier of the Pleistocene glaciers of North America, such melt-water rivers are revealed by channels cut deep in bedrock, but now filled to the brim by outwash deposits of the later glaciers (Figs. 5 and 6).

The lobes of the later glaciers of the Upper Mississippi Valley in Iowa and Minnesota, the Late Illinoian and the Late Wisconsin, spread out southward over a flat divide from which the land sloped away on all sides. Both were in consequence surrounded by inner aprons of outwash with loess aprons outside.

Heretofore regarded as true glacial deposits (Iowan), this "two-story" outwash plain of glaciofluvial deposits has a lower portion (Illinoian) that is largely buried beneath the upper (Wisconsin). Both outwash plains have a surface pebble band, and the upper plain has scattered ventifact boulders, which have been drilled by the sandblasts. By contrast, the lower plain was once strewn with gigantic saxums glaciated on their under surfaces and projecting through the surface of the overlying deposits. They have now disappeared because they have been quarried for local building stone.

Glaciofluvial and glacioeolian deposits of the Pleistocene of North America thus offer a close parallel with those observed in Greenland off the border of its still-existing continental glacier. The North American and Eurasian glaciers of the latest (Wisconsin) stage (Fig. 7) have been rather generally surrounded by inner aprons of outwash gravels and outer ones of loess.

THE TOX LAB

JOHN O. HUTCHENS

A native of Noblesville, Indiana, Dr. Hutchens received his Ph.D. from Johns Hopkins University in 1939. He was a National Research fellow in zoology at Harvard during 1939–40 and a Johnston scholar at Johns Hopkins in 1940–41. Dr. Hutchens then became professor of physiology at Chicago and is now director of the Toxicity Laboratory there.

IN THE rear of a big parking lot on the University of Chicago campus sits a group of low tile and brick buildings surrounded by a high wooden fence topped by several strands of forbidding barbed wire. Close by stands the smokestack of the old University powerhouse, and into its base a bank of huge blowers pours the air from a maze of 18-inch ducts that converge from the laboratories inside the fence. Throughout the war these outward aspects were all the outside world was allowed to know about the University of Chicago Toxicity Laboratory.

Scientists of the United States and allied nations engaged in chemical warfare research knew, however, that to the Toxicity Laboratory were funneled all the new candidate chemical warfare agents synthesized by O.S.R.D. chemists throughout the nation. There they were tested to find out whether they were lethal if inhaled, would blister the skin, or do any of the possible kinds of damage that would allow them to supplement or replace the mustard gas, phosgene, and other war gases of World War I, including the highly touted Lewisite.

The administrative history of the Toxicity Laboratory goes back to April 1941 when a contract was established between the University of Chicago and the National Defense Research Committee of the Council of National Defense. Dr. E. M. K. Geiling, professor and chairman of the Department of Pharmacology, was named official investigator, and Dr. Franklin C. McLean, professor of pathological physiology, was appointed as the first director in charge of research. In two years, under the direction of Drs. Geiling and McLean, the Toxicity Laboratory grew to its present size from the single original building comprising one experimental laboratory and animal quarters, with a staff of half a dozen scientists working.

Today the staff numbers half a hundred scientists and technicians working in seven large laboratories and an administration building inside the big fence and in laboratories in the Departments of Physiology, Pharmacology, and Pathology. The large animal quarters house all sorts of animals, from monkeys and mice down to the cockroaches and silkworms used in studies of insecticides and basic problems in biology.

In 1943 Dr. McLean resigned as director to accept a commission (as Lt. Colonel, Medical Corps) for service with the Medical Division, Office of the Chief, Chemical Warfare Service. Dr. R. Keith Cannan, professor of chemistry, New York University, became the second director of the laboratory and served until March 1945. From May 1942 until this time Dr. William Bloom, professor of anatomy at the University, was in charge of testing the skin-blistering properties of compounds and the effectiveness of protective ointments.

In March 1945 the O.S.R.D. was being dissolved, and support of U.C.T.L. was undertaken by the Chemical Warfare Service. Dr. William L. Doyle, now associate professor of anatomy, became director and served until March 1946, when I succeeded him. Throughout the period of its existence, the directors and staff have had the advantage of a large Advisory Committee with representatives from many departments in the Biological and Physical Science Divisions of the University of Chicago.

To those who worked there through the hectic days of 1941–43 the growth in size of the physical plant of the Toxicity Laboratory was an easy accomplishment as compared with assembling a staff, defining research

VENTILATION SYSTEM, TOX LAB

problems, and gaining the experience necessary to interpret experimental results in terms of the needs of the armed services. In common with other scientists throughout the country who were requested to engage in defense research, those at Toxicity Laboratory asked one another, "What do they want to know?" Not only were we unacquainted with the strategic and tactical problems of the military but we had not the slightest knowledge, at the outset, of the state of development to which the pitifully few scientists in the laboratories of the Chemical Warfare Service at Edgewood Arsenal had been able to bring their problems. A second problem in staffing a wartime laboratory was created by the shortage of trained scientists. Men had to be obtained, some virtually kidnapped from prewar jobs, to do whatever was to be done.

In the meantime, something had to be done about the tens, then hundreds, and finally thousands, of chemical compounds that were arriving by mail, express, and personal messenger. Each, it was the fond hope of the chemist who had synthesized it, was the new superchemical warfare agent. They had to be tested. What would they do? As soon as the staff undertook such tests, one thing became abundantly clear—the sum total of knowledge in biology, medicine, and the physical sciences would have to be used. So, of their own volition, or responding to urgent pleas for help, pharmacologists, physiologists, biochemists, pathologists, chemists, physicists, mathematicians, ophthalmologists, and dermatologists joined the group.

Compounds were tested for every manner of effect they might have on the animal body—first, of course, for toxicity when inhaled. But this was not enough. Did it cloud the eye or blister the skin as did mustard gas? Did it make a man cry, or sneeze, or his skin itch? And then, if it did any of these things, why? And what could be done about it? How good were the gas masks, the antigas ointments, protective clothing? Along with other O.S.R.D. laboratories and the rapidly growing Army and Navy installations, Toxicity Laboratory scientists tackled all these problems.

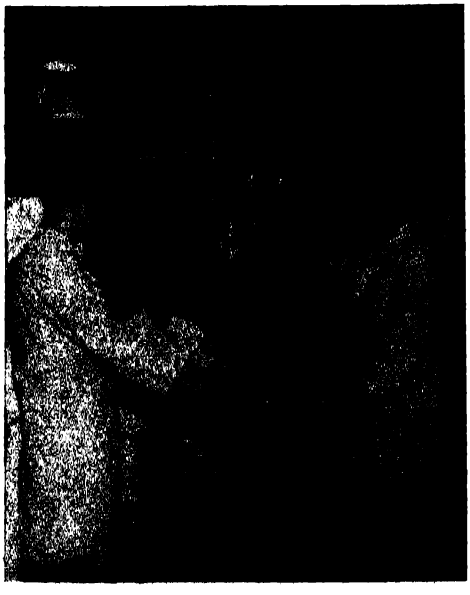
The answers of general interest have either been, or will be, published in open scientific journals. Others of more direct military importance still reside in secret or confidential reports. Many of the contributions to medicine that grew out of this work have been outlined by Bodansky (*Science*, **102**, p. 517).

TODAY, Toxicity Laboratory scientists are having an opportunity to pursue some of the ideas that occurred to them as they investigated the several thousand chemicals submitted to the laboratory during the war. Many compounds useless as chemical warfare agents were put aside in the knowledge that they acted in strange ways on one or more of the organs or tissues of the animal body. It is a curious fact that scientists learn most about the normal function of these tissues and organs by observing the effects of departure from normality. The importance of an endocrine gland is discovered when it is removed or if for some reason it becomes hyper- or hypofunctional. The metabolic role of a particular enzyme is disclosed if its activities are inhibited. Among the compounds studied, many appeared that would cause a chemical removal of an enzyme, a cell type, or an organ from participation in the body economy. Others stimulated organs to excess activity. Here were chemical tools to be exploited in physiology, pharmacology, biochemistry, and clinical medicine!

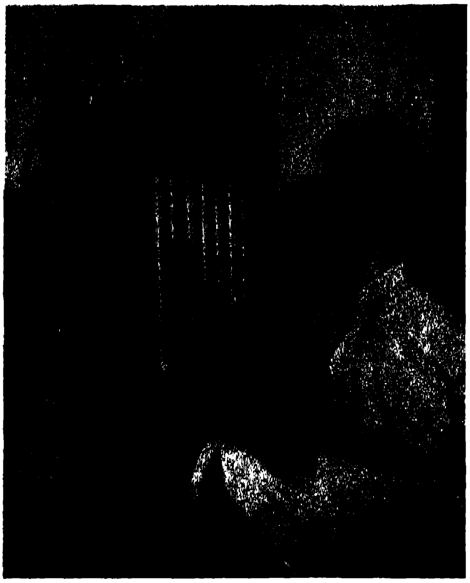
Probably the best known of the agents discovered in chemical warfare research are the nitrogen mustards. This story began at the Toxicity Laboratory in 1942 when Dr. C. C. Lushbaugh, pathologist, first noted that mice gassed with nitrogen mustards had many fewer white blood cells than normal and that the bone marrow and lymph nodes of the animals no longer were forming blood cells.

This finding stimulated various groups, including Drs. Leon Jacobsen and Charles Spurr, in the Department of Medicine at the University of Chicago, to test the effectiveness of the nitrogen mustards against certain diseases such as leukemia, lymphosarcoma, and Hodgkin's disease, which are characterized by the presence of abnormally large numbers of white blood cells. The compounds were found to have effects like those of X-rays.

What is not so well known is that there are many "nitrogen mustards" yet to be tested, first on laboratory animals and, if the findings warrant, on patients suffering from those neoplastic diseases. In 1942, when intelligence reports indicated that the Germans were experimenting with an agent called "Stickstoff Lost" (*Stickstoff*, "nitrogen"; *Lost*, the German name for the mustard gas of World War I), an intensive search for a nitrogen-containing compound that had the properties of mustard gas was instituted. O.S.R.D. chemists synthesized a large number of such compounds, and these were tested at the Toxicity Laboratory. Those most toxic, most vesicant, and most damaging to the eyes were studied in great detail.



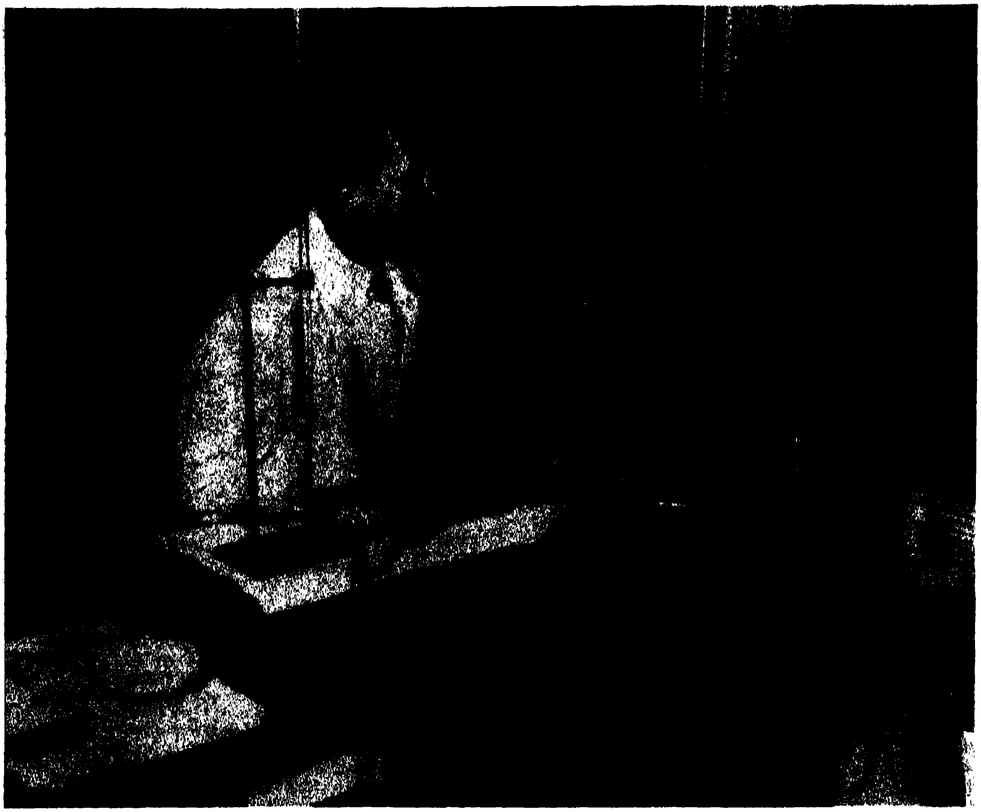
TESTING PENICILLIN AEROSOL



STUDYING OXYGEN CONSUMPTION

All were amines (organic ammonialike compounds) bearing at least one β-chloroethyl group. One of these, bis (β-chloroethyl) isopropyl amine, received the most attention in clinical studies. Nothing was more natural than to employ the compound about which most was known from animal studies. One must bear in mind, however, that the object of the original experiments was to find a very toxic compound. For therapeutic purposes, this must be used in small doses. In recent months it has been possible to restudy some of the nitrogen mustards set aside as relatively nontoxic in 1942–43. It has been found that some of these affect white cells while having a lesser toxicity; that is, they have a better potential "therapeutic index." More nitrogen mustards can be synthesized for testing.

The nitrogen mustards are only one of the



INVESTIGATING PHARMACOLOGICAL ACTION OF A DRUG ON A RABBIT

groups of compounds of interest in cancer research. Among the several thousand chemicals tested during the war are many related to types that have shown some indication of activity against cancer. From this store and from the accession lists of the Chemical-Biological Coordination Center of the National Research Council, new candidate agents are being drawn in the search for one more effective against cancer. Through the Chemical-Biological Coordination Center, compounds stored at the Toxicity Laboratory are made available to other scientists interested in using them for cancer research, to produce genetic mutations, as enzyme inhibitors, and for many other purposes.

Potent poisons discovered during the war also have potential usefulness in the control of insects and rodents. The well-known 1080 (sodium fluoroacetate) was first studied, in the form of the methyl ester, as a chemical warfare agent.

Among the compounds at the Toxicity Laboratory are many having a high toxicity for rats, mice, insects, and other pests. They cannot be employed as insecticides or rodenticides, however, until intensive investigations have established several other facts about them. The requirements for an ideal insecticide or rodenticide are many. It should have little toxicity for desirable animals, its symptoms of poisoning should be distinctive, and there should be a method of treatment in case of accidental poisoning. These and many other properties of the compound must be studied before field trials are undertaken. Since 1945 a large group of workers in the Toxicity Laboratory has concentrated on the study of insecticides and rodenticides, investigating both new compounds and extending observations on those already in use.

All the available rodenticides fail in one or more ways to match the ideal. For example, rodents exposed to a nonlethal dose of ANTU (α-naphthyl thiourea) become resistant, so that further dosing is ineffective. It is potent against the Norway rat, but not so effective against the Alexandrian rat. 1080 is poorly accepted by some rodents, and it is more toxic for other animals, such as dogs and cats, than for rodents. No effective specific method for treating ANTU or 1080 poisoning has yet been discovered. Nevertheless, both compounds have been extremely useful in rodent control, and probably will continue to be used until better agents are found.

Groups under Drs. K. P. DuBois, G. H. Mangun, J. M. Coon, and J. O. Hutchens are studying ANTU, 1080, the German rodenticide "Castrix," and a variety of other agents, primarily phosphorus compounds in which many phosphorus atoms are linked together by oxygen atoms (esters of polyphosphoric acids). Scientists interested in the results of these studies are rapidly informed through the medium of a bimonthly progress report. When brought to a satisfactory stage of completion, the results are published in scientific journals.

Of the many compounds studied, the polyphosphoric acid esters are among the most interesting. During the war di-isopropyl fluorophosphate (DFP), which contains only one phosphorus atom, was discarded as a war gas, but its ability to inhibit an enzyme (cholinesterase) important in nerve conduction suggested it as a possible substitute for the drug eserine. At the University of Pennsylvania, Drs. Irving H. Leopold and Julius H. Comroe found it useful in reducing the abnormally high tension of the eyeball occurring in glaucoma. These workers and Dr. A. McGehee Harvey and others at Johns Hopkins tested DFP in myasthenia gravis, a wasting disease of the muscles that can be treated by eserine. In some respects it was more effective than eserine, but there were undesirable side effects.

TESTING THE EFFECTS OF MUSTARDS ON ANTIBODY DEVELOPMENT

When hexaethyl tetraphosphate (a four-phosphorus compound) was introduced as an insecticide, Drs. Mangun and DuBois suspected that, like DFP, it acted by inhibiting cholinesterase. This proved to be true, so a long series of polyphosphate esters were investigated. One in particular, tetraethyl pyrophosphate (TEPP), is a more potent anticholinesterase than any other tested. By altering the chemical groups surrounding the phosphorus atoms, the enzyme-inhibiting action of the polyphosphates and other of their chemical properties can be altered almost at will. The potential practical usefulness of these compounds is in itself enough to stimulate investigation. To the physiologists, pharmacologists, and biochemists pursuing the study, the additional light that may be shed on mechanisms whereby nerves conduct their impulses is an even more intriguing problem.

Experimental techniques developed during the study of inhalation of toxic compounds also have been found important for peacetime use. Among the chemicals studied at U.C.T.L., many consisted of such large complex molecules that they were nonvolatile and had to be dispersed in the air as finely divided mists or dusts (aerosols).

Methods were developed for measuring the size of these aerosol particles and for studying their behavior in the respiratory tract. It was of course known that a large dust particle, if breathed through the nose, would be removed there and would not reach the lung, and that fine particles such as those in tobacco smoke could be breathed into the lung and then many of them exhaled. The techniques developed by Dr. H. D. Landahl and his associates permit quantitative measurement of the percent of particles of any given size and density that would be retained in the nose or lung at a given rate of breathing. Inhaling aerosols of corn oil, sodium bicarbonate, calcium phosphate, and glycerine water mixtures, they learned the importance of particle diameter, density, velocity of the particle as determined by breathing rate, and mean time spent by the particle in the lung.

These techniques have already proven useful in studies of the effectiveness of aerosols of the antibiotics, penicillin and streptomycin, in treatment of respiratory infections. It has been possible to measure the actual dose of antibiotic retained in the body and thus compare it with the amount that would have to be injected to obtain a similar therapeutic effect. In collaboration with Dr. Robert Bloch, professor of medicine, and Dr. William Adams, associate professor of surgery, Drs. Erwin Levin and G. H. Mangun, of U.C.T.L., have studied the effect of penicillin and streptomycin aerosols on a variety of respiratory infections.

The information gathered in these and Landahl's studies promises to be of use in even more important ways. Such diseases as silicosis and asbestosis result from retention of fine dust particles in the lung. Knowledge of nasal filtration and the retention in the lung of particles of known size should permit more accurate prediction of the hazard created by industrial dusts and fogs.

THESE are but brief sketches of some of the varied research interests of Toxicity Laboratory. With a staff composed of members of the Departments of Physiology, Pharmacology, Biochemistry, Mathematical Biophysics, Pathology, and Medicine, interests are bound to be broad. To the scientists of each of these fields every new problem offers a special challenge and is susceptible to a different mode of attack—each new finding has a special significance. More than six years of association, struggling with common problems, has taught these scientists to depend on each other for stimulation, advice, and assistance.

This mutual respect and dependence makes itself felt in those departments of the University that furnish staff members in the Toxicity Laboratory. Students reap the benefit of the broadened experiences of their instructors. Some graduate students carry out in the Toxicity Laboratory the research on which their Ph.D. theses are based, working under a staff member from their department of specialization. Thus, these graduate students, the research men of tomorrow, can draw on the store of new techniques and new chemical tools as they are being discovered.

THE LIFE OF THE WATER FILM*

LORUS J. MILNE AND MARGERY J. MILNE

Reprinted by permission from Natural History *(June 1947), the following article won honorable mention in the 1947 AAAS-George Westinghouse Science Writing Awards. The Milnes are free-lance writers whose fascinating natural-history stories and photographs have appeared in such magazines as the* Atlantic Monthly, Fauna, Nature Magazine, *and* Popular Photography. *Dr. Milne is associate professor of zoology, University of Vermont; Mrs. Milne is assistant professor of botany at the same University.*

ONE of the first facts learned by every child is that "water is wet." In more mature years this wetness is so taken for granted that any exceptions to the rule arouse great interest. Thus the sewing needle that can be lowered gently onto the surface of a tumblerful of water, there to float completely dry, is a startling discovery. Yet to a large number of different animals and plants, this problem of wetness and dryness is a matter of life and death. Many of them find the "dry" surface of water to be a place to live, albeit precariously. Suspended between the air above and the depths below, they inhabit the surface film of ponds, streams, lakes, and even oceans. Theirs is an almost two-dimensional realm, a special niche in nature for use of which certain requirements must be met.

When a substance attracts water molecules more strongly than water molecules attract each other, the water wets the surface. The liquid creeps along, invading every crevice, clinging tightly to each irregularity. But some materials, such as waxes and oils, attract water molecules so little that the water draws away, pulling back into itself and leaving the surfaces dry. Aquatic birds take advantage of this principle by regularly adding oil to their outer plumage, thereby keeping their feathers from becoming water-soaked. The many creatures that walk on water do so by means of well-waxed, hair-booted feet which the water cannot wet.

Best known of all the animals that walk dry-shod on ponds and streams are the water striders—bugs with four long legs stretching out to the sides and a shorter pair held under the head. Their slender feet are covered with greasy hairs which the water fails to penetrate. Each foot presses the water surface and makes a dimple in it, but the water does not run around and let the foot fall through the surface film as it would if the waxy hairs were absent. Instead the insect's weight is supported partly by the buoyant force of the water displaced from the dimples and partly by the surface tension, which tends to erase the depressions and bring all the water film to the same level. The strider uses chiefly its hind- and foremost legs to hold its body well above the smooth and slippery surface of the pond, while working the middle pair as oars to scull itself along. Mirrored in the water film below the bug is its image—a reflected "double" seldom seen except by small creatures close to the water surface. Below the strider, on the bottom, are dark shadows cast not only by the insect, but also by the dimples in the surface film where its feet press downward. Sometimes, on sunny days, these shadows on a sandy bottom are more conspicuous than the insects themselves. They drift along and follow every movement of the rowing striders on the film above.

A considerable *length* of surface must be called upon to support an insect as heavy as a full-grown water strider. If its hair-booted feet pressed on the film at only six small points, the bug would penetrate into the water and sink at once. But the strider's legs are spread so widely that its feet make elongated dimples or furrows in the water film. So secure is the insect on a quiet pond or stream that it can shift its weight freely among its feet. Most spectacular are the demonstrations of this when a strider cleans itself.

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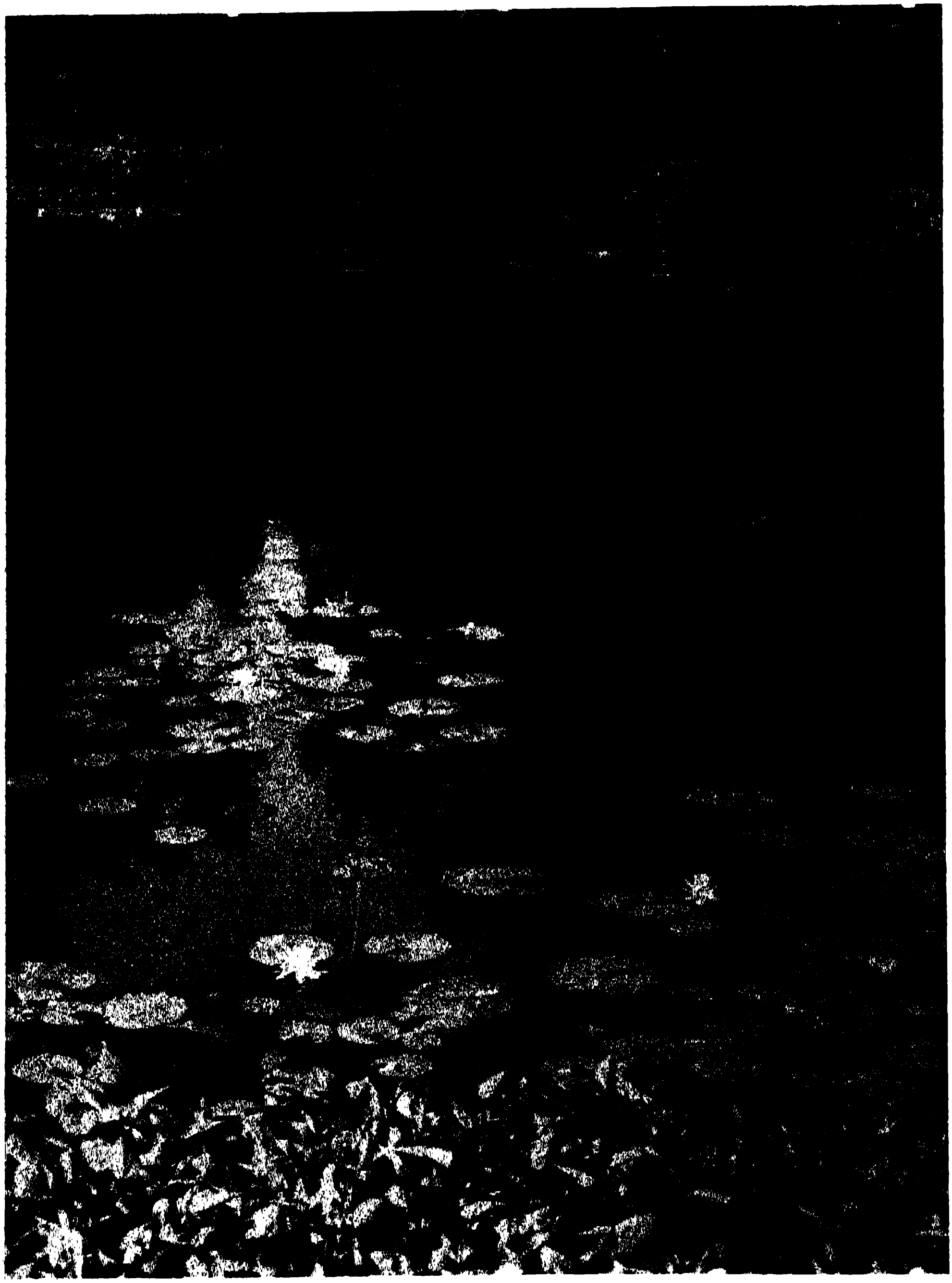
* Both text and illustrations are from *A Multitude of Living Things* (New York: Dodd, Mead, 1947. Chap. 11), copyright by Lorus J. Milne and Margery J. Milne.



A QUIET POND

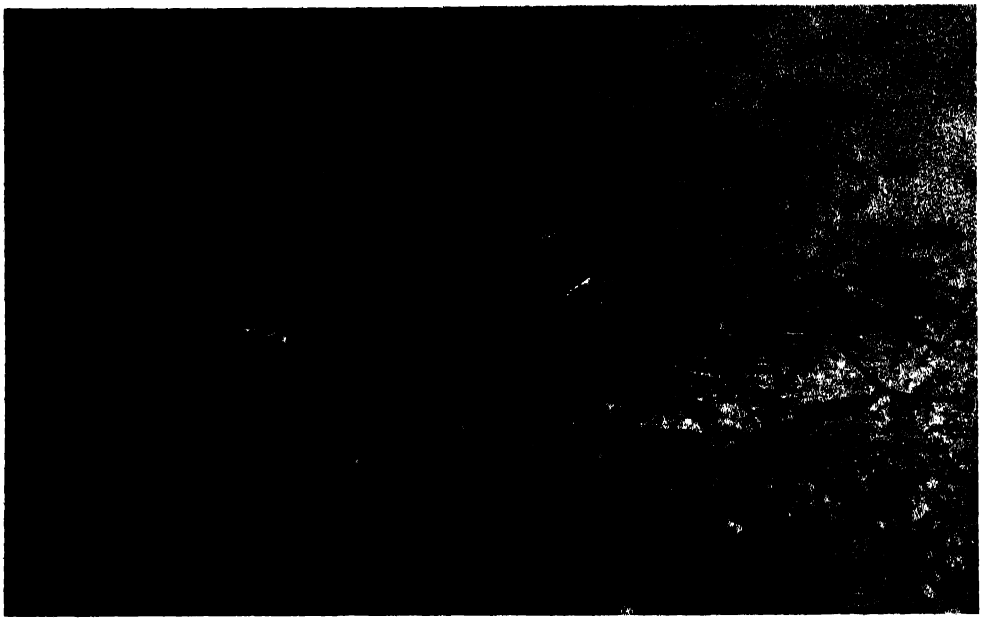
HERE THE NATURALIST HAS AN EXCELLENT OPPORTUNITY TO STUDY THE LIFE OF THE WATER FILM.

A WATER STRIDER ON THE SURFACE OF A POND

THE INSECT ROWS DRY-SHOD ON THE WATER. DEPRESSIONS IN THE FILM APPEAR UNDER THE BUG'S FEET.

Drawing its rowing legs far back, it stands with its head almost in the water, while its hind legs are raised well above the surface and rubbed one against the other much as the housefly does. Then the insect rests on forefeet and one hind foot, with the rowing leg on that side as an outrigger, while the middle and rear feet of the opposite side are raised into the air and rubbed free of clinging particles by a similar fiddling movement. To accomplish this contortion, the bug practically lies down on its side. The water film stands the strain, but the shadows cast on the bottom shift and spread as the pressures on the fewer surface furrows are increased. Finally, the strider stands on rowing feet and rear pontoons while its body and forelegs are raised high above the water. The insect washes itself much as a kitten does, transferring dust particles from feelers, beak, and body to the forefeet, then rubbing these together until they are satisfactorily clean. The bug seems to give great care to every detail, and if uninterrupted, such a complete toilet operation may take ten minutes.

Other animals that spread their weight on outstretched feet can rest with safety on the water film. Small gnats and midges flit from place to place on ponds, alighting with equal equanimity on film or foliage. Even large crane flies settle with surprising grace upon the water surface, and rise again with their long legs trailing behind. Each foot combines the advantages of waxed hair covering and slender length which can distribute the insect's weight along the surface film. Sometimes the water striders catch small flies that fail to take flight in time. Occasionally, too, a crane fly dies of unknown causes while resting on the water surface. Without muscle action to hold its body well above the water, the insect sags into the pond and sinks in or barely stays afloat. Water striders gather around it to salvage such nourishing juices as remain. It is but one of the many types of food the striders seek as they push their way along the transparent but rubbery surface film. Some of their sustenance floats up to them from below; each dead fish attracts a crowd of striders. But most of the food of these insects falls into the water from the air above. Ants tumble from leaves overhanging the water. Beetles close their wings and drop or blunder into ponds and streams, where the water wets them and renders them helpless prey to the predaceous bugs. The striders investigate every particle, often making great leaps over the water to reach some newly fallen object. Small particles such as drowning ants are picked out



TWO STRIDERS AND THE CAST SKIN OF A *DOLOMEDES* SPIDER

ONE WATER STRIDER CARRIES AN ANT IMPALED ON ITS BEAK WHILE REMOVING THE NOURISHING JUICES.

of the surface film and held on a slender black beak while the life juices are drained away. Often a strider is seen carrying with it a gnat or other carcass as it glides along the water surface. Occasionally other striders chase the food-bearing relative across the pond, just as chickens pursue a hen fortunate enough to have found a large grub.

On ponds and streams the striders stay close to shore or hurry to reach it if a breeze springs up and the water's surface becomes ruffled. In rain and in winter, the striders leave the water and crawl out upon the bank. In spite of these precautions, the insects do get wet at times. Although they show great ability in navigating streams, and can spring ahead to make progress against the current, an occasional bug is swept through a riffle and fails to stay afloat. In such situations, striders may be found below the surface film, rowing to shore where they can crawl out again to dry and clean themselves.

There is also a seagoing water strider—a small gray form common in tropical and subtropical lagoons and mangrove swamps, where it congregates in large groups. These same water striders are found at great distances from land, riding the waves like the best sailors. No one knows what they do during a storm at sea or when it rains. They must get wet, and what is there to crawl out upon to dry? To add to the problem, these seagoing striders often crawl down into the water during calm weather, and row along to feed there, upside down, on the underside of the water film. Many of them live out their lives hundreds of miles from shore and raise their families at sea. The eggs are laid on seaweed at the surface of the ocean or on the infrequent feathers dropped by sea gulls.

Spiders and mites of several kinds frequent the water film in pursuit of the insects there. They have the same means of staying dry as do the striders, and they scamper about on ponds picking up food wherever they can find it. Most of the water spiders are tan with dark stripes. Many of the females lay their eggs in a creamy sphere of silk, and drag this precious ball after them wherever they go, even out upon the water film. One of these ball-making spiders is a giant called *Dolomedes*. A full-grown mother may measure two inches or more between the tips of outstretched legs; her egg sphere may be half an inch in diameter and contain hundreds of potential spiders. Even after they hatch, the spiderlets stay with their parent, and the adult is often seen with a fuzzy covering that can scurry off, like goslings from a mother goose. Such a family group is quite a prize for a hungry fish, and those spiders that hesitate while running on the water film may lose their lives. However, spiders seldom stop on the surface; they run from shore to plant or from leaf to lily pad, carrying their prey with them to a safe spot. But fish in ponds and streams follow walkers on the water to profit from their occasional unwariness. Sometimes a fish makes a mistake and seizes a spider's ghost—the empty, castoff skin. Often these skins float downstream, casting on the bottom a shadow much like that of the spider itself. But the skin rests on the water like a dead crane fly, while the living spider walks well above the surface with only its eight feet furrowing the film and making sharp silhouettes on the sand below.

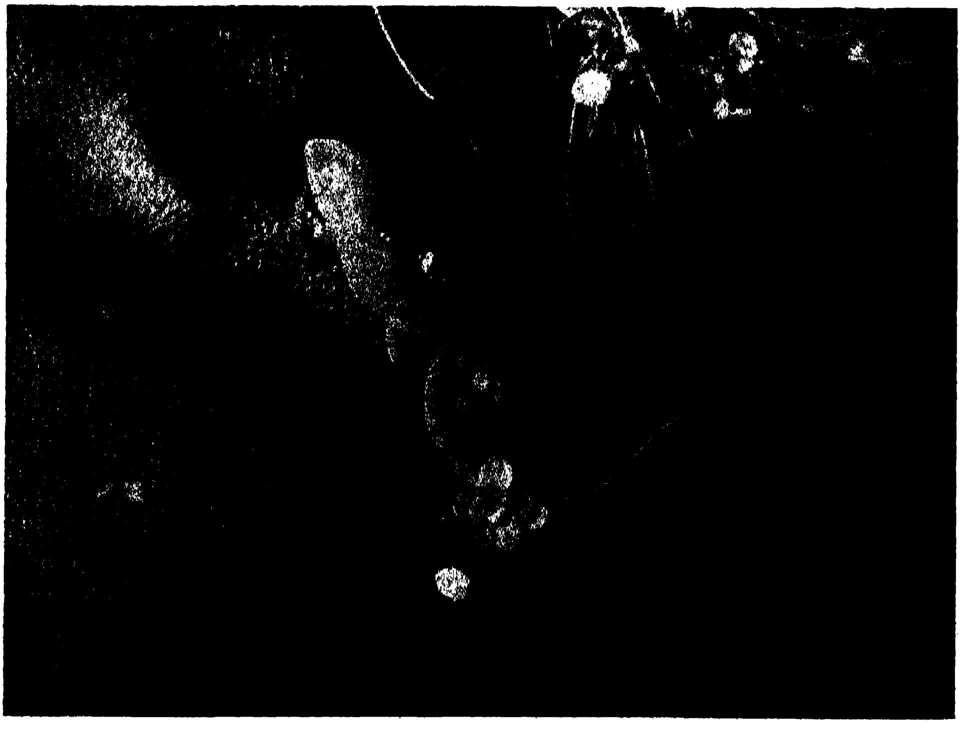
Most conspicuous of the water mites is a common one with a ball-like body of brilliant, velvety red. The full-grown mites reach a diameter of a quarter of an inch. They run so smoothly on their very short legs that they seem to glide over the surface film rather than move on distinct feet. The females leave solitary brown eggs on floating vegetation, to hatch into immature mites with six legs instead of the characteristic eight. These larval mites spend a few weeks as parasites on some insect. They lie in wait for water striders or diving beetles—anything that comes their way. Sometimes they ride on damsel flies; more often they catch the striders. One strider may carry several of these clinging mites, each sucking nourishment, yet seeming to do little harm to its host. Eventually they drop off, molt to gain another pair of legs and the spherical form of the adult body, and forage for themselves as their parents do. The mites not only run along the surface, but frequently climb down plant stems into the water and swim about. Their eight short legs give them an even motion by which they may be distinguished easily from all other aquatic animals.

Very small insects with waxy feet can stand upon the water film without the additional precaution of spreading their legs widely. The smallest mites have this advantage. So do the several kinds of springtails and the many leaf hoppers that jump over the surface. These animals are so very light that even when they press down sharply on the water film to throw themselves into the air and escape some danger, they do not produce any sizable dimple in the surface. The leaf hoppers have leaping legs like a locust's or a katydid's, but the springtails have a much more ingenious way of catapulting themselves into the air. They are grotesque insects, with an extended tail. Some merely keep this tail curved under them, almost resting on the water film between their six short legs. To jump they simply straighten out, but do so suddenly. Others carry the tip of the underturned tail in a special catch, like the notched trigger of a mousetrap. The tail is strained against the catch just as is the mousetrap spring. When the insect is frightened (or sometimes seemingly just for fun), the catch is slipped, the tail whacks the water film, and the springtail is thrown high into the air, to land somewhere else. For a creature so minute, air has an excellent cushioning effect, so that the springtail settles without damage, usually on its feet.

Two types of springtails are common on fresh water and one on quiet bays of the ocean. The more abundant of the lake and pond forms is bluish black, about an eighth of an inch long. It congregates in such enormous numbers as to appear as a conspicuous blue-black band along the water's edge. The individuals walk about among their fellows, but at the slightest disturbance, the group flings itself into the air like tiny corn kernels popping on a hot griddle. They alight many inches away, no longer in association with one another. To all intents and purposes, they have vanished. Like other springtails, these have a tubular extension from the underside near the catch for the springing organ. With this "ventral tube" they are able to hold themselves to the water surface. The tube can be wet by the water and forms a sort of anchor for the insect. The seagoing springtail and the seagoing strider are among the relatively few insects that are not bound to a land or fresh-water existence. The seagoing springtail is found all over the world along sandy and muddy coasts and on tidal pools in rocky shorelines. No one knows what happens to them during storms and showers at sea. Not all of them can come ashore for such occasions.

THE underside of the water surface is also used by a surprising number of aquatic creatures. Every now and again a pond snail crawls up a plant stem as far as the water surface, there to roll over and glide out under the water film, its flat foot pulsing with slow waves of movement from aft to fore along its length. In this position, many of the snails apply to the water surface a part of the body between foot and shell, and there open up the single hole that leads into the lung, so they can breathe in a load of air to take below. In very shallow water, a flat sole similar to that of the pond snail, but much smaller, shorter, and narrower, turns out to belong to a worm that is all sole, with almost no thickness. This flatworm, a free-living scavenger related to the liver flukes and tapeworms, is commonly called a "planarian" and seemingly has crossed eyes spotted on its speckled back. It is a source of never-ending delight to all biology students, and a laboratory pet with a firm grip on life. The animal is so elementally constructed that pieces cut from a single planarian can reorganize to form a whole. Biologists have worried out the philosophies of "self" in terms of many-headed, several-tailed planarians, which creep along the sides and water surface in laboratory jars to mock their captors.

Another animal capable of remarkable regeneration is *Hydra,* named two centuries ago by a man who, discovering that it could multiply heads if mutilated, thought of



POND SNAILS IN A SHALLOW PUDDLE

THE ONE WITH THE BLACK-SPOTTED SHELL IS ON THE BOTTOM ; THE OTHER CRAWLS UNDER THE SURFACE FILM.

the Greek mythical monster of that name. *Hydra* looks like a discarded umbrella without any cloth covering—merely a stalk with long arms from one end. The arms are tentacles with nettling cells for catching microscopic life, and between the arms is an opening into the animal's interior through which the prey is thrust for digestion. The opposite end of the stalk is armed with a sticky disk, the stickiness of which is under the creature's control. Often *Hydra* reaches a few of its tentacles upward and attaches them to some plant stem, then lets go with the sticky disk, to somersault in slow motion and glue its body to a higher point. Repetition of this process or a gradual gliding of the sticky disk may bring the animal almost to the water's surface. There *Hydra* often hangs, foot stuck to the underside of the water film, body pendant, tentacles outstretched for an inch or two beyond, waiting for unwary water animals to bump into its battery of stinging cells. *Hydra's* weight upon the water surface forms a dimple there, but the depression is not like the furrow under the water strider's foot. It is more like that around the snail or flatworm, and *Hydra* can creep along the water as they do, although with no visible waves of movement. These submerged creatures produce a water-repelling material from the flat area applied to the surface film. The water draws away, clinging wetly only to the rim of *Hydra's* disk or of the soles of snail or worm.

Two types of minute crustaceans upon which *Hydra* feeds have odd relationships to the underside of the water film. One of these, *Scapholeberis,* by name, has special waxy bristles with which to puncture the water film from below and lay hold upon it. Since these bristles are on the underside of the crustacean, the creature rests back downward, supported by the film. For purposes of camouflage its body coloring is related obviously to its upside-down position, for instead of being dark-backed and light-bellied like fish and most other animals, *Scapholeberis* is the opposite. In this position, the crustacean rows itself about with its long antennae, browsing on algae that float upward from below, and upon pollen and other flotsam accumulated on the water surface from the air above. A gust of wind tows the surface water and *Scapholeberis* attached to it—a "sort of submarine sailing," some have aptly called it.

A MOSQUITO LARVA

THIS WRIGGLER HANGS FROM THE SURFACE FILM BY ITS BREATHING TUBE, WHICH HAS A WHORL OF WAXY HAIRS AROUND THE ORIFICE OF THE TUBE.

The other type of crustacean is typified by *Bosmina,* a tiny creature often trapped by accidentally breaking through the water film as it swims along below the surface. Unless a wave or similar disturbance knocks the helpless creature below the film again, it must wait in this position—partly in and partly out of the water—until it can molt its skin and slip out of the old covering into the lake below. The difficulties encountered when a small underwater animal is caught by the "dry" surface of water are similar to those experienced by animals of similar bulk and strength when they fall into the water from the air. Unless a branch is near by upon which they can crawl out, they usually drown or are picked up by water striders and other carnivorous creatures that make this two-dimensional world their home. Even water striders have difficulties; readers of Frank E. Lutz' field book are cautioned to carry home their striders in a dry pail, not in water, lest they drown.

The many insects that live in ponds and streams must have atmospheric air to breathe, and remarkable provisions have been made for reaching through the surface film.

Back swimmers, mosquito wrigglers, diving beetles, and other water beetles come to the surface from time to time to thrust through the water film some tubular mechanism in order to replace the air stored beneath their wings or in their breathing tubes. Only by such frequent restocking can they carry on their precarious submarine existence. Some of the fly young, shaped like maggots and other peculiar things, have telescoping segments at their hinder ends, which they can extend to and through the surface film for gathering air continually while the creatures burrow busily to find food in the mire. Some of the water-beetle larvae not only come up to get their air but also drag living or freshly killed prey to the surface and thrust it out into the air, where it can give less resistance to being swallowed and where gravity can be of more help.

One of the water beetles has a greasy back, which repels all moisture like the feathers of a duck. This is the whirligig beetle, which passes much of its life at the surface of ponds and streams. Actually, it is a double animal—dry above and wet below—with paddlelike feet to propel it rapidly through the water. Even its eyes are divided into an upper portion for vision into the air and a lower part with which to watch the water's depths. These beetles are very vigorous swimmers, familiar to most people as they zig and zag along the water surface, commonly in groups, leaving behind them little Vs of waves like tiny speedboats.

The water film forms a definite barrier for insects that must lay their eggs in the water itself, and an almost endless variety of solutions to the problem can be observed. Perhaps the simplest is that of the water-lily leaf beetle, which cuts a small, circular hole in the dry top of its lily pad, pushes its abdominal tip through the hole into the water below, and while standing high and dry on a familar surface, lays two rows of eggs on the undersurface of the lily leaf in close concentric arcs. Many dragonflies, caddis flies, May flies, and others extrude a single egg or a group of eggs from the abdominal tip while soaring over the water. They fly down close and suddenly flick the abdomen through the surface film and liberate the egg. Pulling quickly out, they zoom away to repeat the process when the next egg is ready. The insect's momentum ensures it against being caught in the surface film and dragged in to drown. Much more careful are the ordinary biting mosquitoes, which literally lay a raft of eggs. The raft floats upon the water film with only its lower surface wet. The eggs hatch through their lower ends and the young wrigglers emerge into the water directly, many to be eaten at once by hungry beetle larvae and fish. The malaria mosquito, in contrast, lays her eggs singly on the surface of the water.

Another little fly, the *Dixa* midge, stands on the surface like a water strider while she deposits an extruded wet mass of eggs suspended by a strand of gelatine. As the egg mass is let down into the water, the fly adds to the supporting filament a circular, transparent disk that repels water. The disk catches on the surface film and pulls down a dimple as the strand below lengthens out and the suspended eggs sink farther into the water. The fly leaves, but the eggs with their little float drift around as the water surface is blown or as currents move the water itself. The eggs may become stuck to some vegetation or break their mooring and sink to the bottom, there to hatch.

Some other insects, when ready to lay their eggs in the water, wrap their wings around their bodies like a cloak, enclosing a bubble of air, and then crawl down stones or stems through the surface film and into the depths below. Those that succeed in laying their eggs and escaping capture in the water may later crawl back out into the air, dry off, and fly away. Male and female damsel flies coöperate in this. The male uses a pair of claspers at the end of his long abdomen to hold the female by her slender neck. After her eggs are fertilized and seem ready for laying, the pair alight at the water's edge and the female backs into the water down some stem. The male holds on to her and remains above the surface at least as far as his wings. When the eggs have been deposited, the female starts upward and the male pulls, fluttering his wings, so that between their combined efforts, the female is brought out of the water again, to dry off and fly away.

THESE many special abilities and difficulties related to the water film are based on the very high surface tension so characteristic of this commonest liquid. Each adaptation in form or behavior is a means of using or of circumventing the strong surface forces involved. None of these methods do anything to change the surface tension itself. Yet this is possible, and is the basis of a familiar parlor trick. If a chip of gum camphor is dropped into a dish of water, the chip spins around and sails in erratic courses, propelled by a mysterious force. Actually the camphor is dissolving faster along some parts of the water line than in others, and since camphor greatly reduces the surface tension of water, the surface forces are weakest where the gum is dissolving most rapidly. The chip moves because of the inequality of surface forces pulling it from all sides. The weakest forces are behind the moving chip and show where the camphor is dissolving fastest.

One of the rove beetles makes use of this trick. Like its relatives, *Stenus* is an active little beetle, running or flying around much of its time in search of carrion or prey small enough to overcome and eat. Sometimes *Stenus* falls into a puddle or a pond. It has no waxed hair to keep its feet dry and therefore sinks well into the surface film, its legs and underparts thoroughly wetted by the water. But *Stenus* merely expels from its anal glands a substance that makes the water wetter—reducing the surface tension at its posterior end. Undiminished surface forces in front of the beetle promptly draw it forward. As long as the insect continues to emit this magic substance, it sails along with no apparent effort. Often the beetle reaches some dry object upon which it can crawl to dry off and again take to flight. This rove beetle can keep up its speedboating for many minutes, but if it is deprived of the abdominal tip with the anal glands, the insect is quite helpless in the water.

The leaves of some of our pond plants are like the whirligig beetles, with a water-shedding top and a lower surface that rests in, and is wet by, the water. Lily pads are of this sort, anchored to their loglike roots by long slender ropy stems. Their two surfaces support two different types of clinging life, one wet, one dry, while in between the water lily gathers up the sunshine energy to make it grow. The duckweeds too, small flakes of green, are always at the water surface. The larger kinds rest on the surface with rootlets dangling into the pond below. One of these duckweeds is our smallest flowering plant, floating freely just below the water film among the lily pads. Contrast with this the largest water leaf of the Victoria lily, whose six-foot disk with turned-up rim will float a human child of medium size, all safe and dry.

When spring comes to the lake or pond surrounded by pine-clad hills, the water film takes on a golden yellow cast with squandered pollen grains. The wind makes patterns of the driven dust, while the whirligigs cruise through it and raise a wake like tiny boats, rocking all the water bugs and giving rise to quiet lappings on the near-by shore. Spent and useless to the trees around, this pollen dies and sinks below. Throughout the years it may build up a layered record of the past. From just such fossil pollen we know many of the plants which lived and passed away but left no other mark. Pollen, wind, and water film combined to make a fossil trail of bygone trees.

NATIVE ROCKS AND MINERALS AS FERTILIZERS

W. D. KELLER*

Dr. Keller worked in professional nonmetallic and petroleum geology and as a ceramic technologist until he took his Ph.D. in geology at the University of Missouri in 1933, after which he became assistant professor of geology at that school. He taught at the U.S. Army University in Florence, following Italy's military collapse, and returned to Missouri as professor of geology.

THE need for the addition of inorganic (mineral) plant nutrients to soils is continually becoming more acute. The most obvious warning is the decline in productivity of soils impoverished by extractive farming, but other warnings, such as malnutrition of animals fed on plants growing on deficient soils, are appearing. Every farmer since the days of Columbus has recognized by shrinking crops when he has "mined out" his soil and has been ready to abandon the old place and stake out a new claim. We are now meeting ourselves coming back from the search for virgin soils and must give thought and energy toward conserving and maintaining the geological heritage that is in the soil.

In principle, the return is simple, but in actual practice some serious problems arise. The very magnitude of the soil requirements poses an astounding problem of the adequacy of their sources. Moreover, the mineral or chemical compositions of the materials available for addition and their relative solubilities become highly significant. The problem is both pedological and geological. In this article I shall consider the thesis that future replenishment of soil minerals will come advantageously and predominantly from relatively crude native rocks and rock-forming minerals that are available in sufficiently large quantities to meet our needs, and that selected native silicates, or blended silicates, phosphates, and carbonates will be added, as well as processed salts.

ANY of or all the mineral fertilizers needed by soils might be discussed with profit, but only one example, that of potassium, will be considered in any detail. Where, for instance, can we obtain sufficient potassium for all our present soil needs—and the continuing and mounting needs? The answer is not a simple wave of one hand toward the Texas–New Mexico evaporite basin and the other toward Stassfurt. Second, is a processed potassium salt (muriate, for instance) the logical, natural, and best form in which to add it, or should not the major addition of potassium be in the compounds similar to those from which the soils have evolved? Both these questions invite consideration from the geological point of view.

In 1944, a year of record potash production up to then, 652,260 short tons of K_2O were delivered for agricultural uses in the United States. This is a large and truly laudable production, but when spread over actual needs it dwarfs to almost microscopic proportions. At least half the tonnage was taken by only eight states (Florida, Georgia, Illinois, Maryland, North Carolina, Ohio, South Carolina, and Virginia); what can we expect the other forty to do about meeting their needs?

The Southern states need enormous amounts of potash for improved yields. A recent writer recommends for the development of good pasture land in Mississippi an initial application of 200 pounds of 50 percent muriate of potash per acre and a subsequent annual addition of 50 pounds. The entire production of potash for 1944 would have treated initially only about 13,000,000 acres, less than half Mississippi's area (probably about the area of its crop land), leaving none for the rest of the country.

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* The author is indebted to his colleagues in the Soils ond Geology Departments at the University of Missouri for their aid and close cooperation in the preparation of this article. Professors Albrecht and Graham, in particular, furnished data and helped repeatedly by constructive criticism.

Moreover, continued farming rapidly depletes everywhere the potash reserve our soils have inherited. For the year 1944 we took out of our soil in corn (grain) 454,880 tons of K_2O, 169,761 tons of K_2O in wheat, and 771,084 tons of K_2O in alfalfa.*

We hauled away in alfalfa more K_2O from our soil in 1944 than the entire potash fertilizer delivered! The combined corn and wheat grain, not counting the fodder and straw, took out almost an equal amount. Obviously, this exhaustive withdrawal from the soil, without replenishment, cannot continue long without very serious results.

Three possible means of replacement of soil potash reserves are apparent: (1) provide it as commercial fertilizer to supplement that weathered annually in the surface soil; (2) let nature bring it in with wind, flood water, glacial ice, or landslides; and (3) let weathering of the subsoil or subjacent parent rock make available virgin potassium-bearing minerals to a descending soil system.

Although the wind does move many tons of soil each year, it blows away, as well as adds to, any one soil plot, and the net result of gain from wind action, broadly viewed, is practically nil. Except for local stream flood plains and alluvial fans, the additions to soil by water, ice, and gravity are chiefly of academic interest. Item (2) of the above three can therefore be dismissed.

With reference to the release of potassium by the weathering of subjacent rock, the rate of this may be determined for average conditions with moderate accuracy. The K_2O content of an "average" limestone (composite analysis of 345 limestones) is 0.33 percent. If this limestone were to supply 106 pounds of K_2O for an acre of alfalfa, about 32,121 pounds of the limestone would need to weather per acre per year. Using a figure of 160 pounds for the average bulk weight of limestone per cubic foot, and an area of 43,560 square feet per acre, the 32,121 pounds of limestone would be equivalent to a thickness of 0.0046 foot over the acre; that is, the average limestone will need to weather to a depth of 0.0046 foot in one year, or 1 foot in 218 years, to replenish the soil in K_2O taken out, assuming that no K_2O is lost by erosion. But an estimate of actual chemical denudation (the way limestone weathers) in the United States, excluding the western Great Basin (arid) region, places the rate at only 1 foot in 23,984 years (and 1 foot in 30,000 years for the land of the earth).

Hence, for the average data taken for all aspects of this problem, in order for weathering of parent underlying limestone to replenish its "rich limestone-derived soil" in K_2O when alfalfa is grown thereon, weathering will need to be accelerated to *one hundred times* the present normal rate!† The additional loss of K_2O by soil erosion places additional demands on replenishment. Return of potash by barnyard manure from animals fed on the alfalfa will compensate for part of the potash extraction, but this return is far from being adequate to replace the amount removed.

The potassium contributed to the soil by the addition of agricultural limestone is very slight because that limestone is usually selected for purity and high content of soluble Ca (and Mg in dolomite). In passing, probably we should revise our practice and add limestone containing "impurities" rich in potash and/or phosphate minerals.

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* Data on production from A. C. Britton, statistician, U.S.D.A., Columbia, Mo.

3,203,310,000 bu. corn on 97,078,000 ac., av. 33 bu./ac.
1,072,177,000 bu. wheat on 59,095,000 ac., av. 18 bu./ac.
31,863,000 tons alfalfa on 14,548,000 ac., av. 2.2 tons/ac.
7.1 lbs. K_2O/25 bu. corn amount to 9.4 lbs./av. ac. corn.
3.8 lbs. K_2O/12 bu. wheat amount to 5.7 lbs./av. ac. wheat.
48.4 lbs. K_2O/ton alfalfa amount to 106 lbs./av. ac. alfalfa.

† Average limestone would have to weather 0.000408 foot in thickness annually to replace the K_2O taken out by corn.

$$\frac{9.4 \times 100}{.33 \times 43{,}560 \times 60} = .000408$$

It would have to weather 0.0002475 foot in thickness annually to replace the K_2O taken out by wheat.

$$\frac{5.7 \times 100}{.33 \times 43{,}560 \times 160} = .0002475$$

Chemical denudation is about .000417 ft./yr. (1/24,000).

Sandstones contain on an average 1.32 percent K_2O; an average shale contains 3.24 percent K_2O; and an average igneous rock of the United States, 3.23 percent K_2O. From the standpoint of composition, they should be somewhat better soil replenishers of K_2O than limestone, but experience from cropping soils overlying the noncarbonate rocks does not indicate any significant relief by natural weathering from shortage of potash reserves.

Potassium (and other fertilizer elements) may be added to soil by man following two procedures. One is the present conventional practice of adding, in a commercial fertilizer, a selected few moderately pure, relatively concentrated elements in salts of high solubility to supply quickly the mineral-derived substances (usually potassium, calcium, and phosphorus) most acutely needed. This may be thought of as dosing the ailing soil-patient with a quickly assimilable, concentrated, processed, stimulating food to revive its strength.

Another procedure to restore soil reserves might be termed the natural, long-term, geological scheme. Hereby would be added, in clay, silt, and fine-sand sizes, carefully chosen, and perhaps blended, mixtures of rocks and minerals that would weather and decompose rapidly in the soil from a geological viewpoint, but slowly compared to reactions in the laboratory; these would supply a long-lasting, full-diet, soil builder. The elements and plant nutrients liberated would be considerably more diverse than those of the more nearly pure, processed fertilizer, and would include those needed in minor quantities, such as Mn, Zn, Cu, Co, and B. The intention would be to fertilize as nearly as possible according to the principles followed by nature in soil building.

The use of the conventional commercial fertilizer puts emphasis on high concentration, quick availability, addition of minimum bulk, and deliberate selection of a few elements. Many well-known advantages accrue from this practice; but, over a long period, some disadvantages may result from the very features that have appeared beneficial for a short time. Long-continued use of the chloride salt (muriate of potash) will bring about increased concentration of the chloride ion in the soil, a factor that is well recognized as affecting adversely the germination of seeds. Moreover, general experience has not favored a chloride-rich soil. Then, too, the advantages of high solubility and quick availability of a commercial fertilizer make for short life and the necessity of repeated application.

Another serious defect of a highly soluble, concentrated fertilizer is the powerful mass-action effect that it exerts to overstock with a few elements the humus and clay colloids from which the plants accept their nutrients, thereby suppressing, or blotting out entirely (for practical purposes), the availability of other elements also sorely needed by plants for optimum growth. The rich solutions from the soluble fertilizer convert the "nutrient jobber" colloids into a relatively homogeneous, undiversified system, which forces onto the plants an excess of a few elements to the exclusion of others. By its overconcentration in some constituents such a fertilizer creates nutrient deficiencies in others—despite its purpose to correct deficiencies. Albrecht clearly pointed out the advantages of heterogeneity in the soil system when he wrote:

> These facts support the concept that the soil need not be a uniform medium as to degree of acidity or as to the distribution of all the essential plant nutrients. Rather, the soil may be a mixture representing a heterogeneous collection of foci of each of these in the mineral or rock forms weathering slowly while in contact with the acid clay. Plant growth may then represent the summation of root contacts with all these different centers of fertility as the roots move to and get from them all that is needed for maximum crop productivity.

His lucid description of probably normal plant growth and search for nutrients is definitely contraindicative of an artificial dosing of the colloids. Instead it describes a mechanism compatible with the nutrition-deriving experience of plants through their eras of evolution, the utilization of rock and mineral particles as ultimate sources of nutrient:

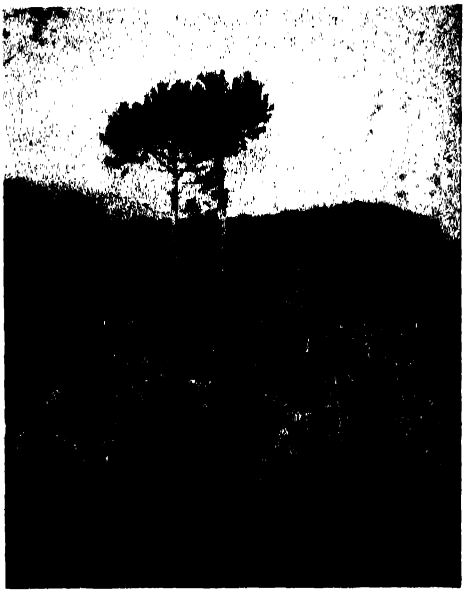
> It seems more reasonable to believe that the plant is growing much better if its advancing roots find one area around a limestone particle that may be

nearly neutral but providing much calcium, then at some distance another area more acid, where iron can be taken, then another where around some feldspar its potassium is slowly made available, then another where the acidity is mobilizing the phosphorus, and still others where manganese and the different essential nutrient elements are on the clay in exchangeable form because the clay is in contact with some mineral fragment and maintains itself in a weathering equilibrium with it.

The deleterious effects of calcium overdosage because of excessive additions of limestone that has been crushed too fine may be prevented by use of coarser particles, which dissolve and release calcium more slowly. Satisfactory release may also be had by applying natural calcium silicate or artificial metallurgical furnace slag, which decomposes more slowly and releases calcium accordingly. The advantages of the slow-acting calcium compounds point the way to similar advantages from other slow-acting native mineral fertilizers.

The need for the minor elements such as boron, zinc, copper, and manganese may not be supplied in the proper natural balance by man's too-narrow, selective, artificial replenishing. Plants have been evolving a long time by feeding on a diet wide in variety of elements available. Eventually we shall discover the proper concentrations of each element needed and synthesize the perfect fertilizer, but while these investigations are maturing, a "shotgun" application of the original natural food, the primitive rock, is logically indicated.

The primitive geological rocks and minerals, through which evolution of the plant world has progressed so far, most probably were composed of the silicates like those occurring in the igneous rocks available for quarrying today. In general, the igneous rocks contain a wider variety of minerals than do the sediments that are differentiates, or "purer and refined" concentrates, of quartz, carbonates, and clays (and the soluble constituents of the ocean). Consequently, the igneous rocks are to be looked to as the more nearly balanced and complete inorganic plant food, and as a source for a soil replenisher, and sedimentary minerals as special, more concentrated supplements.



GROWTH ON VOLCANIC SOIL

GRAPES AND PINES GROWING IN THE LOOSE CINDERS AND DUST ON THE SIDE OF VESUVIUS. VERY LITTLE WEATHERING HAD TAKEN PLACE IN THIS VOLCANIC MATERIAL. PHOTOGRAPHED BY THE WRITER IN 1945.

Proven practical testimony supporting the case for the efficacy of igneous rocks lies in (a) the rich soils rapidly converted from volcanic rocks—for example, at Vesuvius, in Mexico, some parts of the northwestern United States, Hawaii, and Java—and (b) the fully nutritional plant foods grown in our Western states on soils that contain a rich silt reserve of igneous rock minerals.

The quick conversion of Vesuvian basaltic volcanic dust and cinders to a singularly productive soil is classic. In Hawaii the basalt-derived soils, which still contain a reserve of basalt not completely decomposed, release potassium over an astoundingly long interval of extraction. The volcanic soils of Java supply food for a population density of over 900 per square mile, in contrast to the approximate density of 100 per square mile on near-by Borneo, whose soils are leached lateritic types with no recent volcanic activity.

The properties of these rich parent rocks that are significant to soil development are that the rocks are mafic (name derived from magnesium and ferrum) to intermediate in type, containing abundant calcium and significant potassium, rather than silicic (like those containing quartz or free silica); that they contain notable amounts of glass; and that they are fragmental to a greater or less extent.

Mafic rocks are readily oxidized (Fe″ to Fe‴) and hydrated and therefore are susceptible to rapid weathering. Where they are glassy, they are highly susceptible to hydrolysis; if fragmental, they will possess thin edges, angular shapes, and porous packing—all of which operate, with the mafic composition, to convert the rock quickly to an early and highly productive soil. Artificially crushed, glassy to fine-textured rocks, even though not naturally fragmental, should respond much the same way, and pedologists may well take the hint from Mother Nature and duplicate her work.

Laboratory experiments have shown that relatively pure silicate minerals hydrolyze when ground in distilled water. Graham found that silt-sized mineral fragments in contact with acid clay hydrolyze and release potassium, calcium, sodium, and magnesium. Simultaneously, hydrated aluminosilicates or, possibly, hydrated alumina and/or silica are freed. A new base-exchange medium is immediately established upon their breakdown. The plant food from the silicate minerals is delivered slowly, at a natural soil-forming rate, from *within* the soil body, and not as a deluge of outside intoxicant.

With reference to the rate of release, Graham showed that within 107 days, 3.4 percent of the total calcium of anorthite feldspar had been taken into solution. Those data extrapolated to 100 percent weathering indicate that the silt particles of anorthite would be completely altered by a highly acid soil within about 10 years. This figure is not to be taken too rigorously because the rate of weathering will be modified by the acidity of the soil, the extent of leaching by ground water, the action of plants removing Ca from the clay, the effect of CO_2 and humic acids, the temperature, other cations present, and diminishing size of the weathering silt particle. However, the 10-year figure is probably a good indicator of the order of magnitude of the length of time that anorthite silt can remain in, and contribute to, the soil. It indicates that this calcium feldspar is not exhausted in 1 year or that it would not last 100 years, and it is not out of line with our experience of calcium availability of soils under actual cropping conditions.

Graham's valuable pioneer work needs to be supplemented by working in the laboratory with a greater variety of minerals than he covered in his first experiments and by testing rocks that, as well as containing the minerals and elements desired, are available for quarrying and transporting economically to areas of soil need. Field tests of the response of impoverished acid soils to additions of selected crushed rocks and minerals may well be run along with laboratory work.

Pedologists and geologists should cooperate to test and supply the most readily available and practical raw materials within various regions. It is important that the mineralogy and petrography of the rocks to be tested should be considered. For example, potassium-bearing orthoclase and microcline feldspars in the Missouri red granite apparently are not as effective sources of potassium as is leucite, the potassium feldspathoid in Vesuvian lava, or the lava itself. Therefore, simple reference to the chemical composition of a rock does not furnish adequate information on its properties. In this respect, Ross and Hendricks point out, in a discussion of the Piedmont soils: "Here then is an excellent example where the course of weathering is determined by the mineral composition of the rock rather than by the overall chemical composition." Minerals of the olivine, pyroxene, and calcic plagioclase groups are most susceptible to weathering attack.

A choice of rocks and minerals that would most likely be best as mineral fertilizers will be a compromise between the ideal mineral composition, the ideal texture, availability, and adequate quantity. A list of such rocks, with their chemical compositions, occurring widely separated, is given in Table 1.

The rocks numbered 1, 4, and 5 are from regions whose highly productive soils were developed readily from the rocks; therefore,

TABLE 1

COMPOSITION (PERCENT) OF ROCKS POSSIBLY SUITABLE FOR USE AS FERTILIZERS

	1	2	3	4	5	6	7
SiO_2	48.10	52.23	44.40	49.73	55.06	46.04	49.69
TiO_2	1.41	2.27	1.53	3.05	0.36	0.64	0.85
Al_2O_3 ...	17.56	11.22	10.95	16.39	17.92	12.23	18.06
Fe_2O_3 ...	2.48	3.38	5.15	7.58	4.39	3.86	2.64
FeO	6.10	1.84	2.77	3.98	5.24	4.60	6.19
MnO	...	...	0.08	0.23	0.50	Tr.	0.13
MgO .	4.27	7.09	1.75	4.06	3.26	10.38	5.73
CaO	8.16	5.99	8.49	7.17	8.29	8.97	8.24
Na_2O ...	2.65	1.37	6.50	4.12	3.09	2.42	2.99
K_2O ..	7.93	9.81	8.14	1.93	2.01	5.77	3.90
H_2O+ .	0.12	....	1.17	0.54	0.07	....	....
		2.65	....	....	....	2.87	0.91
H_2O- . .	0.04	....	0.24	0.81	0.23	....	....
P_2O_5	1.01	1.89	0.37	0.84	....	1.14	0.81
ZrO_2 ..	Tr.	. .	0.03	0.03	....	...	....
BaO	0.08	..	0.01	0.03	....	0.48	....
SO_3	....	0.74	0.06	....	....	Tr.	...
F ...	....	0.50	....	..	...	...	....
Others .	...	1.68	0.12	....	...	....	...
Total	100.82	100.62	100.76	100.53	100.42	99.76	100.27

1. Vesuvian lava, 1903. ROSENBUSCH-OSANN. *Elements der Gesteinslehre.* Stuttgart: E. Schweizerbartsche.
2. Wyomingite, Leucite Hills, Wyo. SCHULTZ, A. R., and CROSS, WHITMAN. *U. S. Geol. Surv. Bull. 512,* 11.
3. Arkite, Magnet Cove, Ark. WASHINGTON, H. S. *J. Geol., 9,* 1901, 616.
4. Basalt, Mauna Kea, T. H. WASHINGTON, H. S. *U. S. Geol. Surv. Prof. Paper 99,* 1917, 522.
5. Pyroxene andesite, Java. WASHINGTON, H. S. *U. S. Geol. Surv. Prof. Paper 99,* 1917, 512.
6. Leucite basalt, Highwood Mountains, Mont. CLARKE, F. W. *U. S. Geol. Surv. Bull. 770,* 1924, 462.
7. Basalt, Table Mountain, Colo. *U.S.G.S. Bull. 770,* 460.

they are examples of proven desirability. The high K_2O content (7.93 percent) of No. 1 resides mainly in the feldspathoid mineral, leucite. A pyroxene mineral contributes Ca, Mg, and Fe oxides. No. 2, which occurs in Wyoming, also carries its K_2O (9.81 percent) dominantly in leucite, with pyroxene, amphibole, mica, and dense matrix material furnishing abundant Ca, Mg, and Fe oxides. It is logical to expect the rocks from the Wyoming Leucite Hills to respond in a soil much as the Vesuvian lava does. A large quantity, proven reserves, is exposed to view in Wyoming. Schultz and Cross computed in 1912 the following minimum:

Leucite-bearing rocks in tons	1,973,496,177
Potash (K_2O) in tons	197,349,617

These figures do not take into consideration the rock that occurs in the dikes associated with the volcanic necks and surface flows, nor the material underlying the mesas which has not been estimated in the discussion of the separate exposures.

Perhaps some of the demand for K_2O can be shifted from the Texas–New Mexico region to Wyoming.

Leucite-bearing basalt, No. 6, and other alkali-rich rocks occur in great quantities in the Highwood Mountains. Minerals composing No. 6 include leucite, pyroxene, olivine, biotite, analcite, and apatite. In the Bearpaw Mountains of Montana occurs pseudoleucite tinguaite, which contains 9.81 percent K_2O. Montana also possesses adequate reserves of potash-rich rocks.

In the Magnet Cove, Arkansas, region occurs arkite, No. 3 in the table, which is rich in potassium. It is composed of leucite, nepheline, orthoclase, pyroxene, and garnet. The analysis shows considerable Ca as well as determined amounts of Mn, Ba, Zr, and other minor elements. Other alkali-rich rocks occur in association with arkite.

Rocks No. 4 and No. 5 do not show particularly high K_2O content, but they weather to some of the most productive soils known. No doubt their occurrence in a climate favorable to a high rate of weathering is a significant factor, but still they serve to demonstrate that basaltic rocks do furnish the elements needed for plant nutrients. The basalt, No. 7, from Table Mountain, Colorado, compares favorably with those from Hawaii and Java. Basalts are exceedingly widespread in distribution, and, because they vary in mineral and chemical composition, they offer a number of possibilities for soil additions. Each individual basalt should be examined on its own merits.

Besides the primary rocks discussed, there are other rocks and minerals that are logical sources of potash. Glauconite is a sedimentary, complex, hydrated potassium iron silicate with proxying Na, Mg, and Al, which occurs in abundance in the green sand of New Jersey, in some Gulf Coast formations, and in certain older, lithified sediments such as the Bonneterre dolomite of Missouri. Green sand averaging about 6 percent in K_2O content could be produced from large reserves. Glauconitic dolomite, besides furnishing potassium, would supply also calcium and magnesium, and the quarrying and crushing of it would cost no more than for ordinary limestone.

A dolomite, the Waterton formation, which contains about 40 percent by volume of authigenic glassy orthoclase feldspar in grains 0.01–0.05 mm. in diameter, occurs in large quantity (at least 60 meters thick) in the Waterton Lakes region, Montana–Alberta. Daly reported the chemical and mineral analyses for it given in Table 2.

Although current work indicates that igneous orthoclase furnishes potassium very slowly to soil colloidal clay, the fact that the Waterton feldspar is authigenic in the dolomite reopens the possibility that it might be more responsive to weathering. What an attractive limestone it would be, *if* it proved to be effective.

TABLE 2

COMPOSITION OF A DOLOMITE (PERCENT)

SiO_2	30.46
Al_2O_3	6.86
Fe_2O_3	4.53
FeO	1.89
MgO	10.07
CaO	16.02
Na_2O	0.38
K_2O	5.77
H_2O	1.42
CO_2	22.55
	99.95
Sp. gr.	2.749

The estimated composition of the rock (assuming the iron oxides to represent magnetite) is, by weight:

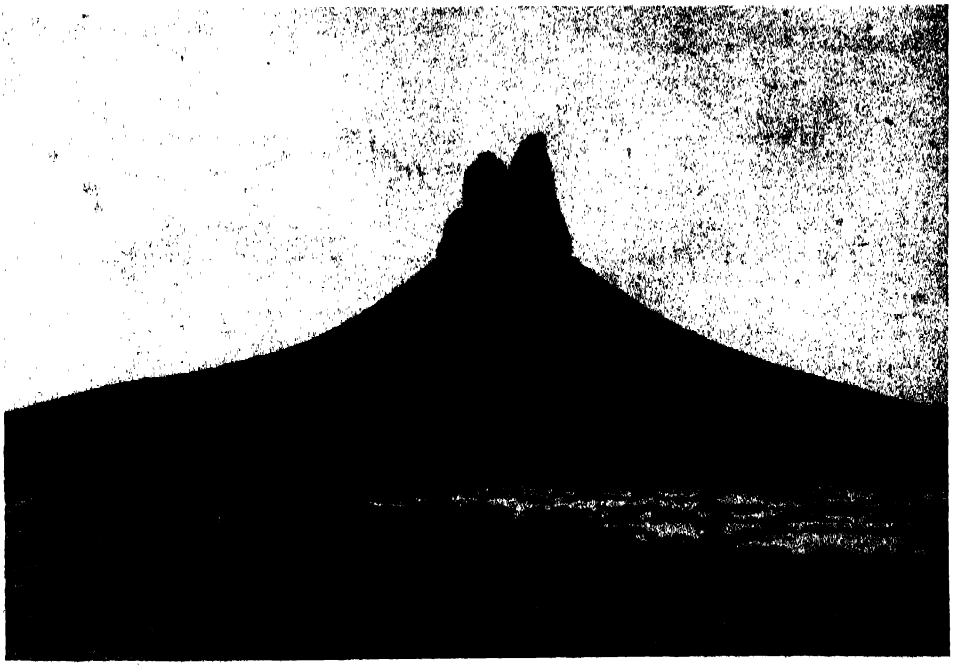
Orthoclase	34.5
Albite*	3.1
Quartz	6.0
Magnetite	6.3
$MgCO_3$	21.2
$CaCO_3$	28.6
	99.7

* The albite is probably in solid solution with the orthoclase. So far as known by published analysis, no other non-metamorphosed dolomite or limestone even approaches the Waterton dolomite in its abundance of the alkalies; feldspar makes up about 40 percent of its volume.

Alunite and alunitized rock, such as occur in concentrated deposits and in altered rock associated with epithermal ore deposits in Utah, California, Nevada, Colorado, Arizona, and elsewhere, can furnish up to a theoretical 11.4 percent K_2O. Alunite is a sulphate, $KAl_3(OH)_6(SO_4)_2$, the sulphate radical being of questionable desirability. Alunite is sparingly soluble in its natural occurrence, but its space lattice is broken down by heating to 700°C.–800°C., after which the potassium sulfate dissolves readily. In commercial practice, the alunite would probably be calcined at the mine, to save shipping the combined water, and crushed to desired size. Possibly calcination should be only partial in order to slow down the solubility of potash. Along this line, consideration should be given to calcining other earth products for the purpose of increasing the solubility of certain of their constituents. Heating of mixtures—for example, lime with a silicate—may improve the rates of availability of both. Although heat processing costs money, and the product will have to compete with other materials whose composition or location enhances their value, heating may still be profitable.

EMPHASIS has been directed toward the mineral and chemical compositions of rocks that are potential sources of mineral fertilizers, without much comment on the quantity of rocks available. One exception has been the tonnage occurring in the Leucite Hills. The volumes of rocks exposed in the Highwood Mountains, the Bearpaw Mountains, Magnet Cove, and basalts in Colorado and elsewhere are to be reckoned in terms of cubic miles rather than in tons. Concern is not about quantity of these rocks, but proximity and means of transportation to regions of need. The problem of supply is parallel to that presented by agricultural limestone today; it is practical availability, not scarcity of "ore." Long shipping distances may be feasible under some specialized conditions. For instance, if Vesuvian cinders were used as ship ballast, that material might be shipped on an economical basis as a by-product to Florida, where it could be spread on those thin, sandy soils. Whereas a highly soluble commercial fertilizer has to be applied perhaps three times a year, and is leached away from the sand, the weathering Vesuvian rock would leach very slowly. Instead, it would make available a concentration of nutrient elements normal to plant development, and would weather most probably to a clay of the montmorillonite-beidellite family having a high base-exchange value. The Vesuvian soil is productive at home in grapes and citrus fruits; Vesuvian cinders could be expected to perform equally well in furnishing a fairly complete range of nutrients to Florida citrus groves.

Whereas the reserves of the potassium "ores" of the New Mexico–Texas area are more definitely circumscribed in quantity and location, the use of crushed native rock as a potash replenisher opens wide many other possibilities. The continued demand for



THE BOAR'S TUSK

A VOLCANIC PLUG IN THE LEUCITE HILLS OF WYOMING. THE NAME APPEARS APPROPRIATE. WYOMINGITE OCCURS IN THE BOAR'S TUSK, BUT THE GREATER QUANTITY IS PRESENT IN THE LAVA FLOWS THAT TOP NEAR-BY MESAS.

highly soluble potassium salts is anticipated to remain so high that even with the use of silicate rocks our richest evaporite reserves will be depleted too soon.

Efficient use and development of native rocks for soil replenishment calls for close cooperation between soils specialists and geological surveys or institutions to classify rock deposits on bases of mineral and chemical composition and tonnage of rock that can be quarried, crushed, and transported economically. Compromises on these factors may provide the most mineral nutrients per dollar spent. Possibly a locally occurring, low-cost, but partly deficient rock may be advantageously blended with small quantities of another of higher cost to give a better-balanced mixture at lower cost than if only one rock were supplied. Individual regions with their particular needs and resources will require individual solutions. This discussion can only point out generalities.

Promptness, immediacy, or speed of response by impoverished soils to native rock additions will not be as early as to quickly soluble fertilizers. One or more years of weathering will elapse before the ions from the rocks will become available to the plants. Just as raw rock phosphate is slower in action than acid superphosphate, so will the silicate rocks be slow to get started.

A thorough addition of a balanced rock mixture should last several times as long as an equal application of quickly soluble fertilizer, and at no time would there be any effect of overdosage or overconcentration. A rock supplement lasting 10 years (utilizing Graham's data) would be cheap even if it cost, say, three times as much as a soluble fertilizer that had to be applied each year.

Decline of fertility as the rock fertilizer became used up would be gradual, much as our fertility is declining now, rather than an almost catastrophic slump such as occurred in certain European districts after war cut off their fertilizer supplies. Subsequent maintenance additions of rock fertilizer could be made at the physical and financial convenience of the farmer without an immediate press of necessity. The strategic value to our nation of longer-lasting fertilizer is obvious.

It may be as shocking financially as the appearance of new taxes to press the point and insist on hauling tons of rock onto the soil, but, when we face squarely and quantitatively the measure of soil fertility (mineral nutrients), we know that every pound of mineral matter hauled off the land in crops is extracted from the soil, and that it can be replaced in only two ways: from the subsoil or subjacent rock, or by being carried in. Either we extractively mine the soil, or we replace what we take out. If we maintain or restore our soil mineral heritage, we must haul back pounds of rock (soil mineral builders) for pounds of crops hauled away to urban districts. We cannot thrive on carbohydrates alone, plant-synthesized air, water, and sunshine, whether or not we like the idea of carrying rock back to the soil. The inescapable fact is that we will be forced to replenish soil minerals. Logically, geologically, and pedologically, and from the standpoint of quantitative adequacy of reserves, eventually the native rocks must be utilized to furnish most of our mineral fertilizers.

METABOLISM AND BIOENERGETICS

OLIVER P. PEARSON

Dr. Pearson, who completed this work at the Biological Laboratories at Harvard, is now with the Museum of Vertebrate Zoology, University of California, Berkeley.

THE most fundamental physiological property of all plants and animals, the rate at which they are living, is reflected quantitatively in their metabolic rates. Metabolism is a complex, inscrutable process—but its rate is easily measured. Heretofore, such measurements have been used chiefly by physiologists as tools for studying the functioning of organisms; I shall endeavor to show that they can also be used by ecologists and philosophers in understanding the energy balance of the earth, and that a person could even, through measurements of metabolic rates (and here I exaggerate only slightly), relate every beat of a mouse's heart to the entropy of the cosmos.

Consider the mouse represented in Figure 1. He takes in food, water, minerals, oxygen, nitrogen, and many other essential, helpful, or useless materials, but almost everything is returned to the environment, as shown in the segment of Figure 1 marked "Borrowed and Returned." Some substances, like the nitrogen of tidal air, are borrowed for only a moment; an indigestible seed might be borrowed for several hours, and a milligram of calcium for a fleeting lifetime, but all are returned to the environment to play a part in the lives of other animals or plants. The only permanent loss to the environment is the energy dissipated in the form of heat, represented in the third segment of Figure 1. The mouse has taken a bit of solar energy (captured and made edible by plants) and released it. Perhaps we should say the animal has squandered it, because the energy has been degraded to such an extent that it is beyond recall by the plant and animal kingdoms. It is this energy degradation that is measured as the metabolic rate.

As long ago as 1886, Boltzmann (quoted by Tizard, 1932) remarked that the struggle for existence is a struggle for free energy available for work. Therefore, to know the metabolism of an animal for a day is to know how successful it has been in the struggle for existence, how successfully it has deprived some rival of energy, how much of its organic heritage it has reduced to impotent isothermality.

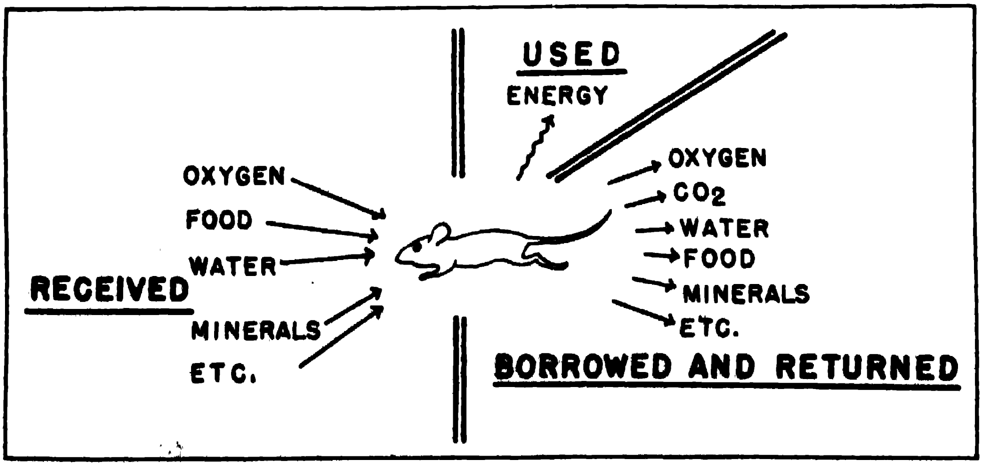


FIG. 1. INTAKE AND OUTPUT OF A MOUSE

Since Mother Nature's ability to keep her cupboard supplied is limited by the earth's meager quota of sunshine, since many animals and plants are making use of the cupboard quite freely, if not recklessly, and since I am somewhat fearful that this pilferage may become so rampant as to make it difficult for my friends and me to satisfy our needs, I have decided to expose some of the culprits.

The rate of living of many citizens has been measured and recorded in the papers. Consequently, I can calculate the extent of their thievery from the public energy supply and can state definitely that big, boisterous Smith, because of his flagrant metabolism, is Public Enemy No. 1, whereas little, mild Miss Jones is only a Lesser Public Enemy. But I can go further. I can group all the Smiths and Joneses together (for not one of them has a clear record) into a genus, *Homo,* and compare their thieving, their dissipation of this limited supply of energy, with that of a host of smaller public enemies—the mice, for example, or the earthworms.

Table 1 might be considered a rogues' gallery of the records of most of the common small mammals living in forests in eastern Pennsylvania. The animals are listed in order of their metabolic (or catabolic) importance. The metabolic values are taken from Pearson (1947) and are not basal rates, but are average daily rates taken over 24-hour periods at a temperature of about 25° C., with the animals free to move about and express their normal nocturnal-diurnal rhythms. The figures for population per acre are estimates based on personal trapping experience in the northeastern United States and on many recently published population studies.

TABLE 1

THE ENERGY DISSIPATION OF SMALL MAMMALS ON AN ACRE OF FOREST IN PENNSYLVANIA IN SUMMERTIME

	Ave. wt. (gm.)	No. per acre	Gm. per acre	METABOLIC RATE	
				Kg. Cal./kg./day	Kg. Cal./acre/day
Short-tailed shrew (*Blarina brevicauda*)	18.0	4.0	72.0	600	43.2
Deer mouse (*Peromyscus leucopus*)	20.0	5.0	100.0	416	41.6
Red-backed mouse (*Clethrionomys gapperi*)	22.0	0.5	11.0	414	4.5
Woodland jumping mouse (*Napaeozapus insignis*)	24.0	0.5	12.0	360	4.3
Flying squirrel (*Glaucomys volans*)	70.0	0.1	7.0	231	1.6
Long-tailed shrew (*Sorex cinereus*)	3.5	0.2	0.7	1,800	1.3
Total		10.3	202.7		96.5

It will be seen that short-tailed shrews and deer mice are metabolically the most important species. Deer mice are more abundant than shrews in the forest, but since they have a lower metabolic rate they are no more important. The long-tailed shrews, despite their astonishingly fast rate of metabolism, have little effect on the total energy exchange of the area because they are so small and so few. The total impact of all these small mammals from the Pennsylvania acre considered in Table 1, all 203 grams of them, is reflected in a metabolic rate of 96 kg. Cal. per acre per day.

This metabolism may be compared with that of other kinds of animals living in Pennsylvania (Table 2). Deer, for example, which are unusually abundant in the state, dissipate the energy in vegetation at a rate of 70 kg. Cal./acre/day—about three-quarters as fast as all the small mammals listed. This calculation is based on the following assumptions: 1,100,000 deer in the state (Biological Surveys Inventory, 1943), average weight of 100 pounds, and an average daily metabolic rate of 40 kg. Cal./kg./day (twice the basal metabolism of goats).

To introduce man into Table 2, I have used the density of Indians rather than that of modern man because Indians lived in closer harmony and more exact balance with their environment than does man today. In the year 1600 there was about 0.00054 Indian per acre in New England, New York, Pennsylvania, and New Jersey (calculated from data given by Mooney, 1928). At a daily metabolic rate of 2,500 kg. Cal. per person, their total metabolism would have amounted to only 1.4 kg. Cal./acre/day—about the same as that of the smallest mammal of the region, the long-tailed shrew. Modern man, however, is living in Pennsylvania at a density of 0.3 persons per acre, which results in a metabolism of 750 kg. Cal./acre/day—nearly seven times greater than that of all the small mammals listed in Table 1, and 535 times greater than that of the Indians. Even when we consider a less-urbanized state, such as New Hampshire, with a present population density of 0.09 per acre, the energy dissipated by man is about 225 kg. Cal./acre/day, which is nearly double that of all the smaller mammals put together.

TABLE 2

Disposition of the Energy Falling on an Acre in Pennsylvania in Summertime[*]

	No. per acre	Gm. per acre	Metabolism	
			(Kg. Cal./acre/day)	
Used by man (Indians)	0.00054	25	1.4	
Used by deer	0.04	1,700	70.0	
Used by small mammals	10.3	203	96.5	
Used by earthworms	125,000.0	25,400	205.0	
Used by microorganisms	3×10^{18}(?)	3×10^{6}(?)	2,700.0	
Total used by above animals			3,072.9	3,073
Energy still available to other organisms				12,927

[*] Rate of incident radiation, 25,000,000 kg. Cal./day; rate of capture by plants (net productivity), 16,000.

Actually, man is an even more expensive item in the energy budget than these figures indicate, for he has been able to supplement his own metabolism by putting fire to work. I shall not attempt to calculate exactly how much of this energy is totally wasted and how much is "saved" by charging batteries, heating houses to lower man's metabolism, etc., but let us suppose that each person finds it necessary to burn one ton of carbon each year, and that all the resulting energy "goes up the chimney." All this supplementary oxidation would drain 1,765 kg. Cal./acre/day from New Hampshire and 5,980 from Pennsylvania. With this outside help, man's impact on the environment, his bioenergetic importance, becomes much greater than that of any other mammalian species—probably greater than that of all other mammals combined. For many years he has been drawing on the earth's capital to support this high living; most of the other animals live frugally within the earth's income.

Having found that modern man is the greatest energy squanderer among the mammals, let us see if he has any rivals among other groups. We are faced at once with a shortage of population and metabolism data, but we can make some rough estimates. Using the method described by Eaton and Chandler (1942), I have estimated that there were 125,000 earthworms (25,400 gm.) to an acre of hardwood forest at Swarthmore, Pennsylvania, on June 10, 1947. At 19° C. these would use oxygen at a rate of 0.07 cc./gm./hr. (Loewy, 1926), or 1,778 cc./acre

/hr. At an equivalence of 4.8 kg. Cal. per liter of oxygen, this would be 205 kg. Cal. /acre/day. The worms on this acre, therefore, would be about twice as important metabolically as the mice and shrews, but less important than "barehanded" modern man in Pennsylvania.

If we go to the bottom of the size scale and consider the microorganisms in the soil (mostly bacteria), we find that on an acre such as the one postulated in Table 2 they are the most important of the groups listed. Metabolically, they are more than ten times as important as the earthworms. (I have used Russell's figures [1937, p. 459] for the energy dissipation of microorganisms on unfertilized land.)

We must now compare the rate at which these organisms are depleting the supply in Nature's cupboard with the rate at which it is being stocked—that is, the rate at which plants are capturing and storing our quota of energy. The energy serving to warm the earth and atmosphere is not being considered here, for although an unknown amount of it is "conserved" by lowering the metabolism of birds and mammals, at the same time it increases the metabolism of the cold-blooded animals. During Indian times in Pennsylvania, plants captured about 16,000 kg. Cal./acre/day of the 25,000,000 kg. Cal. that bathed each acre daily. I have assumed that all the land was forested and had an annual net productivity of 200 tons of organic carbon per square kilometer (Noddack, 1937). In Table 2 this income is compared with the expenditures of the few groups of animals that I have mentioned. It may be seen that these animals dissipate 3,073 of the 16,000 calories made available each day, leaving some 12,000 available for the birds, bees, toadstools, and a host of others, or for storage. This storage is temporary, however. More energy is captured each day during the summer than is used, resulting in the storage of some, but almost all seems to be used up over the winter. Geologists support this belief that there is little energy being stored in forests today, and Russell (1937) states that the soil microorganisms live right up to their income in the matter of nutrients and energy supply.

In the preceding pages I have only outlined an approach that should yield interesting results when it is applied to different kinds of terrain at different seasons with *all* the animals and plants carefully accounted for. I have not undertaken this task, for, finding unexpectedly that my friends and I with our cars and furnaces and washing machines are by far the greatest of the public enemies, I have had a twinge of conscience, a resurgence of altruism, and am preparing to retire to a life of studied inactivity and heat conservation under the whispering leaves of the nearest palm tree.

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FOR NOW AND THE FUTURE

ROSS T. McINTIRE

Admiral McIntire, wartime Navy Surgeon General and White House physician for President Roosevelt, was appointed director of the National Blood Program on July 20, 1947. A native of Salem, Oregon, Dr. McIntire attended the University of Oregon and received his M.D. from Willamette University in 1912. He entered the Navy in 1917.

THE turn of the new year marked the beginning of one of the greatest and most far-reaching undertakings of this century in saving human life. The new National Blood Program of the American Red Cross is under way—an adjunct to medical science and a step forward in national preparedness.

The wastage of human lives in war—and in peace—is so obvious that it requires little comment. However, when we find that war has developed new lifesaving methods, these are worthy of further study for the use of mankind during the times of peace.

Blood might not be considered a new lifesaving agent. In a way this is true, but in World War II we learned how to use whole blood in entirely new ways. The men in the field used daring methods in their efforts to save lives. At Tarawa, where so many of our marines lost their lives, hundreds owe theirs to the use of plasma, serum albumin, and whole blood. Four-way transfusions, giving plasma and whole blood simultaneously through both arms and legs to men who had lost a major part of their own blood, were performed with success. Those men are alive today.

The American people gave freely of their blood so that our fighting men might have a better chance of recovery, regardless of their injuries. In the Navy and Marine Corps, the record of lifesaving showed that only 2.37 percent of the wounded failed to survive. We can give a great deal of credit for this extremely low mortality to the blood and its derivatives that were supplied in abundance to our armed services all over the world.

New medical findings are being reported constantly. Today, one of the broad fields of research is in human blood. What is it? What are its functions? What makes it work? The latter two basic question have been answered, oddly enough, more fully than the first one. We know that blood carries food to the tissues, acts as the transportation system for waste products, and circulates the hormones, enzymes, and many chemically active substances needed to regulate body functions. We know it is a circulating fluid, deriving its main propulsion from the heart, nature's most effective pump.

But the question, What is it? though the one logically asked first, is the one last answered. We know much of the answer, but not all.

Blood is a highly complex fluid that ties all parts of the body together. We can separate the solids, the red cells, from the liquid part, the plasma. Between World War I and World War II, a sound method was devised for drying the plasma and, by proper packaging, preserving it for use over a period of years. It proved an excellent substitute for whole blood, for it could be easily transported and did not require complicated storage facilities.

Believing that plasma itself contains substances of great value in saving life, Dr. Edwin J. Cohn, of Harvard University, devised an ingenious method for fractionating plasma and producing from it serum albumin. The Surgeon General of the Army and I had an opportunity to work with the National Research Council in putting into effect the studies made by Dr. Cohn and his associates. Consequently, from Pearl Harbor on, there was never a lack of those lifesaving substances.

There was more to be found when scientists probed further.

As dramatic as the story of how plasma saved thousands during the war is the story of blood plasma fractionation. Just as crude oil is broken up into countless useful products, plasma may be separated into as many as sixty constituent parts. Some of these fractions have already found their niche in medical science, but the secrets of many others are still locked within them. Through research, we are diligently seeking their uses and their applications to modern medicine. Though research work on plasma is still in its infancy, the wealth of knowledge we now have is serving the physician, the surgeon, and the public-health officer.

In breaking down the plasma, one of the first fractions drawn off is the substance that causes the blood to clot. Two products of great and practicable value are being made from this fraction: antihemophilic globulin and fibrinogen. Antihemophilic globulin supplies the substance lacking in the blood of hemophiliacs, those unfortunate persons commonly known as "bleeders." The blood of such persons does not clot normally, and they often bleed to death from a minor cut or other wound. Antihemophilic globulin, injected intramuscularly, makes it possible to perform even major surgical operations on hemophiliacs without undue risk, for the blood can temporarily be made to clot normally. It is hoped that this plasma fraction eventually may be used by hemophiliacs in much the same way that sufferers from diabetes administer insulin to themselves hypodermically.

Fibrinogen, the long needlelike molecules that combine with thrombin, another substance of the blood, to form blood clots, is also found in Fraction I. This is a most helpful aid to surgeons in controlling bleeding. It is this product of Fraction I that has the widest use.

Fraction II of plasma contains in concentrated form the antibodies of the blood, both the natural ones and those built up through recovery from certain diseases. Most prominent and widely used product from this fraction is immune serum globulin, used to modify and prevent measles. For this purpose, it is the best therapeutic agent known today. Measles, though a common disease usually associated with childhood, can be very dangerous. Its danger lies in the possible aftereffects, such as fatal pneumonia, a weakened heart, and damage to the eyes and ears. Administered in proper dosage after exposure, immune serum globulin allows a mild case of measles to develop. This generally builds up a lifelong immunity, at the same time preventing a serious case from which complications may ensue. When necessary, in the event a patient already ill is in a weakened physical condition, this plasma fraction can prevent the disease entirely. Preliminary investigations give some hope that immune serum globulin may be effective in preventing complications of mumps, especially in adults. Similarly, investigation in the use of this product to combat infectious hepatitis suggests that it may be effective in modifying and preventing this disease. It is imperative, however, that further investigation be carried out before this globulin can be used in general medical practice for mumps and infectious hepatitis.

The third plasma fraction contains the isoagglutinins of the blood. To give whole-blood transfusions safely, blood must be typed according to the four groups: A, B, AB, and O. Concentrated agglutinins, prepared in a stable form, can be used to type whole blood quickly, providing the best-known agent for accurate blood grouping. From the isoagglutinins of selected people, we can obtain the very necessary serum for determining the Rh factor The discovery a few years ago of the Rh factor in blood has resulted in safer use of whole-blood transfusions. In addition to blood grouping and cross-matching, we now know that the blood to be used for transfusion purposes must be compatible with that of the recipient.

Also from Fraction III comes thrombin. When thrombin is combined with fibrinogen (found in Fraction I), fibrin is the result. Fibrin is a powerful hemostatic agent. From fibrin, two very valuable products can be made: fibrin film and fibrin foam. As its name denotes, fibrin film is an almost transparent, cellophanelike substance which, when soaked in a thrombin solution, is pliable and can be applied to the contours of the tissue. Surgeons find it excellent material in replacing the dura of the brain, and of tremendous help in nerve surgery. Fibrin foam, a sponge-like product derived from whipping the liquid fibrin into a foamy mass and allowing it to dry, is soaked in thrombin to give it hemostatic action. Both these fibrin products are of distinctive importance: They can be left in an incision and, being pure blood protein, will gradually redissolve into the blood stream without causing adhesions or other harmful effects. It has been found that skin grafting can be performed more successfully by the use of this combination of fibrinogen and thrombin.

Fraction IV can be divided into many subfractions. In these, we find globulins that make it possible for iron and fat molecules to combine and be assimilated by the body.

The largest percentage of plasma, almost half, is serum albumin, Fraction V. Serum albumin contains proteins of the blood that maintain osmotic pressure in the blood vessels, thus preventing the liquid portion from seeping out into the body tissues, as happens in cases of shock. During the war, processed serum albumin was packaged in a stable liquid for use as a plasma substitute where space conservation was necessary, particularly in planes and submarines. Its competency in preventing shock is about five times as great as an equal volume of plasma. Knowledge we have of serum albumin opens new avenues for the treatment of certain kidney diseases, such as nephrosis. No doubt one of the major uses of serum albumin will be in cases of hypoproteinemia, where the protein of the blood has been greatly decreased. This condition may result from numerous diseases. Serum albumin is also being used to great advantage in supplying digested proteins to the blood of premature infants. Albumin as a substitute for blood offers advantages over plasma, but in many accident cases and in burns, whole blood is needed as soon as possible.

There are still other fractions of blood plasma that warrant continued study and clinical investigation. This research is currently being done, and there is evidence that many portions of plasma will find extraordinary uses in medical practice.

The red cells, too, are the subject of much scientific study. We know more about them now than was known during the war, when millions of pints of these cells were washed down the drain in the preparation of plasma for the armed forces. They are no longer being discarded. Already under way are studies that will increase the knowledge we now have. In many patients with various types of anemia, there is an inadequate supply of red cells, but these patients have adequate plasma in their blood streams. By concentrating the red cells in the same amount of liquid volume, it has been found that the red blood corpuscle count can be restored to normal in a shorter period of time than would be possible with whole blood. Like whole blood, these red cells centrifuged from the plasma can be put into a preservative solution and with proper refrigeration can be kept usable for as long as twenty-one days.

Red cells can be dried and ground to a powder, or they can be allowed to remain in a thick liquid, or paste. Their use on vascular ulcers and wounds that are stubborn in healing has proved very helpful.

Research on the red cells has not stopped; it is still being carried on, as are the studies on the plasma fractions.

Many of these findings are the results of research intensified during World War II. These are the things we know human blood can do in surgery and in preventive medicine. The question now is, Where and how can these advances be applied to peacetime medicine? Blood is a rare commodity. There is no satisfactory substitute for human blood.

Though we have emphasized the importance of the component parts of blood, we must also point out that the greatest use of blood is for whole-blood transfusions. Blood is amazing in its ability to restore life where blood loss has occurred through accidents, certain diseases, childbirth, surgery, and burns.

WHERE to get enough blood, and how to bring it and its derivatives within the reach of anyone who needs them, were the problems facing physicians after the war. Even in localities where hospital and medical facilities are unusually good, it was difficult to procure enough blood. Blood derivatives such as described here were next to impossible to procure, and, if they were obtainable, they were so expensive that only a limited few of the nation could afford them. The blood plasma declared surplus by the Army and the Navy, returned to the Red Cross, and distributed through state departments of health to hospitals and physicians throughout the country is being used up more rapidly than had been expected. That supply will be exhausted about the end of this year. There was no national planning to provide these products of blood and whole blood itself, needed in ever-increasing quantities. In the event of a national disaster, we would be unprepared.

These problems were being given serious thought. Along with the thinking, tentative plans were formulated to determine whether such a project as a national blood program could be undertaken. The Red Cross, through its Advisory Board on Health Services, composed of eminent authorities in every phase of medical and health fields began to consider whether it should, would, or could carry out such a task.

After months of careful consideration, exploration, and consultation with the many national organizations that would be concerned with such a program, as well as with Red Cross chapters, the decision was made that the Red Cross organization would undertake a National Blood Program. It was discussed with, and approved in principle by, leaders in the American Medical Association, the American Public Health Association, the American Hospital Association, the Army, the Navy, the Veterans Administration, the United States Public Health Service, the American Dental Association, and the Association of State and Territorial Health Officers. Each group was aware of the need for a national blood program and each agreed that the Red Cross, with its broad wartime experience in collecting blood on a large scale, was the organization best suited for the job. In June 1947 the final decision was made by the Board of Governors of the American National Red Cross.

What the National Blood Program will do is to furnish blood and blood derivatives for medical use throughout the nation. There will be no charge for these products, for they will be supplied by the American people themselves, from their gifts of blood. These products will be made available for anyone who needs them, and the only cost to the patient will be the fee of his physician or hospital for their administration.

This is a vast program, one that will require time to be put into effect throughout the entire country, one that will require continuing cooperation on the part of all the American people. It will not be an easy task. Nor will it be an inexpensive one. But it will benefit, directly or indirectly, every person in the country.

In its operation, the National Blood Program must be sufficiently flexible to meet widely varying needs and conditions in large and small communities. It must be carried on with the support of the medical profession, hospital authorities, and the duly constituted health officials.

The problem of obtaining sufficient blood and blood products for wide use will be of far greater magnitude than was the one of providing blood for the military. In order to derive the maximum advantages for all the people, at least two things are necessary: one, to broaden the base of blood procurement, processing, distribution, and research; the other, to organize these activities so that whole communities may participate. These two requirements are interdependent. Broadening the base geographically to include a population of many millions makes it possible to build up dynamic public support, to collect blood economically, to apply mass-production methods to the processing operation, to employ technical specialists, and to ensure a steady flow of blood. The need of organization for such a project is obvious enough. We must not fail to realize that not only are the hospitals and physicians concerned, but that the general public is a very necessary partner.

The National Blood Program may be divided into four distinct phases:

1. Collecting the blood.
2. Processing it for use as whole blood and blood derivatives, including packing and storage.
3. Distributing the blood and blood products for ready availability to physicians and hospitals for the needs of the people.
4. Making blood available for continuous research and investigation to ensure safety of the products and to determine the uses to which they may be put for the greatest benefits.

Collecting blood will follow two procedures: through permanently established centers in large communities; and the use of mobile units that will periodically visit outlying communities, taking with them the equipment and technical personnel required. Suitable quarters will be provided for both types of centers. Medically competent personnel—physicians, nurses, technicians—will staff these centers and mobile units; within the centers volunteer nontechnical assistance will be needed. Red Cross chapters will have the responsibility of organizing their communities to ensure the best possible support in this continuing program. When the project is in full operation, it is expected that approximately 3,700,000 pints of blood per year will be collected.

Processing the blood will require highly skilled work and intricate machinery. Some of the blood collected will be examined, typed, and distributed to local hospitals for use as whole blood, for which there is the greatest need; when available some will be shipped to commercial laboratories equipped to produce the derivatives. Contracts will be made with reliable concerns by the Red Cross national headquarters for the production of derivatives.

It must be remembered that whole blood is a perishable product, requiring careful handling to avoid wastage. Facilities for the rapid distribution of whole blood to hospitals and laboratories must be available. In each community both whole blood and the derivatives will ultimately be accessible to physicians and hospitals for their patients. Not only civilian hospitals, but those of the Veterans Administration, the Army, the Navy, and the hospitals of the United States Public Health Service will be included.

Specialists throughout the country will be supplied with blood and blood derivatives for research purposes and clinical investigations, to make sure that the products are put to the best use. This controlled research will be under the guidance of the Blood and Blood Derivatives Committee of the Red Cross Advisory Board on Health Services, and in collaboration with the National Research Council. The program will bring to medical practice the advantages of new discoveries and new blood derivatives as they are developed.

It is of course imperative that the highest of standards shall be maintained—in the technical work; in the methods for collection, storage, and distribution; and in the investigative field.

This vast program will require large sums of money for its operation. However, the costs will be less as a national program than would be many individual community or state-wide programs. Five million dollars may be needed during the first year and perhaps fifteen million when the program expands to serve the entire nation. The cost of this comprehensive blood program will be borne by the Red Cross, which receives its contributions from the people. No separate fund drives are contemplated; the money is expected to come from funds collected during the annual Red Cross fund campaign. Approximately ten cents per capita is a small price to pay for a national blood program. Yet there is no way to measure the value of lives saved, diseases prevented, and the general improvement of health.

Initial operations will begin in six localities. These have been selected for the advantages they offer geographically and strategically for the program's beginning. These first centers will open in Rochester, New York; Washington, D. C.; Louisville, Kentucky; Atlanta, Georgia; Wichita, Kansas; and Stockton, California. Others will follow as quickly as time, personnel, and equipment will permit. Gradually, this expanding program will reach every community in the United States.

Several communities have shown foresight in planning to meet local needs for blood. Three state departments of health are at present conducting blood programs with Red Cross chapter aid—Michigan, North Dakota, and Massachusetts. Massachusetts has the only complete program, i.e., providing whole blood and the derivatives. Communities that have been considering the establishment of local programs might well proceed with their plans. They will be a step ahead, for eventually it is hoped that such programs may be integrated with the National Blood Program. In this way, full benefits from the fractionation phase of the project will be available to all.

Ahead lies a tremendous task, essential to peacetime needs and national preparedness.

THE ROADS

From my window I see many roads,
Many narrow roads pass by my house,
Highways old are they.
They are silent roads though much used,
No sounds of going do I hear from them.
I never see a traveler there,
No passers-by on any of them,
Yet they are busy roads.
Words travel on these roads—
Words from the rich, a lengthy caravan,
Words from the poor, a little company,
Words power laden,
Words sorrow laden,
Words joy laden,
Swift as the light,
Pass north, south, east, west.
Wonderful! These many narrow roads,
These busy, silent roads
That pass my house.

PAUL B. JOHNSON

IMPRESSIONS OF INDIA*

ALBERT F. BLAKESLEE

Dr. Blakeslee (Ph.D., Harvard, 1904) has had a long and distinguished scientific career. In 1904 he went to Europe as an investigator for the Carnegie Institution; in 1915 he became its resident investigator for plant genetics and then held the posts of assistant and acting director of the Department of Genetics, finally in 1936 becoming director of the Department, which post he held until 1941. He has taught at Harvard, Radcliffe, and the University of Connecticut and is now director of the Smith College Genetics Experiment Station. Dr. Blakeslee was president of the A.A.A.S. in 1940.

THIS is a report of our trip during the winter of 1946–47 to the Indian Science Congress in Delhi, to which Dr. Wm. Edwards Deming and I were delegates from the A.A.A.S. With the exception of Dr. Shapley, who arrived after the Congress had started, the American and Canadian delegates left La Guardia Field December 22 on a BOAC Constellation four-engined plane for an eleven-hour flight to Shannon Airport in Ireland.† There, while we had an early breakfast, we learned a lesson in nationalism from the labels over the various doors—they were in Erse as well as in English and French. Questioned, all the attendants confessed, however, that they could not speak the Irish language, although it is now a compulsory subject in Irish schools.

In London we waited a week for a chartered plane. This took us and the British delegates to Karachi, India, with stopovers at Malta, Cairo, and Basra. An Indian government plane then took us from Karachi to Delhi. Delayed by fog at the London airport, we were a day late in arriving; in consequence, the Congress opened its sessions one day late.

After the Congress, for the most part in the company of Dr. W. F. Hanna, of the Rust Research Laboratory of Winnipeg, I visited the Forest Institute at Dehra Dun, universities and scientific institutions at Lahore, Lucknow, Benares, Calcutta, Darjeeling, Hyderabad, Madras, Bombay, Jodhpur, and Karachi. Each of us gave one or more lectures in most of these cities. I left India by plane on February 9 and, after short stops in Paris and London, arrived in New York on February 18 on the *Queen Elizabeth.* Some of the delegates had shorter, and some longer, stays in India.

I shall not attempt to give a detailed report, but shall instead report some impressions of India gleaned from an all-too-hurried visit to that interesting country.

INDIA is faraway from us in customs and taboos, near though it is in time (thirty-four actual flight hours). It is a land of great contrasts—in climate, races, languages, religions, and types of government. To us the greatest contrast was the extreme poverty of the masses and the comfortable, even luxurious standards of living of the upper classes. As an example, we found people sleeping in the streets and on the steps of the great Mosque of Delhi because they had no other place to go; later, we were lavishly entertained in the Viceroy's sumptuous palace in New Delhi and were given a banquet by a wealthy industrialist, where we were served various wines and champagnes. Delegates to the Congress came in contact primarily with the university people and with the governing classes, both Indian and English.

In the Science Congress we found the meetings of various scientific sessions much like those of our own American Association for the Advançement of Science The discussions were keen, and the criticisms of the papers were searching We were all impressed, I believe, with the knowledge the Indians had of foreign literature, and I had an experience similar to others in finding that my own work was well known to the specialists in my particular field.

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* Contributions from the Department of Botany, Smith College, New Series, No. 21.

† The Canadian delegates were W. F. Hanna, T. L. Tanton, and R. B. Thomson. Those from the United States were A. F. Blakeslee, W. E. Deming, E. N. Harvey, Oscar Riddle, and Harlow Shapley. Those from England were P. M. S. Blackett, William Brown, Sir Charles Darwin, Sir Arthur Fleming, Munro Fox, L. J. Mordell, Sir Harold Spencer-Jones, Dudley Stamp, and Sir D'Arcy Thompson. V. P. Volgin and E. N. Pavlovsky represented the Soviet Union; Jacques Hadamard represented France; and S. S. Chern, China.

It was a surprise to many Americans to learn that the Indian Congress was conducted entirely in English, although there are several hundred more or less distinct languages or dialects spoken in India. Those in the north trace their origin to the Sanskrit. The Dravidian languages in the south have an entirely different origin. English, in consequence, serves as a lingua franca and is spoken fluently by all educated Indians. With the exception of the two Moslem universities, it is the language of instruction in all Indian institutions of higher education. The value, at least to science, of a single universal language in which all educated men can converse is obvious. It was with some concern, therefore, that we found among some of the university people in India a desire to develop a single, national Indian language to take the place of English, although even Hindustani would have to be learned as a foreign language by a large proportion of the people. Perhaps this feeling was both an expression of nationalism and a reaction against anything connected with the English. Possibly our desire to see the spread of English as a world language fostered rather than restricted is somewhat influenced by our own nationalistic instincts, but I believe I should feel the same way about French or German if either were as widely used and appeared to have a better chance of general adoption.

India attributes to us many wonderful things for which we should not have credit. For example, an able woman botanist, in discussing the economic value of algae, said that in India they ought to do as we did in the United States, where every farmer had a fish pond in which he put certain chemicals to induce the growth of algae. Fish fed upon the algae, and the farmer could go out and bring in a crop whenever he desired! Some of the American delegates had heard of this scheme, but none of us had ever actually seen such a fish pond.

Indians believe that we do everything by mechanical contrivances, and, when told that in our homes we had to get along very largely without servants—of which there is an abundance in India—they would reply, "Oh, yes, but you do everything in your houses by push buttons."

Indian students also look to America. We were besieged by students who wanted our assistance in securing admission to American universities. They even called at our rooms before breakfast. One boy who wanted to come to the United States to study atomic physics was only fifteen years old.

The Indians with political influence look to America. For instance, Nehru, who invited Dr. Shapley and me to dinner one evening with two Indian scientists, was very eager to learn the attitude of Americans regarding our foreign policy, more particularly in its relation to Russia. He asked especially about the effect of the Republican victories in 1946 on our policies. We were greatly impressed by Nehru's keen interest in, and understanding of, movements in other countries and by his efforts toward the betterment of his own people. Although Indians of all classes look to American and Western civilization, they do not wish to be placed in the position of an inferior people asking alms. They resent the "master-race" discrimination exercised toward them both in India and in South Africa. In retaliation, they have established a boycott against African goods, and in the Taj Mahal Hotel in Bombay there are signs reading: "No South Africans Allowed."

The British also depend upon America. In January and February, 1947, when we were in India, the British government had not yet set a date for leaving India, but all the British officials realized that the government would soon have to be turned over to Indians. The British expect America to give at least moral support in helping India to get on its own feet.

The Indians have a civilization of which they are justly proud, for its origin may be traced back to Sanskrit times, some of the literature in the Vedas going back to about 2000 B. C. I became convinced that they are not mentally inferior, as we were led to believe before going to India and as was also the impression one would gain from discussions with the British governors of India. The Indians realize that they must depend on Western civilization for their science and technology. I feel, however, that the differences in their scientific and technological outlook are due primarily to environment rather than to genetic constitution; hence, they are more amenable to change.

In my talks with Indian educators I pointed out that up to the early part of the present century we had to look to Europe, especially to Germany, for leadership in science, and that it is not so long ago that America (and Europe) came to realize that this was no longer necessary. In the same way, the Indians realize that they must get foreign training, and they have established fellowships for their best students to study abroad. Whatever shortcomings in colonial policy may be blamed upon the British, there was evidence that they had done much for the development and application of Indian science, at least in medicine. Many Englishmen we met had devoted the better part of their lives to scientific service in important posts in India, and some of them were seeking positions in other countries in view of the early withdrawal of English control. It is to be hoped that the value of their service will not be entirely lost, and that standards may even be raised now that India has taken over complete control of her own destiny.

The British in India have said that as scientists the Indians are most successful in mathematics and theoretical physics. This seemed to be true, although Raman, who received the Nobel Prize in physics, was and is a good experimentalist. The Indians, however, do not seem in general to do so well in experimental sciences. There were, of course, some notable exceptions, and in some of the laboratories we visited there was a keen spirit of research.

The greatest fault I could find with Indian scientists was a certain disinclination to use their hands. This fault I have also found among some other foreign scientists. The lack of appreciation of the dignity of manual labor, as well as the caste system—which now appears to be in process of gradual elimination—is a great handicap to their technological advancement. For example, an Indian who, when I met him, was scientific and technological adviser to one of the Indian states, told me that when he was a student in one of the institutes of technology in the United States he did as many of the other students did; namely, earned money during the summer in manual labor, washing dishes in a hotel, etc. When he wrote his parents what he was doing, he received two cablegrams telling him to stop it at once—that he was disgracing his family. The same scientist told me that he had available an industrial position at 1,200 rupees (about $400) per month, but that if he paid someone that salary he felt the man would not want to work in the shop, but would wish merely to sit at a desk. Perhaps as valuable a thing as any that I told Indian scientists with whom I had conversations was that I helped my wife wipe the dishes in our own household. The willingness to do—even at times the necessity of doing—things with one's own hands would be one of the best lessons, I feel, that could be learned by Indians who visit our country.

One of the American vice-consuls in India told me that the difference between Indians and Americans could be demonstrated by the fact that, when an American gets a Ph.D. degree, that is the start of his going to work more intensively, but when an Indian gets a Ph.D., that is likely to be the end of his work—he feels he has reached his goal.

I did get the impression that there was in many cases a letdown in the scientific activity of Indians who had reached a full professorship in their universities. I also found that the position of a man seemed to be more esteemed than the work he was actually doing. In one or two cases, the men who appeared to me to be doing the most original work in certain institutions had considerable difficulty in getting to me to talk about their investigations. In fact, I sometimes had the feeling they were actually being kept from me.

That "pull" is considered of much value in advancement was suggested by several requests made to me that I mention the work of certain Indians when I gave my lecture at their institutions. Another example is a letter recently received from a student who had failed to secure a fellowship from the

Indian government to study for a doctor's degree in the Genetics Experiment Station at Smith. He intimated that influence was important and suggested that I write to certain government officials urging them to reconsider their decision not to award him a fellowship. We heard other complaints that on account of political and personal influence Indian fellowships did not always go to the best students. Whether these complaints were sometimes justified or were merely the reactions of disappointed candidates, we were not able to judge. Naturally, however, I did not write the letters requested.

I should like to be a maharaja, not because of the great wealth and multiplicity of wives or their equivalents, with accompanying luxurious living, but rather for the good that I might do by means of science and its application. We were impressed with the need of, and opportunities for, intelligent philanthropy, both public and private. We had the privilege of visiting a few institutions established and maintained in whole or in large part by private donations. Among these were the Bose Institute for Plant Research in Calcutta, the Tata Hospital in Bombay, and the Sir Shri Ram Industrial Institute in Delhi, the dedication of which we attended during the Congress. Such institutions, however, appear to be relatively rare. Wealthy industrialists are not so numerous in India as with us, and in general they seem not to have acquired the habit of philanthropy in support of science and education.

Many prominent Indians have had British titles conferred upon them. These are highly esteemed despite the feeling in some quarters that their bestowal has been an attempt to buy allegiance to the British crown. If I were a member of the Indian government, I would suggest that India take over the conferring of titles, but on a revised basis adapted to her own country. I would suggest the title of "Indian Benefactor" for those who had made significant contributions to the welfare of India in art, literature, science, or philanthropy. Such titles might correspond to Nobel Prizes, but on a broader basis. A man so honored might write after his name the initials I. B. (B. I., "Benefactor of India," might be confused with "British India.") He might be addressed in the new Indian system as Benefactor Iziid Singh or, more simply, Bene Izzid, instead of the English system of Sir John Doe, which is usually simplified to Sir John. Such a system of nobility might emphasize the fact that service to one's own people is most worthy of recognition.

THE frank criticism I have made regarding some things that I feel are imperfections among the Indian scientists could be made also of many of our own American institutions. I came back from my trip, however, with a great enthusiasm for our Indian friends and a strong feeling that India is a land of great potential power.

The scientists and students we met appeared very appreciative of our visits and lectures. I myself feel that I got more than I gave in adult education. The trip was undoubtedly of value to both the scientists of India and those abroad in fostering international understanding. This, I believe, can best be done through science, for there is only one science.

THE COUNSELOR SAYS "M-HM"*

DANIEL MALAMUD

Mr. Malamud, born in Russia in 1921, received his M.S.S. from the New School for Social Research in 1942. He did clinical psychological work at Norwich State Hospital, Connecticut, until, in 1945, he entered the Army, where he administered group therapy at the Fort Knox (Kentucky) Rehabilitation Center. In 1947 he was appointed research director of the Central Harlem Street Clubs Project at the Welfare Council of New York.

"IT WASN'T what I expected at all, He just listened to me talk, said 'M-hm' a good deal of the time, didn't give me any advice or anything like that, and yet I feel that he helped me a lot. It was almost as if I were talking to myself, but with someone listening and trying to understand me. Blowing off steam right out loud like that and hearing it made me think about it, and helped me see things in a new light!"

That's the way a GI veteran summed up his experience with a new kind of counselor—one who helps clients solve their personal problems in their own way without giving advice. These counselors have come to believe that they have no right to direct a client's life. They feel that an individual must choose his own goals, and that if he gains enough understanding of himself and the nature of his problems he will make such choices wisely.

These counselors let the client do most of the talking. They encourage him to explore his problems and feelings freely, and come to his own solutions. They do not ask probing questions. They do not approve or disapprove of the attitudes he expresses. They do not tell him what they would do if they were in his shoes. This type of counseling focuses on the client—his feelings, his problems, his decisions. It is nondirective and client-centered.

Many other counselors, however, use a different approach. When someone comes to them for help they "take over." Like a doctor intent upon making a diagnosis, they ask many questions about the client's problems and symptoms and finally decide just what the trouble is and what caused it. Then, by means of pep talks, subtle suggestions, or logical arguments, they persuade the client to follow definite courses of action that they feel are for his own good. In the extreme of this type of counseling all the client has to do is answer the counselor's questions and follow his recommendations. This kind of counseling is directive and counselor-centered.

More and more counselors are beginning to feel that such directive techniques, in most circumstances, are not really helpful at all. Sometimes the initial problem that the client brings to the counselor is only a surface symptom of a more deeply underlying maladjustment. For example, it is well known that many students seeking vocational or educational guidance are really troubled by more basic difficulties. Yet, some counselors, because they take the initiative out of the client's hands and concentrate on the first problem presented, may miss this underlying maladjustment entirely.

Sometimes when a counselor takes the responsibility for solving a client's problems he may find the "answers," but at the expense of making the client a more dependent individual. The client may feel inferior because he had to lean on someone else for help. As a result, he may feel less confident to handle future problems on his own.

Some clients secretly resent being told what to do. Although they may listen passively enough, they will not follow the counselor's advice. Perhaps they have always rebelled against authority. Perhaps they feel their integrity is threatened when they are told what to do. Some clients, believing they have not been given the right advice, shop around until they find a counselor who tells them what they want to hear.

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* The author is grateful to Dr. Carl R. Rogers for permission to quote brief excerpts from his writings. Examples given are reprinted by permission of the copyright owners from *Counseling with Returned Servicemen* (Rogers, Carl R., and Wallen, J. L. New York: McGraw-Hill, 1946).

Sometimes clients are unable to follow the suggestions they get from counselors. Advising a depressed person to buck up or an alcoholic to control himself is like telling someone to pull himself up by his bootstraps. People may be able to force themselves into states of cheerfulness or self-control, but usually for short periods of time only. When the almost inevitable relapse sets in, they are left feeling worse than when they started. They believe they are spineless weaklings without will power.

The unsuccessful experiences of many psychologists and psychiatrists with this latter type of counseling has led to the evolution of the nondirective approach. Dr. Carl R. Rogers, professor of psychology at the University of Chicago, has most clearly defined this type of counseling. In his two books, *Counseling and Psychotherapy* and *Counseling with Returned Servicemen* (the latter written in collaboration with Dr. J. L. Wallen), Dr. Rogers has given clear expression to his belief that many individuals can work out their own problems better than anyone else could do it for them—provided they get a chance to understand themselves better.

In a majority of cases, nondirective counseling goes through three main phases: in the first, the client freely talks out his problems; in the second, he gains a better understanding of himself; and, in the third, he decides what definite actions to take. These phases don't always come in one-two-three order; they often overlap. But they have been so frequently noted that their appearance is one of the most predictable characteristics of nondirective counseling.

IN THE first phase, the client is encouraged to talk freely about his problems and his feelings about them. The counselor encourages him by means of attitudes and techniques which, although seemingly simple, are extremely difficult to acquire. He is friendly and receptive. He neither feels nor shows approval or disapproval. At times he says nothing more than "M-hm," "Yes," or "I see"—remarks that indicate simple acceptance. Frequently, he will restate what the client has said in such a way as to reflect and clarify the feelings expressed. Sometimes the feelings are those of despair, or hostility, or guilt—but whatever they are, the counselor, by recognizing them and reflecting them back to the client, creates an atmosphere in which the client recognizes he is being understood and accepted for what he is. Several examples might be given. The first one is taken from a case of an aviation cadet who was failing in his solo flights:

Cadet: I should have soloed long ago. And here is something. Before I joined the Navy I was an overhead electrical crane operator, and that takes depth perception, coordination, and alertness; and I'm positive that I can apply that to my flying.

C: You feel that your training as a crane operator should help you in your flying.

Cadet: That's right. And here's something else . . .

The second example is from an interview with a serviceman who was AWOL:

Dan: Well, it's a long story. I was drafted and I didn't want to go, but I went. I was sent to "X" Field for my basic training. I didn't like that at all. It was very dull, and there was nothing to do in town, and I found that all the interests I had at college were slipping away.

C: The whole business of getting into the army was pretty distasteful to you.

The third excerpt is from an interview with a bride of twenty who came to the counselor because she could not work in her disturbed emotional state:

Mrs. B: It's me and my husband. He's a sailor. We don't get along. I only knew him a week before I married him. Then he was overseas for a year, but now he's been home on a 30-day leave. It just didn't work out.

C: I see.

Mrs. B: He still loves me, but I don't love him. I don't think I did when I married him.

C: You doubt that you've ever had much feeling for him.

Mrs. B: He's nice in some ways. We talk together for hours about music and things. We have quite a little in common, but I just feel I was too young to get married. I'd be missing something if I stayed married. I want to go around with other fellows. I'm not ready to settle down!

C: You don't want to be tied down to one man—at least not yet.

The more accepting and permissive a counselor is, the freer the client becomes to dig deeper and deeper into his feelings. He begins to see that any of his attitudes, both negative and positive, can be brought out into the open. He sees that he needn't worry that the counselor will criticize him in any way. He finds that he can drop his defensive front. He becomes free to look at himself and his problems objectively without having to protect or justify himself.

Thus, the client enters a second phase in which he begins to understand things about himself that he didn't realize before. Things that were once unrelated now seem to tie up. He sees his problems in a new light. For example, in the case of the aviation cadet, counseling uncovered an intense hatred for his unreasonably strict father. Gradually the cadet began to see the connection between his attitude toward his father and his resistance toward his flight instructor's directions:

Cadet: You know after the last interview I wondered what made me tell you the things I did. Could it be possible that the instructor is a symbol of my father? Is that hatred coming back to blot my memory? Could that possibly be significant?

C: You wonder if perhaps the instructor might be a symbol of your father?

Cadet: Yes, he was telling me what to do just like Dad always did. I fully intended to carry out the instructor's directions; I couldn't not want to do them. Maybe I forgot because I thought of Dad and wanted to forget.

Mrs. S, a young, highly educated mother who was having trouble with her boy, began to face the underlying rejection of her child:

Mrs. S: I'm afraid I'd have to say this of myself, I really didn't want Buddy. We were married two years, and I had a job. My husband didn't want me to work. We thought children would be the best solution. We felt social pressure too. With the birth rate up in the lower groups, college graduates should have children. In a limited way we were emotionally interested in him, but not deeply. And I've never adjusted to having him! It's terrible to say this!

One client saw the relation between himself and his brother in a new light:

Client: I see now why I resented my younger brother so much—why we have never been able to get along together. It was because I was envious of the attention my mother showered on him. It wasn't because of anything he did. And all this time I thought it was because he was so unfair and mean.

In the second interview the young bride talked about her unhappy home life, how she was "farmed out" to relatives, how she gradually developed the feeling that you "grabbed onto everything you could get, and hung onto it." She realized that this was the reason why she could not be generous to others and sacrifice anything for the sake of her marriage. As the interview drew to a close she said, "Maybe it would help if I just grew up."

In the third phase of counseling it is fascinating to watch the client begin to make positive decisions and to act consistently with his insights. These decisions are likely to stick because they come spontaneously from within. They grow out of the inner needs of the client himself, not in response to an outsider's advice or persuasions. For example, the soldier who had gone AWOL and talked out his situation with a counselor concludes, "I realize I've got to go back and face the music." He adds realistically, "There is no use in pretending that I won't be afraid when I go back."

Another soldier, after realizing that he didn't go to parties and dances because he feared doing the wrong thing and being rejected by others, decided to develop social skills such as dancing in order to fit in better with other people.

The young bride began her last interview by saying that she had tried being generous and thoughtful toward two of her friends and found to her surprise that it worked. They did not take advantage of her generosity, and she felt much better within herself. She spoke of her marriage in a new way: "I've decided I'm going to give marriage a try. I've never really done that. We may not be able to make a go of it, but I'm certainly not going to give him up and give up our marriage without really working at it."

As the client takes hold of his own life and feels the original strains reduced, he takes the initiative to break off the counseling contacts. He feels as if he has matured during these counseling sessions. He feels that he has not only solved his original problems, but that he is much better prepared to face whatever future difficulties may arise.

THE nondirective approach relies heavily on the individual's own drive to achieve better adjustment. It permits the individual to choose his own way of life. It does not set the counselor up as superior to the client. It does not attempt to impose any set of values or beliefs on the client. It truly respects the individual's right to decide his own problems for himself.

A growing number of professional workers have become interested in the nondirective approach. They have found it effective with the majority of servicemen, students, children and parents, and other adults with adjustment problems. It also seems to have possibilities for the extremely maladjusted or abnormal, but experience in these extreme fields is limited and no definite conclusions can as yet be drawn.

In addition to defining nondirective techniques, Dr. Rogers has gained widespread recognition for taking the counseling process out of the mysterious realm of art and studying it as scientifically as any other human behavior. By means of phonographic recordings of counseling sessions, he and his students have been able to make objective analyses of just what goes on between the counselor and the client. Also, by means of objective psychological tests, they have successfully measured the changes in personality brought about by counseling. These accomplishments make possible scientific comparison of the different types of counseling approaches—an achievement of the greatest significance for applied psychology.

Dr. Rogers has used phonographic recordings not only in research, but also as an aid in training future counselors. The counselors-in-training, by playing back records of their sessions, analyze their mistakes under the supervision of experienced counselors.

Nondirective principles may apply to fields other than individual counseling. A beginning has been made in the use of these principles in group therapy. Nondirective interviewing in opinion polls promises to eliminate much of the factor of bias. The implications for education, industry, and social relations challenge the imagination.

Dr. Rogers himself emphasized the potentialities of his approach when he stated:

> What we now know or think we know about a client-centered approach is only a beginning, only the opening of a door beyond which we are beginning to see some very challenging roads, some fields rich with opportunity. It is the facts of our clinical and research experience which keep pointing forward into new and exciting possibilities. Yet whatever the future may hold, it appears already clear that we are dealing with an orderly and predictable process which may prove as significant a basic fact in social science as some of the laws and predictable processes in the physical sciences.

PATENTS AND UNIVERSITY RESEARCH*

ARCHIE M. PALMER

Dr. Palmer (D.C.L., University of the South, 1941) has had a distinguished career in the field of education. He has held numerous posts: acting dean of the College of Arts and Sciences of Cornell, assistant director of the Institute of International Education, associate secretary of the Association of American Colleges, and president of the University of Chattanooga. Dr. Palmer is now director of the Patent Policy Survey, National Research Council.

PATENTS are usually fortuitous byproducts of research. They are not necessarily the conscious or inevitable end results of scientific investigation. This is particularly true of the products of research on the university campus, conducted with a view to expanding the frontiers of knowledge, encouraging and stimulating the spirit of inquiry, and contributing toward the training of scientific and technological personnel.

Concerned primarily with the discovery of new ideas and the understanding of nature and its laws, most scientists working in university laboratories are content to pursue their investigations without much thought of the practical application of the results. The discovery and development of patentable inventions are not conscious objects of their research efforts. They feel with Sir Henry Dale that "the primary and special function of research in the universities is to build the main fabric of knowledge by free and untrammelled inquiry and to be concerned with the practical uses of it only as these arise in the course of a natural development."[1]

However, many new ideas, discoveries, and inventions, the result of experiments undertaken with quite a different purpose in view, may have valuable commercial application or require protection and control in the public interest. They may not only be essential to scientific and technological progress and to cultural and social advancement, but these new ideas may also be basic to industrial development and expansion. The protection and control provided under the patent laws may have to be invoked to obtain the greatest public benefit and usefulness from these products of scientific research.

Some scientists in university circles take the attitude that the publication of the results of scientific research and their dedication to the public are sufficient. But, as President Karl T. Compton, of the Massachusetts Institute of Technology, said in his annual report for 1932:

> Responsibility does not always end with the mere publication of a patentable scientific discovery or invention; the public benefits derivable from the patent laws and contemplated by the framers of those laws should not be lost through a failure to solicit patent protection.[2]

Discoveries or inventions possessing commercial application that are merely published and are thus made available to everybody equally are seldom adopted. As Elihu Thomson so aptly put it:

> Publish an invention freely, and it will almost surely die from lack of interest in its development. It will not be developed and the world will not be benefited. Patent it, and if valuable, it will be taken up and developed into a business.[3]

Interest in science and scientific research, particularly in the natural sciences and in their application to engineering and medicine, was intensified and accelerated by our experiences during the war. Research workers and scientific investigators from the university campus and the industrial laboratory aided materially in the magnificent record our nation made in war production and military achievement. Returned now to the campus and the laboratory, on release from wartime responsibilities and occupations, they are more research-minded than ever. Interest in research is being further stimulated by government and industry, which are turning to universities and technological institutes for assistance in solving postwar problems.

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* From an address before the Association of American Universities, Iowa City, Iowa, October 24, 1947.

The situation is made more acute by the critical shortage of scientific and technical personnel and the need for developing a new crop, grounded in fundamentals and trained in research procedures. This new supply of qualified scientists to meet the needs of the future as well as the immediate present must come from the universities. At the same time universities are expected to continue as centers of basic research.

American science faces a challenging future. Can science be mobilized for peacetime purposes as effectively as for war? Will scientific investigation be conducted under conditions favorable to the search for new knowledge? Can we build upon and utilize our wartime experiences and the present research consciousness among scientists and the public generally? How will our universities, the primary source of independent scientific investigation, respond to the challenge?

These are questions of paramount importance if this nation is to discharge its responsibilities and assume leadership for peace and progress in the postwar era. Social and national security, public and private health, and economic prosperity and well-being depend today, as never before, upon the rapid extension of scientific knowledge and the effective application of that knowledge.

Of direct concern to university administrators and scientists engaged in the formulation and conduct of research programs is the policy or procedure to be followed in handling the results of scientific investigation. How can the greatest public benefit be obtained from new discoveries and inventions? Specifically, how should these discoveries and inventions be administered in the public interest, taking into account the objectives of the institutions and the over-all welfare of the scientific workers?

Whether we are to enter upon a rich era of productive research, profiting from our wartime experiences and capitalizing on the present research consciousness among scientists and the public generally, will depend to a large extent upon the philosophy behind our university research programs and the administration of those programs. The public welfare, educational objectives, direction of scientific thought, and the advancement of knowledge are all involved.

As a service to American higher education and to the scientific fraternity, the National Research Council has been making a comprehensive study of this problem. As a first step, a factual survey[4] is being made of the prevailing policies, procedures, and practices in educational institutions and nonprofit organizations for the administration of patentable results of scientific research, with a view to the early publication of the findings for the information and guidance of all concerned. Through correspondence, conversations, and visits to university and other research centers, information has been assembled concerning existing practices and present thinking, in administrative and scientific circles, about research policies and patent management programs.

At present there is a wide diversity of practice among educational institutions—and even at the same institution—in dealing with patentable discoveries and inventions growing out of scientific research. There is no common pattern of policy statement, administrative procedure, recognition of the inventor, determination of equities, assignment requirement, patent management plan, distribution of proceeds, or protection of the public interest. Nor is there any convenient grouping according to type or size of institution, complexity of university organization, or kinds of research undertaken. Existing practices vary from strictly drawn patent policies to laissez-faire attitudes and even an unwillingness to become concerned with patents.

Some institutions follow a hands-off policy, leaving to the individual inventor the responsibility for determining what disposition is to be made of the product of his research efforts. Others take the position that the institution has an interest in all research activity on the campus and have established formal patent policies or follow generally accepted practices for handling any patentable discoveries that may result. Still others observe a definite policy of not having a patent policy. A great many, however, have given little or no consideration to the patent problem, despite the increasing volume of scientific investigation on the campus.

Through the years certain institutions, faced with immediate situations, have formulated more or less definitive patent policies. Yet, less than forty such policies have been formally adopted thus far, more than half of them during the past five years. At a number of other institutions practices and procedures are being currently followed which, though not definitely formalized, are generally accepted as applicable to research activities throughout the institution. A few have adopted special policies or recognize general practices for dealing with those results of scientific investigation that affect public or private health. Others have developed policies and practices only with respect to sponsored research.

Many of the formalized policies and most of the prevailing practices are currently under review to meet changing postwar conditions and current considerations in the institutions. The need at this time for critical examination of the whole question of what to do with the patentable products of research is well recognized. At a number of institutions, many of which have not previously had any patent policy, faculty and trustee committees are currently studying the question, with a view to formulating new, or revising existing, policies.

At many institutions each case is decided on its merits in accordance with a general policy or, in the absence of such a policy, by agreement among the parties concerned. A few still feel that they discharge their responsibility by merely publishing the results of investigations or by securing patents and dedicating them to the public. Others accept full responsibility for obtaining patents and administering the patent rights in the public interest. Many exercise control over the patents by issuing licenses and accepting royalty payments, either directly or through agents whom they have designated to manage their patents.

Some recognize the rights and interests of the inventor and share the proceeds with him, either under a prior contractual arrangement or by mutual agreement, but there is no uniformity in the division of the financial return from patents between the inventor and the institution. Even in those instances where the proportion to be given the inventor is specified in accordance with a general policy, there is a wide variation among institutions in the amount allotted to the inventor. In other institutions the inventor's share is determined in each case after consideration by a special faculty or administrative committee. A few institutions include patent provisions in their contracts of employment, in some instances for all faculty members, but more often limited to members of the staff whose entire or major responsibility is research, especially contractual research.

At most institutions the compulsory assignment of patent rights is not considered desirable, except when necessary in connection with cooperative or sponsored research. Voluntary assignment is preferred and in many institutions is encouraged and facilitated through procedures and special machinery for handling patents set up within the institution. In many instances the services of an outside organization closely related to the institution and under agreement to act as its patent management agent are employed.

Some institutions administer patent applications, and the resulting patents, directly, utilizing their regular administrative personnel, special units within the institutions, or separately organized agencies directly responsible to the boards of trustees of the institutions. Others, for legal or fiscal reasons, use the facilities of separately incorporated patent management foundations, independent of, but closely allied to, the institutions. Still others have entered into agreements with Research Corporation, a nonprofit patent management foundation, to handle patentable discoveries in their behalf, with full protection of their interests and those of the inventors and the public. Most institutions endeavor to avoid becoming involved in the intricate legal and commercial aspects of patent management, mainly because they lack personnel with the requisite specialized knowledge and experience.

IN ORDER to provide incentive and encourage personal research interests of faculty members, most educational institutions place little or no restriction on the disposition of inventions and patentable discoveries resulting from scientific research conducted on an individual's own time and at his own expense, even though the institution's facilities and equipment may have been used. Such inventions are considered to be the exclusive property of the inventor, and he retains the full patent rights and complete freedom to dispose of them as he deems proper.

Institutions with formalized patent policies usually recognize, by explicit reference or by implication in formal policy statements, that an invention or discovery not related to the individual's regular teaching or research responsibilities belongs to the inventor, and accordingly waive all claim to a share in possible financial returns. Similarly, at many of the institutions which, in the absence of an established policy, follow generally accepted practices—as well as those which observe a laissez-faire, or hands-off, policy—ownership of patents resulting from personal research rests with the inventor.

In the absence of established policies, some institutions consider each case on its merits, leaving it to the judgment of the faculty member whether he should bring what may seem to be patentable discoveries to the attention of the president or designated administrative officer or faculty committee charged with consideration of research and patent problems. A few universities with definite patent policies require that all potentially patentable discoveries, as well as the intention to apply for patents, be brought to the attention of the administration, either directly or through appropriate committees.

A number of institutions have special committees or boards to which are referred patentable discoveries, questions of the institution's interest in them, and the desirability of securing patents at the institution's expense. When recommending the specific action to be taken in each case, these committees usually also determine what recognition or reward, if any, should be given to the inventor. In many instances the inventor is required or advised to assign his rights to a patent management organization designated by the institution to represent its interest and handle the commercialization and general administration of the patent rights.

In the administration of formal patent policies, a number of institutions use these committees or the patent management agencies to advise and aid faculty members on matters of patentability, prosecution of the patent application, commercialization of the patent when issued, and general business aspects of patent management. Through these committees and the regular university administrative channels, and also through the facilities of patent management foundations, means are provided whereby, by either voluntary or required assignment of their patent rights, faculty members may be relieved of the burdensome legal and administrative problems associated with the commercial exploitation of patents.

Frequently these committees also have responsibility for determining whether the institution has any interest or equity in the discovery and for defining what action should be taken in line with the prevailing patent policy or accepted practice of the institution. In many instances it is difficult to determine the extent to which incidental or permitted use of the equipment and other facilities, membership in the company of scholars assembled on the campus, professional contacts with colleagues and others connected with the institution, and the general intellectual atmosphere and surroundings contribute to the evolution of patentable ideas. Certain institutions require reimbursement of whatever contribution in institutional time, money, or facilities has been made to the production of a patentable discovery, even though the patent rights remain the sole property of the inventor.

Even where inventions and other developments grow out of research entirely or substantially financed by the institution, there is considerable variation in the patent policy observed, the procedures followed, and the recognition of the inventor. However, when the research is part of the regular duties and responsibilities of a faculty or staff member, it is generally the practice to require assignment of title to such inventions and developments, as well as any patent rights that may accrue from them, to the institution or to its designated agent. In such cases, the institution bears the costs of obtaining the patent and assumes responsibility for its exploitation. Provision is usually made for the patent rights to revert to the inventor if the institution or its designated agent does not file a patent claim within a reasonable time, which is sometimes but not always specified in the assignment agreement.

Most institutions require full-time research personnel and others employed on special research projects to sign patent assignment agreements covering all patentable ideas and discoveries that may result from their investigations. Such agreements are generally required of full-time research employees in state agricultural and engineering experiment stations, and also of those employed on projects conducted in, or under special research institutes affiliated with, educational institutions.

Few patent policies include any reference to patentable discoveries resulting from student research, except where the student is employed or receives specific fellowship aid under an industrial research contract. In general, inventions made by students, including those on academic scholarships and fellowships, are considered to be the private property of the students. Nevertheless, the question of requiring students to sign patent waiver agreements is occasionally raised, especially when the students are given scholarship aid. The acceptance of such scholarship aid, however, is usually not considered as changing the status of the student in regard to title to inventions or developments, since such scholarship funds are in general administered, rather than controlled, by the institutions. The rights of the student include the right to assign or otherwise dispose of his patent rights.

AN IMPORTANT—and controversial—aspect of the patent problem is concerned with patentable products of scientific research that affect public and individual health, particularly discoveries and inventions of a medical, pharmaceutical, therapeutic, or hygienic nature. Those universities that have comprehensive patent policies usually include such discoveries, processes, developments, and inventions within the scope of general over-all policies. A few provide specifically for a different treatment of medical discoveries, designed to discourage patenting except when it is considered necessary in the public interest and then without consideration of profit, either to the individual or to the institution. A considerable number have no fixed policy: when cases arise, each one is handled individually, usually without following any uniform pattern except, as a general rule, to discourage investigators from seeking patents on such discoveries.

The prevailing practices of educational institutions, especially those with medical faculties, are influenced to a considerable extent by the traditional attitude of the medical profession as to the ethics of patenting medicinals and medical appliances. Such an attitude, however, does not necessarily preclude patenting a new process or discovery in the public interest. Yet, many scientists working in this field take the position that the results of their research, both patentable and otherwise, should be shared "without fee or stipulation." In so doing they often fail to differentiate between patenting for personal gain and patenting in the public interest.

Through the centuries, medicine has given freely of its discoveries for the benefit of mankind, and these discoveries have become the property of all who cared to employ them in the control of disease. However, as medical research has become more complex, involving specialized investigations in the fields of biochemistry, physiology, physics, and associated branches, great numbers of full-time research scientists in the hospital and the laboratory work with members of the medical profession but are not bound by the same ethical principles. Many important medical preparations and techniques have been developed in university laboratories, often at considerable expense to the institutions.

The patent problem is not a settled one in the medical schools, and a wide difference of opinion exists among their faculty members as to the ethics of patenting a medical discovery, but in many of these schools the question is being given thoughtful consideration. Much of the stimulation for the establishment of definitive patent policies stems from problems growing out of research projects sponsored by outside agencies, especially commercial firms. Frequently, such practices as are currently followed are concerned solely or mainly with the results of scientific research conducted under grants from those outside sponsors.

SCIENTIFIC research sponsored and supported by industry is a major activity on many a university campus. It is conducted both as an integral part of the educational program and as a special service to industry. Support comes in many forms: as unrestricted gifts, grants-in-aid, industrial fellowships, and the financing of specific research projects.

Industry support of university research is not a new phenomenon. For years industrial corporations and trade associations, as well as individual industrialists, have provided funds for the conduct of both fundamental research and specialized developmental, or applied research, investigations. Progressive business executives recognize the potential value of the research facilities and the scientific personnel in universities and technological institutions in the promotion and expansion of industrial progress.

In its return to postwar status, industry has been turning to the colleges and universities for assistance in solving its reconversion problems. Unable to provide within their own organizations means for producing new ideas for the improvement and replacement of obsolete facilities and processes to meet postwar conditions, large and small businesses alike, as well as trade associations and groups of related industrial firms, have been seeking the services of educational institutions in research on specific developmental problems.

Despite the heavy teaching load resulting from swollen postwar enrollment and their own lack of adequate instructional personnel, educational institutions have been quick to respond to this new call upon them. A number have for years been rendering such service to industry, both on an institutional basis and through consulting and research work on the part of individual staff members. This has been particularly true in state universities, land-grant colleges, and technological institutes. However, largely as the result of experiences with war contracts and observation of what others have done and are doing, there has been a material increase during the past several years in the number of colleges and universities offering research services to industry.

An appendix in the National Research Council's recently published directory of industrial research laboratories[5] lists approximately three hundred educational institutions that offer such services, and the list is admittedly incomplete. At a number of institutions, special research institutes, corporations, and foundations, usually independently incorporated but closely related to the institutions, have been established for the conduct and administration of sponsored research programs, as well as the management of the patentable results of the research.

Encouraged by the success, often more apparent than real, of certain of these organizations, more than seventy colleges, universities, and technological institutes have set up such agencies, many within the past three years, and others are contemplating similar action. These organizations are located in all parts of the country and at all types of institutions, large and small, public and private—at endowed universities, state universities, land-grant colleges, technological institutes, and small colleges alike.

Some are integral parts of the administrative and organic structure of the institutions concerned, operating as special departments or divisions. Others are independent nonprofit foundations, separately incorporated but closely affiliated with the educational institutions and utilizing their regular personnel and facilities. A few maintain special research laboratories and separate personnel distinct from the regular teaching staffs of the institutions. Combinations of full-time services of special research workers and part-time research and supervisory services of regular teaching members are found at a number of institutions.

Many of these agencies have been set up to provide convenient means for relieving the institution's regular business and administrative staff of contractual relations with research sponsors and of patent management problems. In some instances they are also concerned with the general development of new sources of financial support for the institution itself. Still others are designed to provide machinery for conducting sponsored research activities and for handling patents, particularly where restrictive statutory provisions make it either impossible or undesirable for the institutions to perform these services themselves.

The creation of these special research organizations and the conduct on the university campus of extensive research programs supported by industry, and also by government, raise problems whose implications are more far-reaching than is immediately apparent. What influence will such programs have on the character of scientific investigation in our American colleges and universities, and what effect will they have on the educational programs of those institutions? Will the emphasis be on developmental research? Will basic research suffer? Will there be greater interest in immediately usable end results than in the search for new knowledge? Will too much reliance be placed upon the financial return from patents and from sponsored research projects in balancing the institution's budget? Will due consideration be given to the uncertainty of that revenue and its possible effect upon other sources of income and upon the tax-free status of the institution?

There is a wide diversity in the organization and operation of these agencies and in their handling of the policies and responsibilities of sponsored research programs. There is no uniformity in the terms or conditions under which sponsored research projects are accepted or conducted, nor in the determination of the charges made. Some institutions have established specific policies for handling such research; others make the best arrangements obtainable in each case. Some will accept only projects which are definitely related to their educational programs and which can be performed by faculty members and students as part of their regular activities. Others have set up special facilities for sponsored research, employing personnel who devote full time to such activities. A number have established special bureaus or divisions within the institution to relieve the faculty and regular administrative personnel from contractual relations with research sponsors.

The patentable products of such research are handled in many different ways, the ownership and control of patent rights sometimes being retained by the university but more often being turned over to the sponsor under a predetermined contractual arrangement. Certain institutions are unwilling, and a few refuse, to undertake research projects likely to entail patentable developments. Others are willing to undertake such research projects only when they retain complete control over both the patent rights and the publication of the results of the investigation. Still others will enter into contracts under which the sponsor receives, for a consideration, ownership of all patentable discoveries, as well as full and confidential report on the research findings.

It is usual practice for educational institutions to retain control over the publication of the results of research conducted on the campus. When an investigation is financed through outside funds, that control is frequently but not always exercised subject to prior consent of the sponsor, and publication is withheld for a reasonable time to protect patent applications and the interests of the sponsor in commercial development of new discoveries or processes. A few institutions turn over all the results to the sponsor, including publication privileges as well as patent rights, merely reserving approval of any reference to the institution or its part in the investigation. In most instances they proscribe use by the sponsor of the name of the institution in any way.

PATENT management is a complicated business and is expensive. It requires a high degree of legal competence, administrative astuteness, and promotional zeal—a combination of talent not always readily available in an educational institution. The patent search is a specialized technical service. The preparation and processing of patent applications are exacting work for legal counsel. The administration of patent rights demands careful attention to intricate details and constant watch for infringement and interference. The exploitation and disposal of patents, through sale and licensing agreements, require salesmanship of a high order.

It is natural, therefore, that most educational institutions make every effort to avoid becoming directly involved in the intricate legal and commercial aspects of patent management. Some endeavor to accomplish this by adopting a hands-off policy and refusing to handle the patents. Others have established, or have encouraged the organization of, separately incorporated patent management foundations, independent of, but closely related to, the institution by the terms of their charters and by the membership of trustees, administrative officers, and faculty on the foundations' boards of directors. Still others have entered into agreements with Research Corporation to act as their patent management agent.

A few attempt to handle patents as a part of the routine duties of already established administrative units, such as the comptroller's or business office, or through specially designated committees responsible directly to the administration or to the trustees of the university. A number have faculty committees on patents, which exist primarily for the purpose of ensuring that pertinent institutional regulations are observed. Often these committees serve as advisory bodies and are charged with recommending action on matters that range from the desirability of taking out a patent to the determination of equities.

There are at least three distinct equities or interests involved in patentable discoveries or inventions resulting from scientific research in an educational institution: the inventor or inventors, the institution, and the general public; to which must be added a fourth, the sponsor or supporter of the research, in the case of sponsored or cooperative research. When further developmental work is necessary, a fifth interest may be involved, although frequently it is the same as the sponsor or supporter of the original research.

The recognition and protection of these several and diverse interests naturally complicate any individual situation. Self-interest, personal rights, institutional policies, employer-employee relations, academic freedom, contractual relations, patent law, business practices, commercial competition, and the variables in individual cases are some of the elements that contribute to the problem. Nevertheless, to be equitable and effective a patent policy must provide for such recognition and protection, placing the responsibility where it can be best discharged, most expeditiously and with the minimum of burden on the regular administrative and teaching staffs of the educational institution.

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SCIENCE ON THE MARCH

PLANT PROTECTION MUTATES

President Truman has called upon Americans to furnish hungry Europe with 200,000,000 bushels of grain, chiefly through eating and wasting less. L. S. Hitchner, representing the insecticide-fungicide industry, has wired the President that this much surplus grain can be made available without any reduction in home consumption, by full-scale utilization of old and new knowledge of the prevention of crop losses caused by diseases, insects, rats, weeds, and other pests.

World War II was a mighty stimulus to the development of new and more potent chemical pesticides. There was an insufficient supply of the old-fashioned chemicals of protection such as copper, mercury, and arsenic; military demands for protection of personnel and matériel in pest-ridden climates competed for these scarce chemicals with civilian demands for greater crop production through pest control.

The fundamental research essential to the development of new chemical pesticides had been quietly advancing in the prewar years. Now came the pay-off, with such developments from the basic research as the revolutionary insecticide DDT, the weed killer 2,4-D, the powerful rat poison ANTU, and a host of other new, potent pesticides.

Less widely publicized, but no less revolutionary, have been the recent developments in fungicides for protecting crops from disease and for proofing fabrics and other organic materials from fungus destruction. More progress on fungicides has been made in the past decade than in the preceding half century. It is the knowledge of the effectiveness of this arsenal of new pesticides that gives agricultural leaders the assurance that Europe's need for foodstuffs can be met and exceeded by pest-control alone.

The basic research that has made possible the development of radical departures in fungicides has centered around the bioassay of chemicals to determine their fungicidal potentialities. Chemical assay is not sufficient; the toxicity of a given chemical to fungi, living organisms, is conditioned by a variety of factors the effects of which can only be determined in direct tests of chemical against fungus.

In the bioassay of fungicides ingenious equipment is used, in which possibly useful materials are sprayed at a glass target under precisely controlled conditions. Spores of a standard strain of fungus are then tested for germination on the sprayed glass, also under controlled conditions, and this work is supplemented by tests in which living leaves replace the glass target. Using such techniques, thousands of possible fungicides can be "screened," and the few promising ones may then be tested for value in controlling fungi under greenhouse and field conditions.

The bioassay of fungicides is not merely a culling out of hundreds of unsatisfactory compounds, selected at random, in favor of a few promising ones. With suitable assembly and analysis of data from the bioassay, it is possible not only to determine that a given chemical is toxic to fungi, but to shed light on the nature of the fungicidal action, the type and purity of the compound, and the nature of interaction of two chemicals in a spray mixture; the bioassay is the key to the dynamics of fungicidal action. Thanks to it, plant pathologists no longer need to test chemicals at random on the chance that one might be useful; it can now be predicted that compounds having certain molecular configurations will be fungicidal.

When the dosage of the chemical is varied, there is corresponding variation in response of the fungus spores. If dosage is plotted against toxic effect on a probability-logarithmic grid, there results the "dosage-response curve," a beautiful tool revealing much about the dynamics of fungicidal action.

If the dosage-response curves of two chemicals intersect *X*-fashion, one compound is more toxic than the second at higher dosages, and the reverse is true at low dosages. If the two intersect *V*-fashion, one chemical loses its toxicity with dilution more rapidly than the other. If the dosage-response curve has two peaks, separated by a valley, this means that with high dilution a fundamental change in the nature of fungicidal action occurs, such as a change from molecular toxicity to ionic toxicity. Comparing the dosage response curves of two chemicals, individually, with the curves of their mixture reveals whether the chemicals in combination reinforce each other (synergistic effect) or antagonize each other. Moreover, the curves in this case give some clue to the nature of interaction of spray ingredients, whether it is simply additive or subtractive, or whether it is "potentiated," altering the mechanism of toxicity. For an extended account of the bioassay, the dosage-response curve, and the methods of action of fungicides, the reader is referred to the recent excellent book by J. G. Horsfall: *Fungicides and their Action* (Chronica Botanica Co., 1945).

For more than half a century, chemical control of plant diseases had been based almost entirely on copper, the toxic ingredient of Bordeaux mixture, and sulfur, alone or with lime. Today, thanks to the basic research centered about the bioassay, these veterans of chemical crop protection are rapidly being forced into the background by new and better organic fungicides. In the 1930s were introduced the organomercury compounds, which for a time monopolized the practice of seed treatment, a practice so successful that it has been compared with hybrid corn as one of the most outstanding agricultural developments of recent years.

In rapid succession during the last decade there have appeared other spectacular agents of crop protection, among them the carbonyl compounds such as Spergon (tetrachloro-*p*-benzoquinone) and Phygon (2, 3-dichloro-1, 4-naphthoquinone), as seed protectants and sprays lacking the poisonous qualities of mercury compounds. Then there are the triocarbamates, sulfur-containing organic compounds as outstanding in the field of plant pathology as their relatives, the sulfa drugs, in medicine, including the valuable tetramethylthiuram disulfide (Arasan or Tersan) for seed treatment and sprays, and such remarkable spray materials as ferric dimethyl dithiocarbamate (Fermate, etc.), zinc dimethyl dithiocarbamate (Zerlate, etc.), and disodium ethylene bisdithiocarbamate (Dithane), which, against many important crop diseases, have already largely supplanted copper and elemental sulfur. Even more remarkable, in some respects, are the new quaternary ammonium fungicides, compounds with 5-valent nitrogen, the most outstanding of which is phenylmercuritriethanol ammonium lactate (Puratized N5), a compound of a double fungicidal character by virtue both of its quaternary ammonium structure and its mercury content, with toxicity to fungi at the extremely high dilution of 1–5 parts per million.

The new organic fungicides are not merely toxic to fungi; many other useful properties have been considered in their development, such as their nontoxicity to plants and humans, their compatibility with other spray ingredients, and their tenacity of adherence to foliage despite heavy rains.

Until recently fungicidal sprays or dusts have been regarded as *protectants*—preventing the infection of healthy tissues but of no use once the tissues have become infected. This classic conception has been upset both by study of the action of new fungicides and reinvestigation of older ones. It has been found that such products as Puratized, sodium dinitro-*o*-cresoxide (Elgetol, Sinox), and even pure copper sulfate are *eradicant,* not only protecting plants but actually inhibiting or destroying established infections.

Parallel with the development of these new seed protectants, sprays, and dusts, has been the appearance of other chemicals of crop protection designed to rid soil of undesirable inhabitants, fungi, nematodes, and weed seed. Already these are being produced at such reasonable cost that they are being used on increasingly large acreages, and this is often followed by extraordinary increases in crop yields.

These soil disinfestants are volatile organic compounds, and their action is essentially a fumigation of the soil. Fumigation, although widely used in insect control, has had little place in protection against plant diseases, but recently it has been found that the "blue mold" diseases of tobacco and cabbage seedlings can be effectively controlled by fumigating the seedbeds with benzene or paradichlorobenzene.

Outstanding as are these developments, they still do not complete the tale of new discoveries in the conquest of plant disease. Plant pathologists have always concentrated their attention on prophylactic measures, aimed at protecting plants, with little interest in trying to cure them once they are diseased—quite the opposite of traditional human medicine. Now a chapter on the therapy of plants is beginning to unfold, with particularly interesting findings on the chemical treatment of trees suffering from wilt, root rot, and virus diseases. In some cases the effect is not directed against the agent of disease but is a neutralization, or "antidoting," of its toxic products. A diversity of chemicals is being used in such studies, even including the new antibiotic drugs, such as penicillin.

Still another chapter might be written on revolutionary new methods of application of pesticides. The crop-protection airplane is becoming commonplace, and ground equipment of radical new types is replacing the old conventional sprayers and dusters. New principles in pesticide application are involved, including the use of a high-velocity air current as a diluent for concentrated dusts and sprays, equipment that applies the chemical in an artificial dew or fog, and the aerosol method where the pesticide carrier is a highly volatile organic fluid. These new developments in pesticide application are greatly decreasing the cost of crop protection in both labor and chemical required per acre.

It is too early to have a balanced perspective of these developments of the past decade and their impact on agricultural economy, but there is little question that we are witnessing the dawn of a new era in crop protection—one that may come to be regarded as significant as the advent of mechanized agriculture.

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SEX IN BACTERIA AND VIRUSES

THE manifestations of sex are known to all, but its biological meaning is clear to few since it can only be understood in terms of evolution. Evolution by natural selection takes place because there are present in natural populations large numbers of different combinations of hereditary characters, some of which have greater chance than others of survival and propagation.

New characters arise by mutations, that is, by rare and unpredictable changes of determinants of heredity, or "genes." One mutation seldom gives rise to an individual better fit for life than the original. Favorable mutations do, however, arise, if only with low frequency. How will they be made use of for the survival and improvement of the species? As pointed out by Muller in 1932, in asexual organisms, that is, in organisms reproducing from one parent only, a mutant gene will be tested for its survival value only in combination with the other genes already present in the line of descendance in which it has arisen. If several such favorable mutations arise in different lines, they will compete with one another and tend to crowd each other out. Combinations of several favorable mutant genes can only be formed if the mutations happen to occur successively within the same line of descent, necessarily a very rare event. Much good material for evolution will thus be wasted.

Sexuality prevents or at least greatly reduces this waste. In the process of sexual, biparental reproduction the genes of one individual have at each generation a chance to be reshuffled with those of another individual, producing a variety of offspring types. In natural populations, these are generally all more or less different from one another and from their parents. Reshuffling occurs because the genes are carried in chromosomes, nucleoprotein threads present in various number of pairs in the nucleus of each cell, each member of a pair being contributed by one parent. The variety of combinations, arising from the random assortment of the members of each chromosome pair, is further increased by exchanges of parts between chromosomes of each pair within the parents (crossing-over) in the course of the formation of the sexual cells, the sperm and the egg. As a consequence, no set of genes of a grandchild is identical with any one of those found in his four grandparents.

Moreover, the very increase in the total number of gene combinations resulting from sexual reproduction is an advantage for the species: if environmental conditions vary, gene combinations that might previously have been unfavorable may prove useful in the new exceptional situation and increase the chances of survival of the species.

Finally, mutant genes that would be a handicap in one combination might prove actually advantageous in other combinations. Sexuality, so to speak, gives them a chance. In Muller's words, "sexuality, through recombination, is a means for making the fullest use of the possibilities of gene mutations."

Since sex is such a powerful factor of survival, evolutionists have often wondered how organisms like bacteria manage to survive at all if they are, as is generally believed, completely sexless and only reproduce by repeated fissions. As pointed out before, the number of available genetic combinations will be quite limited, and the chance for the appearance of more favorable ones rather small. In part, this limitation might be compensated for by the large size of bacterial populations, which allows the occurrence of even rare mutations, so that a number of different combinations might actually be formed within one hereditary line. In the long run, however, bacteria would probably be better off if they had some kind of sex.

In 1946 Lederberg and Tatum announced the results of experiments strongly suggesting sexlike phenomena in bacteria. From one original parent strain of *Escherichia coli,* a common intestinal bacterium, capable of manufacturing all its protoplasmic materials and of growing in a mixture of very simple chemicals (sugar, ammonia, and inorganic salts), these authors first isolated several "nutritionally deficient" mutants, that is, mutants unable to manufacture some essential chemical needed for their growth. These mutants are easily obtained by irradiation of the parent strain with ultraviolet light. A mutant can only grow if the chemical or chemicals which it is unable to produce is supplied in the culture medium. Indirect evidence, mainly the analogy with the situation in the mold Neurospora, had already suggested that each nutritional deficiency is tied up with a modification of one gene.

Let us suppose that the original strain is capable of synthesizing the four substances A, B, C, and D. A mutant $A-B-C+D+$ (deficient for compounds A and B as a result of two mutations, but capable of manufacturing C and D) is grown together with another double mutant with reverse synthetic abilities, $A+B+C-D-$ in a medium containing plenty of all four substances A, B, C, and D. If the cells of the two mutants, while growing together, undergo some type of sexual fusion, exchanges of genetic determinants might occur. Among such exchanges there may be some leading to the formation of cells $A+B+C+D+$. These would be nutritionally identical with the original strain (prototrophic, from *protos,* "original," and *trophe,* "nutrition") and, like the latter, easily distinguishable because able to grow in the absence of all four substances A, B, C, and D.

Strains with at least two deficiencies had to be used to eliminate the possibility of prototrophic organisms being produced by one mutation. The chance of two mutations occurring in the same line is low enough to be completely negligible.

Lederberg and Tatum actually found prototrophic cells present in mixed cultures of a number of different mutants with several deficiencies and interpreted them as proof that genetic recombination had occurred. This conclusion was supported by the fact that, when strains differing by three or more mutations were grown together, there appeared in the mixed cultures some cells that showed all possible combinations of the characters of the two parents, indicating that the transfer could involve different numbers of genetic determinants.

The mechanism of fusion and gene exchange in bacteria is still unknown, and the results may need careful checking before their interpretation as genetic recombinations can be considered as proved beyond doubt. It seems likely that, if sexual phenomena like these are actually proved to occur at all in bacteria, they will be found to be frequent enough to play an important role in bacterial evolution. It would, in fact, seem strange that a mechanism so useful from the evolutionary standpoint, once arisen in a group of organisms, might remain confined to just a few strains of bacteria. The very fact of having been found in one of the few strains carefully tested would suggest its widespread occurrence.

If bacteria may have sex, what about the filterable viruses? This might seem to be asking too much indeed. After all, viruses are ultramicroscopic entities reproducing inside living cells and often thought to be hardly more than nucleoprotein molecules. And yet we know that some of them undergo changes very similar to sex in their genetic consequences. This was discovered in the course of work on bacteriophages, the viruses that reproduce inside bacteria somewhat as the virus of infantile paralysis reproduces in the cells of the spinal cord of man, or the virus of influenza in the lungs.

Bacteriophages can undergo a number of different and independent mutations, and this writer and Hershey have in recent years described a number of them. In 1946 Delbrück and Bailey infected the same bacterial cells simultaneously with two bacteriophages carrying different mutations. As a result of this mixed infection, new types of viruses, different in their combination of characters from those that had entered the cells, were produced. These workers, for example, used two bacteriophages, T2 and T4, somewhat related but serologically and otherwise distinct, which can give mutants called T2r and T4r. These are easily distinguishable from T2 and T4 by the peculiar appearance of the zones of destruction, or "plaques," that they cause in a layer of bacteria grown on solid media. When particles of both viruses T2 and T4r entered the same bacterial cells, the new phage produced inside these cells consisted of four types: T2, T2r, T4, and T4r. It was necessary to conclude that the property designated as "r" had either been induced or else transferred from one phage to the other in the course of reproduction. Clearly, this amounted to the same result as that of a sexual cross, that is, the formation of new combinations of hereditary characters. These results were confirmed and extended by Hershey with the study of several other mutant properties. They can be taken to prove that, whatever the unknown mechanism of virus growth inside the cell may be, it must allow for some kind of reshuffling between the groups of hereditary determinants brought into the same cell by different virus particles.

In addition to this, this writer has recently found that genetic recombinations between viruses can do more than alter properties of the above kinds; they can restore their reproductive ability after this has been lost. Bacteriophage particles were inactivated by ultraviolet rays and, as a result, lost their ability to be transmitted from one bacterium to another without losing that of penetrating the host. Two or more such "inactive" particles were made to enter one bacterium, and active particles of bacteriophage reappeared. We have obtained evidence suggesting that this "resuscitation" depends on the fact that particles that have been damaged by radiation in different parts can transfer portions while inside the same host cell, with an opportunity for the formation of normally active particles. In other words, it seems that a lethal mutation can occur in any one of a number of parts of a virus particle, each of these parts being replaceable by transfer from another virus particle.

It is interesting to remember that more than ten years ago Berry and Dedrick discovered that the virus causing rabbit fibroma could be transformed into the virus of myxoma, a closely related virus of rabbits, by simultaneous injection of active fibroma and inactive myxoma virus. This phenomenon may find its explanation in some transfer of genetic properties similar to that occurring in bacteriophages.

It would be rash to draw from these results any general conclusion as to the origin of sexual processes and their evolution. It is possible that different mechanisms permitting genetic recombination have appeared independently at various stages of the evolution of the biological world and that not all such mechanisms have developed along similar lines. The frequency of occurrence of genetic recombinations is likely to be very low in the case of bacteria and viruses, where reproduction by fission is probably the rule and conjugation between different individuals resulting in gene recombination, when present, is the exceptional occurrence. It is, therefore, possible that mechanisms for genetic recombination that could prove too radical for higher organisms, where they would be repeated at each generation, might work successfully in viruses or bacteria. It would clearly be dangerous to try forcing analogies from the genetics of plants and animals to that of bacteria and viruses until the latter rests upon a more solid factual basis.

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LIST OF AFFILIATED SOCIETIES AND REPORTERS APPOINTED BY THEM TO CONTRIBUTE TO "SCIENCE ON THE MARCH"*

SECTION B (PHYSICS)

American Meteorological Society	Wallace E. Howell
Optical Society of America	Frank Benford

SECTION C (CHEMISTRY)

American Chemical Society	Walter J. Murphy

SECTION D (ASTRONOMY)

American Astronomical Society	Joseph Ashbrook
The Meteoritical Society	John A. Russell

SECTION E (GEOLOGY AND GEOGRAPHY)

American Geographical Society	John K. Wright
	Raye R. Platt
Seismological Society of America	C. F. Richter
The American Association of Petroleum Geologists	M. G. Cheney
	R. K. DeFord
The Paleontological Society	Harold E. Vokes

SECTION F (ZOOLOGICAL SCIENCES)

American Society of Mammalogists	E. R. Hall

SECTIONS F AND G (ZOOLOGICAL AND BOTANICAL SCIENCES)

Genetics Society of America	S. E. Luria

SECTION G (BOTANICAL SCIENCES)

American Society of Plant Physiologists	J. van Overbeek
Mycological Society of America	Lindsay S. Olive
The American Phytopathological Society	K. Starr Chester

SECTION H (ANTHROPOLOGY)

The American Anthropological Association	Frank H. H. Roberts
	Marshall T. Newman
	Weston La Barre

SECTION K (SOCIAL AND ECONOMIC SCIENCES)

The American Sociological Society	Alfred McClung Lee
	Calvert Dedrick
	Abraham Jaffe

SECTION M (ENGINEERING)

American Society of Civil Engineers	Allen Wagner
Illuminating Engineering Society	Matthew Luckiesh
Institute of the Aeronautical Sciences	John J. Glennon
The American Society of Mechanical Engineers	L. M. K. Boelter
	Huber O. Croft
	Carl F. Kayan
	W. Julian King
	Alexander Klemin
The Institute of Radio Engineers	Laurence G. Cumming

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* As of January 1, 1948.

[*Continued on page 182*]

BOOK REVIEWS

THE NEW VISUAL ART

Television Techniques. Hoyland Bettinger. ix + 237 pp. Illus. $5.00. Harper. New York. 1947.

NOW that television is no longer "just around the corner," but with increasing receiver production and sales begins to emerge as a medium possessing individuality and distinctive character, it is logical to expect the appearance of such a book as Hoyland Bettinger's. Mr. Bettinger, former general manager of television station WRGB in Schenectady, writes with authority concerning program production problems in television today.

The first third of Mr. Bettinger's book is devoted to indoctrinating the reader to the medium. He describes the technical studio equipment and explains how it is used, dwells briefly on viewing habits of the audience, and discusses the all-important limitations of time, space, light, and sound pickup. He then launches a chapter entitled Pictorial Composition and Continuity, an exposition, with line drawings for illustration, of the basic principles underlying all visual arts. In presenting these basic principles of composition as they apply to television, Bettinger saves the student a good deal of digging and research and supplies the working television producer with excellent reference material. The next two chapters, which complete the indoctrinary portion of the book, cover Video and Audio Techniques, and contain much valuable practical information.

One of television's serious problems has been its failure to produce good creative writing. If Bettinger's chapter on Television Writing is widely read and understood, the problem may be well on its way to solution. He contends that "the television writer . . . is not one who sits in an ivory tower and writes as he pleases," but rather must create with full consideration for (1) the intimate aspect of television as it is received in the home, (2) the technical and production limitations of the studio in which his script will be produced and (3) the interpretive limitations which time and budget force on his producer. Mr. Bettinger goes on to discuss continuity and playwriting, with suggested forms for scenario and production script.

The rest of the book, with the exception of final chapters on the use of film in television and studio lighting, is concerned specifically with the problems of the producer-director—mainly in relation to the production of a play. Although at first glance one might think Bettinger puts disproportionate emphasis on dramatic production in relation to a balanced program schedule, it is true that the problems to be solved in dramatic production for television occur in lesser degree and in different combinations in almost every other type of production.

Television Techniques, then, is a primer, a sort of textbook, which should be of extreme value to the student and at the same time can serve to remind those working in television of the reasons behind the things they do.

FRANCES BUSS

Columbia Broadcasting System, Inc.

SOCIOLOGY

The Theory of Social and Economic Organisation. Max Weber. A. M. Henderson and Talcott Parsons, Translators. x + 436 pp. $4.00. Oxford Univ. Press. New York. 1947.

IT IS a curious commentary on a country that likes to think of itself as always up to date that the popularity of Toynbee's *Study of History* should date from 1947, whereas his first volumes appeared in 1934, and that Max Weber, the Colossus of Heidelburg, should wait more than a quarter of a century after his death for the translation of essential portions of his work. Many complacent readers feel that anything worth reading gets an English dress sooner or later, but surely this is true only in the very long run.

Now, thanks to the almost simultaneous appearance of Gerth and Mills' *From Max Weber* and the present translation of Part I of *Wirtschaft und Gesellschaft,* both with excellent introductions, excuse for neglect of Max Weber disappears. Still the barrier of mentality is left, and Weber may appear too much the methodologist, may permit himself a ranging over many disciplines that is suspect to the specialist, and may be too exclusively the "technical analyst of social phenomena" to please the factually minded. For the admiring, however—and their number should be increased by Mr. Parsons' meticulous translation and editions—these rare abilities are precisely Weber's claim to a preeminent place among twentieth-century social scientists.

Weber writes with an Aristotelian meatiness that makes it impossible to underline and difficult to select. A reviewer has no choice but to try to select the most essential points.

The procedure he preaches and exemplifies is the formulation of—

> pure ideal types of . . . forms of action which in each case involve the highest possible degree of logical integration by virtue of their complete adequacy in the level of meaning . . . (p. 110). The more sharply and precisely the ideal type has been constructed, thus the more abstract and unrealistic . . . it is, the better it is able to perform its methodological function in formulating the clarification of terminology, and in the formulation of classifications, and of hypotheses (p. 111).

The very subject matter of sociology is an ideal type. For Weber, action is social only when it is "oriented to the behaviour of others." This orientation must be meaningful. This leads him to a comparative neglect of action that is causally determined by the action of others, but not meaningfully. Distinction between the two kinds of social action is of course justifiable, but limitation to one kind is less so—a fact Weber has to admit in the paradox that "sociology . . . is by no means limited to the study of 'social action'" (p. 114).

The temptation to which Weber has yielded in making meaningful social action central to his subject matter is a result of his definition of explanation. If explanation is an emphatic penetration of the subjective meaning of action, then naturally one chooses to study the type of action that has such subjective meaning, even while admitting that it forms but a part of what goes on in human society. Within this narrower field, insistence on *Verstehen* as necessary to render action intelligible is a valuable contribution. Weber rightly confesses that it is only part of the story and that the ability to attribute interest or meaning to an act may give us only plausibility if we do not have in addition some assurance as to probability of the repetition of such acts. Statistics alone is not enough for the student of causality, but neither is an imagined subjective meaning.

Social action may be treated as action oriented to values, as well as toward persons and their opinions. Such values may be absolute and unconditional, pursued without weighing alternatives or costs. Cases of this sort form one extreme limit in the direction of irrationability; the purely rational and calculated is the other extreme, while concrete human action is usually a mixture of the two. One would like to see a fuller discussion of these means and ends, a fuller admission of the fact that even absolute values depend upon the culture of the individual and are subject to the impact of cultural change.

The distinction made here carries over into Weber's discussion of the nature of economic organization, which is essentially rational and calculated, and most so in its capitalistic form. This part of Weber's work, aside from its emphasis on the well-known "Protestant ethic," contains less that is novel.

The treatment of types of authority, with which the volume closes, is wise, closely reasoned, and roams through time and space with magisterial tread.

Authority may rest on rational grounds, which curiously enough are practically equivalent to legal patterns and to the employment of a bureaucratic administrative staff, which consists ideally of those who have the know-how, and which is defended by insisting that the choice is between bureaucracy and dilettantism. Moreover, by opening the way to those who prove their rational and technical knowledge, it is democratic in tendency, as opposed to the authority of those whose claim rests on the other two grounds of tradition and charisma, the gift of grace. Since followers, and the "people" in general, want results, there is a tendency to replace an ineffective tradition or a failing charisma by a more routine but more rational type of authority, or at least to admit an admixture of the other types of authority better adapted to the conditions of economic life, which are always exerting their pull.

In almost any concrete case, elements of rational, traditional, and charismatic authority will be present. In the Federal government of the United States there is, we hope, rationality, certainly tradition, and some may discern the minimal charisma of a Truman.

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SEISMOLOGY

When the Earth Quakes. James B. Macelwane. xi + 288 pp. Illus. $5.00. Bruce Pub. Milwaukee. 1947.

THIS 288-page book is one of the "Science and Culture Series," of which the Rev. Joseph Husslein is editor. It is written by one of the most distinguished seismologists in the world and is aimed for the most part at the general reader. There are 17 chapters and a geological appendix, as well as a list of reference reading and a glossary of terms. In addition to a general discussion of earthquakes, there are chapters on engineering seismology, seismic prospecting, rock bursts and microseisms. The illustrations are profuse.

A general chapter on What Earthquakes Do is followed by a discussion of eighteen particular shocks. These include a shock (Hawaii, 1868) associated with a volcanic activity. Some 73 illustrations accompany these two chapters, including maps of shaken areas and pictures of damage done.

Chapter 3 is entitled Kinds of Earthquakes. Depth of focus is first considered. The author states that "the great preponderance of strong earthquakes seem to originate at depths between five and twenty miles." These are classed as normal shocks. Those originating at depths much greater than twenty miles are called deep. The classification of tectonic, volcanic, and plutonic (deep focus) is also offered. "Earthquakes that involve a sudden deformation of the earth's crust by faulting or warping are called tectonic." Thus, warping as well as faulting is envisaged as a source. Regarding volcanic earthquakes, the author states, "Earthquakes may be associated with volcanoes in three ways: 1) an earthquake may originate in the neighborhood of an active or a dormant volcano; 2) it may occur simultaneously with an eruption; 3) it may be caused by volcanism." Later, he restricts the term "volcanic earthquakes" to those "caused" by volcanism, so it returns to its old form. How we are to decide whether a given shock is volcanic or tectonic still remains uncertain. Some four particular volcanic earthquakes are discussed in this chapter. At the end of the chapter the writer presses for the term "plutonic" to describe shocks the depth of which is "measured in hundreds of miles."

In the chapter Why Earthquakes? there are listed "geological conditions in a few typical places where earthquakes have occurred." Discussion includes grabens, rifts, thrust zones, ocean troughs, faulting, and warping. Under ultimate causes mentioned are cooling of the earth, isostasy, thermal cycles, migration. The elastic rebound theory is discussed. The geodetic data used by Reid are given in a table. The map (Fig. 112) is mislabeled "The amounts and direction of actual movement in the California earthquake of April 18, 1906." The data on this map are the same as those in Figure 7, *Special Publication 151*, U. S. Coast and Geodetic Survey. The arrows show changes in position of triangulation stations, holding fixed Lospe, Tepusquet, Mount Helena, and Monticello, between surveys made before 1900 and surveys made between 1922 and 1925. According to the elastic rebound theory, these displacements include slow drift (strain) as well as earthquake displacement.

A chapter on the question of earthquake prediction holds out hope of prediction in time only on the basis of accurate geodetic observation of accumulation of strain in the earth's crust. As to where earthquakes are most likely to occur, more can be said with the statistics accumulated by seismologists.

After a chapter on field study of earthquakes, there is one on seismic sea waves. Figure 125 is most interesting: that of a tunami breaking on a beach. One wonders what happened to the photographer shortly after he snapped the picture. Perhaps the picture was a telephoto. The next chapter on earthquake sounds presents them as somewhat of a mystery. The author considers some earthquake sounds as traveling a large part of their path in the air rather than all entering the air from the ground almost directly under the hearer's feet.

Chapters 9 and 10 discuss seismographs. There are no equations in the whole book! Particularly in Chapter 9, the reviewer feels that the subject is complicated rather than simplified by this omission. One could wish that in the definitions of dynamic and static magnifications of a recording pendulum on page 145 the author had in each case given the reference system from which the displacements mentioned were measured; "displacement of the support" and "displacement of the mass" are relative to different systems.

One is impressed with the large number of pictures of seismographs and the detailed word descriptions, coupled with the lack of diagrams and equations. To the reviewer these descriptions were most informative—he learned details he did not know.

In the next two chapters is a summary of engineering seismology and measurement of artificial vibrations. Then a chapter on rock bursts is followed by one on prospecting. This chapter is well supplied with diagrams. One is interested in the author's use of head wave (after Schmidt's *Kopfwellen*) for that portion of the refracted wave after the ray has emerged from the lower or refracting medium.

The last chapter outlines the development of the use of microseisms to detect storms at sea, one of seismology's latest advances.

The book is an excellent one and well worth the reading by the seismologist as well as the general student for whom it was written. Any views of Father Macelwane are always well considered and based on his vast knowledge and experience in the field of seismology.

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HERPETOLOGY

Amphibians and Reptiles of the Pacific States. Gayle Pickwell. 236 pp. Illus. $4.00. Stanford Univ. Press. 1947.

THIS book supersedes all others treating of the area covered. It is the final word, to date, on the herpetology of Washington, Oregon, and California; a necessity for beginners in an interesting field; and a "must" for the technical student. It is refreshing to find an author so gracious in bestowing credit on his collaborators as Dr. Pickwell proves to be in his Preface and throughout his book.

A chapter each is devoted to a unique running account of the systematic arrangement of the amphibians and reptiles. These chapters are "readable"—interestingly so—for anyone. Next follow two chapters on life habits, of which so little is known or mentioned in the average textbook. All authentic information on homes, food, growth, reproduction, and enemies is interestingly portrayed.

Chapter 6 is devoted to methods of collecting, care in the laboratory, and preserving of speciments.

Eighty-five plates and figures are so lifelike that even a person only vaguely interested in herpetology will enjoy studying them.

The Appendix, besides containing unfailing keys for determining adult specimens, also contains keys for the larvae and eggs of amphibians. This innovation will be appreciated by all who desire to work with amphibia. Dr. Pickwell concludes with an ample Glossary, Bibliography, and Index.

This book will do much to stimulate interest in natural history and a more sympathetic understanding of the lower orders of life, besides kindling a desire for field study. The end result will amply repay Dr. Pickwell for his years of painstaking work, the credit for which he has so generously shared with those who helped him write this book.

CHAPMAN GRANT

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DICTIONARY

English-Spanish Chemical and Medical Dictionary. Morris Goldberg. 692 pp. $10.00. McGraw-Hill. New York. 1947.

THIS new English-Spanish technical dictionary contains more than 40,000 current terms used in medicine and allied sciences. A work of this nature implies a great deal of effort on the part of the author, a very worthy effort in view of the existing need for works of this kind. Nevertheless, careful review of the Spanish text discloses some unavoidable mistakes: some are typographical errors, and others are due to inexact interpretation. For instance, in the case of the word "bloodshot," the Spanish equivalent is given as *menchado* instead of *manchado;* "cocoa butter" is given as *Theobroma caco* instead of *cacao;* in explaining *hemólisis* it is superfluous to say "disintegration of the red blood cells" when of course these are only found in the blood; the translation of "inbreeding" is given as *consanguinidad,* which means only the result of closely related mating; *verbio* is used instead of *verbo;* "Adam's apple" is not *bocado de Adán* but *nuez de Adán*—to mention only a few of the instances and not discussing some quite debatable definitions as given in the Spanish text for "antigen," "antivirus," "malaria," "crucible," etc. In view of the great value of such a dictionary, a copy of which should be in the hands of every worker or investigator using both these languages, it would be advisable in preparing a new edition to have the Spanish text proofread by qualified physicians and scientists who have a command of the language of Cervantes.

OSCAR VARGAS

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EXTRASENSORY PERCEPTION

The Reach of the Mind. J. B. Rhine. 234 pp. $350. Sloane Associates. New York. 1947.

IN THE opinion of this reviewer, Rhine's latest book on extrasensory perception (telepathy, clairvoyance, precognition) may be summarized by quoting a sentence appearing on page 44: "If we are not extremely vigilant we tend to find merely what we are seeking."

The history of the development of the scientific method is full of examples of errors in observation and recording produced by experimenters whose objectivity was colored by a driving desire to prove a hypothesis. Apparently, the field of psychical research has primarily attracted enthusiatic believers and only secondarily objective observers who have, by scientific training, learned enough about sources of error in human observation to use the standard instrumentation and experimental method devised over the centuries to secure objectivity.

The first chapter of Rhine's book is devoted to a review of the mind-body problem. In it, the author espouses the "psychocentric," or mind-centered, point of view as a point of departure for reviewing again the history of attempts to establish telepathy and clairvoyance as real human capacities.

The new experimental material of this book appears in Chapters 5 and 6, devoted to precognition (seeing into the future) and psychokinesis (the alleged capacity of the "mind" to affect physical objects). In Chapter 10, the author asks the question, Why is not such evidence accepted at once by the scientists? and suggests that fear of the unorthodox is the explanation. This reviewer believes that there is a more pertinent explanation, namely, that these researches have persistently violated the rules of the scientific method to ensure objectivity in the collection of data.

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PSYCHOLOGY

Hypnotism Today. Leslie M. LeCron and Jean Bordeaux. ix+278 pp. $4.00. Grune & Stratton. New York. 1947.

THE average person does not know much about hypnotism, or he may know nothing at all about it. Yet he is apt to have some very definite notions concerning it. To him, it is mysterious, vaguely tinged with an aura of quackery and of the occult, and, in general, rather anxiety-provoking. Withal, however, he is likely to be interested in the subject and would welcome an opportunity to learn something of it. Unfortunately, the literature of hypnosis is voluminous and full of claims, counterclaims, polemic, and verbiage. What is needed is a sound, careful presentation of the material, which is firmly rooted in the evidence; such a presentation could be recommended to the interested layman. But few if any books on the matter can be so recommended.

Hypnotism Today is a book written in a reasonably nontechnical vein, and it was prepared, in part, to serve the need for a survey of the field. LeCron and Bordeaux are practicing hypnotherapists, and they succeed in removing much of the mystery and magical quality from their subject. Experimental and clinical evidence is cited in abundance, and it is not the authors' fault that clear-cut answers are unavailable to many of the problems that have been investigated. Throughout, the authors emphasize the need for more research, a very healthy emphasis indeed. Their book is recommended by Milton H. Erickson, himself an authority in the field.

I feel that, on the whole, the presentation of the major facts, methods, phenomena, and theories of hypnotism is satisfactory. The material could be somewhat better organized than it is, and more space should probably have been given these topics. As it is, one has the impression that this part of the book was written in haste, or else that too much has been crammed into a very limited space. The naive reader may have some difficulty in following the account for this reason.

The second half of the book deals with hypnotherapy. This was written, apparently, because the writers recognize the need for brief forms of psychotherapy, and they hope that their discussion of hypnotherapy will aid in showing its value as a means of brief psychotherapy. One can do nothing but support their aim and their hope, but I feel that the whole book has suffered because of the amount of material it encompasses. The presentation of hypnotism has the shortcomings outlined above probably because the account of hypnotherapy required space which might otherwise have been devoted to it.

The presentation of hypnotherapy, in the reviewer's opinion, is not so satisfactory as the account of hypnosis. So much is covered —abnormal psychology, psychotherapeutic methods, hypnotherapy itself—that each topic is treated very briefly. Technical points are raised and controversial issues are debated in this section, which, I am afraid, will leave the inexperienced reader somewhat confused.

One can recommend the first half of the book—on hypnosis—to the general reader. For the second half—hypnotherapy—more preparation is needed than he is apt to have. LeCron and Bordeaux should have doubled the length of their book—or, better still, have written two books, one on hypnotism and one on hypnotherapy.

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TOTAL CONCEPT

Personality. Gardner Murphy. 999 pp. $7.50. Harper. New York. 1947.

LIKE a general making an all-out assault upon a military objective, so Gardner Murphy here attacks, with multipronged approaches and techniques, the problem of personality and lays siege to it through 999 pages. The resulting study of personality, its development, structure, and meaning, provides one of the most comprehensive treatments of this problem yet to appear.

Dr. Murphy's approach to personality is made, he explains, "chiefly in terms of origins and modes of development on the one hand, interrelations or structural problems on the other." Recognizing the impossibility of doing justice at the same time to "the quantitative problems revealed by psychometrics, by factor analysis, by ratings, and by questionnaires, or to personality tests or therapeutic and educational problems," he chose to formulate a working conception of personality rather than to define in detail the infinite variability of personalities. Materials referring to diagnosis, to therapy, or other clinical approaches to a specific personality are, therefore, excluded. The understanding of a specific personality can best be achieved, Murphy believes, through a general consideration of personality as such, much as an understanding of the peculiarities of a particular radio proceed from a general knowledge of radios. Thus he has fashioned his book into a *general* psychology of personality.

The leitmotif by which he unifies the wealth of material presented is the view that "every aspect of personality is conceived in terms of the molding, the complication, the interpretation, the concealment, the indirect expression of motives." Beginning with the biology of motivation and proceeding, through a consideration of the learning process (which itself shows dependent relationships to needs), to more complex human motives, and to the symbols, values, and goals that serve as a measure of personality, Dr. Murphy considers such topics as the development of perception, of memory, of imagination, of creativeness (genius); the origins and development of the self; personality structure and personality deviations; and the role of social and cultural determinants of personality.

The scope of this book cannot be indicated, however, by an enumeration of topics, for Dr. Murphy's erudite presentation involves the coordination of data from such fields as anthropology, sociology, biology (genetics, embryology, physiology), education, history, literature, and psychology. In addition, he evaluates and indicates the relationship between experimental, biographical, and clinical findings. Because of this interrelating of various fields the book should prove of value not only to psychologists, but to anthropologists, sociologists, and any others interested in human behavior. It should do much to assist in the cross-fertilization of the various disciplines concerned with the behavior of man.

In the interest of readability, no documentation occurs in the text. All references appear in a separate Index, referring back to page, paragraph, and line. The book has a 749-item Bibliography and a 20-page Glossary.

Dr. Murphy is chairman of the Department of Psychology at the College of the City of New York and editor of the psychological textbooks published by Harper and Brothers.

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SCIENCE ON THE MARCH

Science in Progress. George A. Baitsell, Ed. xv + 353 pp. Illus. $5.00. Yale Univ. Press. New Haven. 1947.

THE Sigma Xi National Lectureships which, with an introduction by Frank B. Jewett, comprise this fifth series of *Science in Progress,* are rather unique among general reviews of contemporary science. The record of advance is described here with clarity and authority, but without concession to the conception of science that pervades most popular reviews. Furthermore, these essays are not restricted to recent experimental results, for science is also presented in progress with respect to the emerging interdisciplinary ideas and tools that will determine experimental emphases for decades.

Dr. Jewett, former president of the National Academy of Sciences, introduces this volume by considering some "broad underlying factors in social affairs, which are destined to influence profoundly the future of all scientific research in the postwar years." His point of view is catholic, and the problems he weighs include the support of science by government, the effect of the war and the atom bomb, and the present dearth of scientific manpower.

In the first review paper, "The Interior of the Earth," James B. Macelwane lucidly reports seismological evidence indicating that the earth consists of "a layered crust underlain by a shell some 600 miles thick, under this an intermediate shell some 1,100 miles in thickness, and at the center a core or nucleus with a little more than half the diameter of the earth." In the following paper, "Development of the Betatron and Application of High-Energy Radiation," Donald W. Kerst tells how the idea of accelerating electrons by magnetic induction gradually developed over a period of twenty-five years and reached a fruition in the betatron and in the applications of the betatron to therapy, metallurgy, and fundamental physics. Hugh S. Taylor then presents the evidence that led scientists in 1940 to "minimize all emphasis on distortion of crystals, abnormal dimensions of lattices, extra-lattice aggregates of atoms with abnormal activity" and regard instead "the catalyst surface as nothing other than the normal faces of particular catalytic crystals." This article, "Contact Catalysis Between Two World Wars," also contains a record of major technical achievements in the period 1919–45.

The fourth paper, "Fundamentals of Oxidation and Respiration," by L. Michaelis, is an exposition in a stimulating style of the postulate that, in oxidation-reduction reactions, "only a single electron can be transferred at a time and that any bivalent oxidation consists of two successive univalent oxidations." Michael Heidelberger then reviews the present understanding of complement, "its intensifying and reinforcing action on some of the immune mechanisms, its important function as the drudge of the diagnostic laboratory, and something of its curious chemical complexity and extraordinary lability."

Within a relatively short period the general problem of the mechanism of inheritance has become accessible to biochemical investigation. In his article, "Genes and the Chemistry of the Organism," G. W. Beadle presents experimental results bearing upon this development and shows why it is now reasonable to regard single genes as inheritable controllers of single enzymic reactions. The article by Peyton Rous, "Concerning the Cancer Problem," is an exceptionally interesting assembly of evidence related to carcinogenesis and concludes with a critical analysis of current hypotheses.

"Mutation and hybridization proceed on a colossal scale among plant pathogens." In his paper, "Plant Diseases are Shifty Enemies," E. C. Stakman explores the important practical consequences of these phenomena. C. C. Speidel shows some "of the characteristic activities of cells, recorded directly from living animals by fast motion cine-photomicrography" in his account of "Living Cells in Action." The last paper, "Recent Advances in our Knowledge of the Anterior Pituitary Hormones," is by Herbert M. Evans. This article describes the discoveries that followed the development of nonfatal procedures for surgical removal of the anterior pituitary body. These include the biological, physical, and chemical characterizations of four pure, or purified, proteins from this source, having hormonal activity.

Without exception, the authors of this volume have documented and explained their complex topics in direct and scientifically honest terms. Each article succeeds in evoking an active interest in its subject, and the fifth series of Lectureships holds in every way to the remarkable standard set by the previous volumes.

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NOMENCLATURE PROBLEMS

The Geographical Names of Antarctica. 253 pp. Free. U. S. Board on Geographical Names, Dept. of the Interior. Washington. 1947.

OF ALL the areas in which confusion prevails over the spelling and proper use of geographical names, the polar regions, perhaps, show the greatest variation. In its Special Publication No. 86, the U. S. Board on Geographical Names deals with some of the problems of Antarctica. For instance, in many cases where explorers, in an excess of modesty, have avoided giving their own names to geographical features and have attached names of close relatives, the B.G.N. has thought it preferable to drop given names and thus honor the actual discoverer.

Appropriate generic terms have been assigned, and the B.G.N. has tried to find geographical delimitations. For example, the name "Marie Byrd Land" has been extended to apply to the whole icecap and its coasts east of Ross Sea and as far as it seems to retain the same character; i.e., to Eights Coast. In the interior, the name "Ellsworth Highland" has been given to the strip along the route of Ellsworth's trans-Antarctic flight, replacing "James W. Ellsworth Land." The latter had been artificially delimited by meridians, and comprised much unexplored land; it included also areas now under the designation "Marie Byrd Land."

We cannot expect immediate or general acceptance of such changes, even though they are based on sound geographical principles. We consider it, for instance, a well-founded suggestion to name the eastern and western part of the coast between 74° E. and 59° 40′ E. Mac-Robertson Coast and Lars Christensen Coast, respectively, with Cap Bjerkö a well-chosen dividing point between them. But why should Australians and Norwegians accept this decision? For that matter, when we *decreed* that Graham Land was henceforth to be Palmer Peninsula, the Chileans adopted not our objective, but our method, and *they* decreed that the region should be renamed in honor of O'Higgins; the Argentinians are about to follow suit with still another name.

It seems to me, also, that the wisdom of now translating everything into English is questionable. "Guardian" Islands may be the translation of Øygarden, the name given by the Norwegian explorer, but it will hardly be recognized as such by everyone. If other countries follow the same policy, the confusion will soon be greater than ever. It might have been better to *recommend* the use of such names both in English and in the language of the explorer.

The work on these antarctic names has resulted in an incidental, but for some users an even more important, contribution; that is, a compilation of a comprehensive list of expeditions. Minor disagreements over just which sealing or whaling expedition ought to be included do not detract from the great service rendered by this compilation.

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PUBLIC HEALTH—100 YEARS AGO

Duncan of Liverpool. W. M. Frazer. 163 pp. 8*s*. 6*d*. Illus. Hamish Hamilton. London. 1947.

DURING the middle portion of the nineteenth century Liverpool might have been considered a continental Singapore. Its principal activities were concerned with maritime operations, and its population consisted to a great degree of foreigners. It grew rapidly, and housing became scarce, with crowded living conditions and poor sanitation. The people became tough, illiterate, and drunken—an ideal medium for the propagation of the diseases brought into the city from other countries on ships.

In 1847 a new office was created to cope with this situation, and Dr. William Henry Duncan became the medical officer of health for Liverpool, the first public-health officer of this type in England. He was courageous and ambitious enough to create an administrative machine that could carry out sanitary reforms and thus greatly raise the standards of health of the community.

This small volume has been written by the most recent successor to Duncan as medical health officer of Liverpool on the hundredth anniversary of the creation of the post. It is compiled principally from public records and letters and contains almost nothing of the personal life of Duncan. It suffers somewhat from lack of spontaneity and will be of interest chiefly to those who seek information on nineteenth-century social and medical conditions in a cosmopolitan city.

Richard B. Berlin

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PRACTICAL HANDBOOK

Birds of Malaysia. Jean Delacour. xiv + 382 pp. Illus. $5.00. Macmillan. New York. 1947.

FROM forty months of bird study, observation, and collecting on seven expeditions to Indo-China, which is northeast of, and adjacent to, Malaysia, and a thorough analysis of the scattered and bulky literature, Captain Delacour has created a practical handbook of Malaysian birds. The simple keys, short descriptions with numerous figures, geographical distributions and relationships, and the hints on habits make this volume particularly valuable to persons interested in birds but without special technical training. Heretofore, only those familiar with the literature or having personal knowledge of the region have had such information.

Malaysia includes the larger islands of Java, Sumatra, and Borneo, the Malay Peninsula, the Palawan group, and the numerous small islands that surround them. The Malaysian subregion consists of five provinces, Malaya, Sumatra, Borneo, Java, and Palawan. The main features of each are included in this volume largely as quotations from F. N. Chasen's *Handlist of Malaysian Birds*. This 600,000-square-mile area, situated astride the equator and characterized by a diversity of climates and altitudes, possesses one of the richest exhibits of bird life in the world, both in number of species and in their specialization.

Delacour's imposing list of birds, excellently illustrated in line drawings by Earl L. Poole and Alexander Seidel, includes such unusual creatures as the Great Argus, the hornbills, and many less famous but equally interesting avian species. All forms of Malaysian birds (except the sea birds and shore birds) known to be valid are sufficiently described for identification by observation. Anyone particularly interested in these groups is referred to *Birds of the Southwest Pacific*, by Ernst Mayr (Macmillan, 1945), and to *Birds of the Ocean*, by W. B. Alexander (Putman, 1928).

This volume follows the pattern adopted for *Birds of the Philippines* (Macmillan, 1946), written by Delacour in collaboration with Ernst Mayr, and whenever possible numerous passages have been reproduced or adapted to make the use of both works easier.

The English vernacular names are those used by Chasen, except when such names were considered to be misleading. Native names have been avoided, and no attempt has been made to give vernacular names to subspecies. These may be easily distinguished by geographical adjectives. Scientific names have, of course, been employed to express precise identities. Nomenclature is generally that used in Chasen's *Handlist*.

Of particular interest is the brief section on Hints to Observers. The author, recognizing that too little is known of most Malaysian birds, encourages bird students and others to add to existing knowledge of these forms at every opportunity.

Birds of Malaysia, Captain Delacour's second contribution to "The Pacific World Series," abundantly fulfills its author's desire to produce a practical handbook, succinct, but complete and up to date.

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COMMENTS AND CRITICISMS

NATIONAL DOG WEEK

Too late for mention in my article on "The Compleat Antivivisectionist," I learned of the establishment of National Dog Week, Inc., to reward the outstanding research contribution of the year to the welfare of dogs (*Science*, 1947, 106, 389).

This strikes me as a most happy inspiration. Because it is based on sentiment, it may expect to win friends for science who judge by their feelings and who can never be won by the hard facts scientists are accustomed to cite in their own defense. Antivivisectionists are characteristically not swayed by logic but by sentiment. Having said some hard things about the ineptness of scientists in public controversy, I hasten to salute this rare instance of the sympathetic employment of the moral values of the opponent. Let us hope that by meeting them on their own ground the scientists will succeed in disarming and winning their opponents.

HELEN MACGILL HUGHES

Chicago, Illinois

RACIAL INTELLIGENCE

It was with amazement that I read Professor Garrett's dissertation on "Negro-White Differences in Mental Ability in the U. S." (October SM). Quite properly, at the outset, Professor Garrett recognizes that while it is ". . . difficult to obtain a fair comparison of American Negroes and American whites, this can be done if proper precautions are taken. Inferences drawn from many comparisons, however, must of necessity be tentative." With these amenities out of the way, Professor Garrett proceeds utterly without caution to conclusions which are anything but tentative.

Professor Garrett's thesis is that differences in mental ability between Negroes and whites are attributable to race differences and not to environment. (In fact, he adduces that the diminishment of the differences in mental ability is to be accounted for by admixture of white ancestry to the American Negro.) In analyzing McGraw's study, the author states that the subjects of this study were babies ". . . all living in a Southern community . . . the white babies were slightly taller and somewhat heavier than the Negro children. . . . The average Negro parent reported six grades of schooling . . . the average white parent reported high-school training. . . ." To emphasize the equality of environment Professor Garrett adds that ". . . many [Negro parents] reported college and normal school attendance . . . [and] many [white parents] had attended only the elementary school." The author then concludes that "environmental influences were minimized, if not eliminated." Would that society could minimize the influence of environment on the Negro as easily as does Dr. Garrett!

I am sure that the author recognizes that even if the duration of school attendance were equal for Negroes and whites, the equality would end there. Does Professor Garrett believe that the influence of environment is only that of school attendance? Can his analysis be considered a scientific recognition of the effects of environment?

Later in his paper, after showing that Northern Negroes scored higher on an Army test than Southern whites, the author argues that ". . . given better schooling the Negro does indeed improve his score—but not his position relative to the white"! Relativity was never so obtuse!

An objective appraisal of the environment of Negro and white even in Northern urban areas reveals the gross inequality in housing, employment opportunities, social security, and education, especially higher education. That, as Professor Garrett states, approximately 25 percent of Negroes do better than the average white is testimony to the courage and perseverance of the American Negro in his attempt to achieve the status of full citizenship despite the many "unnatural" handicaps placed in his way.

Perhaps Professor Garrett's most unfortunate premise, however, is that ". . . Negroes and whites have lived side by side in our country for more than three hundred years." Not only have they not lived side by side, they were not permitted to die side by side in our last two wars for democracy.

DANIEL A. OKUN

Cambridge, Massachusetts

In the October issue, Dr. Henry E. Garrett undertakes to discuss Negro-white differences in mental ability without racial bias; the honest psychologist "does not *care*," says Dr. Garrett, "which race (if any) is the more intelligent."

After this laudable introduction, the nature of Dr. Garrett's unbiased and unprejudiced discussion comes as something of a surprise. On page 332, for instance, he refers to the "shiftless and ne'er-do-well Negro (of presumably low intelligence). . . ." To a Southern plantation owner, this may seem like a scientific description, but I hardly think that the average psychologist would agree, and I may be pardoned for suspecting that a bit of racial bias has crept into Dr. Garrett's science after all.

An examination of his arguments confirms my suspicions. He begins with a 1931 study of Negro and white babies two to eleven months old. "This study is valuable," says Dr. Garrett. Let us consider its value point by point.

1. The sample is small, admits Dr. Garrett. Having made this concession, he blandly ignores it, and draws conclusions as if the sample were large.

2. "Social influences are minimal if not completely absent at these early age levels." Social in-

fluences are now known to have a great effect at these levels. I need mention only one factor, the influence on the infant of direct contact with the mother. Negro mothers are much more frequently forced to leave their homes to support the family, and Negro children suffer from this lack of maternal care.

3. The difference in "D.Q." average was 13 points —trivial, in view of the small sampling, even if other objections are not taken into account. Dr. Garrett draws great conclusions from it.

4. White children had better nutrition, but this, says Dr. Garrett, cannot explain entirely that enormous difference of 13 points. How does Dr. Garrett know? It is impossible at so early an age (compare Gesell, for instance) to differentiate with any sharpness between intelligence, motor ability, memory, etc. Nutrition affects at least motor ability, and therefore the D.Q.

5. Negro and white children react differently to a white psychologist. The importance of the manner of approach to children to be tested, their possible emotional disturbances, and so on are factors now generally recognized by psychologists. Dr. Garrett speaks as if these factors did not exist.

6. The I.Q., D.Q., etc., of children in the first few years of life—and certainly of the first few months —show low correlation with I.Q. results in later life. D.Q. has therefore little to do with Negro-white mental differences.

7. The I.Q. itself is now known to vary with the conditions of the test, the training of the subject, the state of his emotions, his interest in the test, or lack of it, and other factors. An I.Q. test drawn up for American children gives misleading results for British, an I.Q. for city children misleading results for rural, and so on.

It is odd that at this late date Dr. Garrett tries to squeeze support for his statements from the old Army Alpha tests, whose deficiencies are now so generally recognized. Some of the same objections mentioned above apply to them as to the tests on children.

There are other peculiarities in Dr. Garrett's article. Notable among them is the statement that the American Negro "has been exposed to the same manners, customs, and environmental influences (schools, churches, movies, etc.) as the American white." Not to the same schools, certainly, in Dr. Garrett's native Virginia; nor, except for a few individuals, to the same colleges anywhere. Possibly to the same religion, but not to the same churches; sometimes to the same movies, but from different seats—and the environmental influence varies with the seating arrangements.

These factors may seem trivial to some readers. But they affect the entire emotional life of an individual, and hence his intelligence as we know it. When attention is centered on emotional needs, the I.Q. shows significant changes. For instance, Dr. Bernardine Schmidt, working with so-called feeble-minded children, has raised I.Q.s tremendously, many children earlier classified as imbeciles or low-grade morons being improved till they came within the normal range. Similar results have been found by others.

In short, for Dr. Garrett's conclusions, I should substitute these:

1. Environment has a tremendous influence on mental ability.

2. Negroes and whites in the United States live in different social and physical environments.

3. Until this factor is fully taken into account, no study of Negro-white mental differences will be worth the SCIENTIFIC MONTHLY paper it is printed on.

JOSEPH SAMACHSON

New York, New York

THE MUDFOG ASSOCIATION

On looking over the "Headquarters Assignments" for the Chicago Meeting in a recent number, I am reminded of Charles Dickens' contribution to the reporting of scientific meetings: his reports on the meetings of the Mudfog Association for the Advancement of Everything.

Admittedly, the wit is a bit heavy and labored; admittedly, Dickens is carried away by his hostility toward the sciences. But the section headings are funny, and the episode of the pug dog closely parallels actual occurrences during my undergraduate struggles to obtain specimens for the study of the anatomy of the cat. His parody of the eager reporter trying to build a sensational story out of nothing indicates that the past hundred years have not brought about any change in this type of activity.

Members of the A.A.A.S. unfamiliar with the *Reports* might be glad to make their acquaintance. They are a pretty good antidote for pompous writing on scientific subjects; they even make one think a bit as to whether a pet research project has real value.

J. D. REICHARD, M.D.

Staten Island, New York

THE MIMA MOUNDS

I read with a great deal of interest the article in the October number by Dr. Scheffer, "The Mystery of the Mima Mounds."

Excepting the gopher theory, I have heard all the other explanations he says have been advanced for the origin of these mounds and some others that he did not mention, including buffalo wallows, beaver mounds, prehistoric elephant wallows, and that they were made by shovel-nosed sharks when the land was submerged. The last theory was advanced by an oil driller who had found shark's teeth in the area around Bakersfield, California, where these mounds cover hundreds of square miles. The gopher theory is new to me, but I think I can disprove it.

First, gophers do not work in a manner that would tend to form such mounds. The University of California found at its cattle experiment station near Fresno that gophers, when fenced off from their natural enemies so that they are unmolested, work the soil quite evenly, so that each year the mounds of soil they pile up tend to make a sort of summer fallow job of cultivation. This cultivated soil produces a better crop of grass the following year, and hence gophers do not deplete the soil. They cultivate one part of an area one year and another part the next. They do not move the soil or other material toward a central point.

Second, gophers and squirrels will not live in these mound areas, commonly called "hog wallows," if there is any other more suitable terrain, the reason being that these mounds provide their natural enemies, coyotes and foxes, with a good cover for close approach. It is not nature's way—that gophers could have survived for long ages by building good cover for their enemies.

Dr. Scheffer mentions these mounds being found near San Diego, California. This is true. They are found on most of the coastal mesas from San Diego northward into Orange County and southward into Lower California. San Diego County also provides examples of these mounds under different conditions and different terrain. They are found in a number of inland valleys with narrow canyon outlets and at elevations of 200–4,500 feet. El Cajon Valley near San Diego is about 200 feet in elevation; San Marcos Valley, 350 feet; Ramona Valley, 1,500 feet; and Mendenhall Valley, 4,500 feet. All these inland valleys in San Diego County have one thing in common: they have narrowly restricted drainage outlets. The great Central Valley of California has the same characteristics, only on a larger scale.

It is my contention that these mounds are gigantic ripple marks made by deep water flowing slowly out of these valleys through the narrow openings. The same thing can be seen on a small scale in any tidal basin where the outgoing tide leaves mud flats. The size of the ripple marks on these mud flats will be found to vary with the depth of the water and the rate of flow. The deeper the water, the larger the mounds, provided there is a wide expanse of flats where the water begins to flow faster in channels; then the bottom will be smooth or in ridges paralleling the direction of flow.

If we could view the floor of the ocean between Florida and Cuba where the Gulf Stream moves quite swiftly (for an ocean current), then we might see such giant ripple mounds as we now see on dry land. Again, if we could fill any of the valleys mentioned above, or the great Central Valley of California, with muddy water several hundred feet deep and allow that water to drain out through its narrow outlet to the sea, we would get the same sort of mounds we see there today. A scale model of one of these valleys should prove this point. The ripple mounds in these valleys are never found on the steeper slopes or in the channels where the current moved too swiftly but on the gentle slopes and flats where the current was gentle.

I agree with Dr. Scheffer that these mounds may be found on any type of substrata, glacial gravel, basaltic rock, clay, hardpan, etc. This proves that their origin had nothing to do with ground water or drainage. He asks how widespread are these "gopher mounds of the Mima type?" I have not heard of their existence elsewhere in the world, but if I am correct in my theory of their origin, they should be found all over the world wherever the land was suitable in topography for their formation.

Allan O. Kelly

Carlsbad, California

The solution to the "mystery of the Mima mounds" proposed by Victor B. Scheffer in the October 1947 issue may indeed seem farfetched to the casual observer of this magnificent microrelief. However, to a soil scientist who has worked intimately with the problem for the past two and one-half years, the rodent theory is not only credible, it seems to be the only possible explanation.

In Merced County, California, in the "hog-wallow" country of the San Joaquin valley is an area of exceedingly ancient hardpan soil known as Redding gravelly loam, covered almost continuously by Mima-type mounds ranging 3–6 feet in height above the intermound level. This area extends over more than 20 square miles, and when one contemplates the fact that there are about 15 mounds per acre, or nearly 10,000 mounds per square mile, the amount of work expended in this one small area is seen to be tremendous. To attribute this colossal earth-moving feat to small rodents certainly appears ridiculous at first glance.

However, closer observation reveals currently active ground squirrels that have mounds as large as 40 feet in diameter and 10 feet in height. And when one considers that this treeless grassland area has teemed with literally millions of rodents for a period certainly extending far back in the Pleistocene era and possibly further, the amount of work required of each rodent shrinks to plausible dimensions. (Some may question the time period mentioned, but the soil described rests upon an almost undissected alluvial terrace plane, which is the oldest and highest of a series of six or seven terrace levels, the most recent of which already has a hardpan soil of advanced age developed thereon.)

The evidence presented by Dr. Scheffer concerning the occurrence of mounds on shallow soils, the lack of orientation (on level areas), the lack of continuous surface drainage, the accumulation of large cobbles in the intermound areas, the mound "roots," have all been observed in this area. However, there are two contradictory bits of information that should be added to the problems yet unsolved.

In the Merced area, the mounds occur very definitely on the slopes of the dissected parts of the terrace plane. Aerial photographs reveal that on the slopes the mounds are oriented into definite almost parallel rows running directly down the slopes. This does not preclude the rodent theory, of course, but, to include this in the rodent theory, the effect of the slope or moisture conditions on "rodent orientation" must be considered.

A more difficult problem, one less reconcilable with the rodent theory, is the occurrence of externally similar mounds upon softly consolidated sediments of the Sierran andesite period. Here the surface of the softly indurated bedrock follows closely the surface of the mounds, the depth of soil being fairly constant at 8–15 inches and the height of the mounds varying from 24 to 36 inches or more. In this particular instance, rodents do not appear to have played any large role in the mound formation; some form of erosion or weathering process seems to be the only possible explanation for this phenomenon.

However, this is a special case, and in no way should reflect upon Dr. Scheffer's able presentation; the writer concurs heartily with the rodent theory as the explanation for *most* of the Mima mounds, or "hog wallows." I believe it significant that a biologist and a soil scientist should reach almost identical conclusions while viewing the problem from the different points of view.

Congratulations to Dr. Scheffer for his fine contribution to a fascinating problem.

RODNEY J. ARKLEY

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NATURAL RESOURCES

A perplexing question may be raised by comparing two statements in the September 1947 issue of THE SCIENTIFIC MONTHLY.

On page 184 Dr. Ira N. Gabrielson states: "It makes little difference whether it is in terms of tree or crop growth, wildlife or grasslands, fisheries or fur, if the crop taken is so great as to destroy or to deplete the soil and water producing them, it is exploitation, not management. For every person who understood this fact ten years ago, there are thousands now who understand it at least dimly."

On page 208 Mr. P. C. Keith states: ". . . the coal-to-oil process will cause us to reckon our future supplies by the hundreds of years rather than by decades. At that time the cry 'of our dwindling oil reserves' will be heard no more."

No doubt Mr. Keith is correct—but I wish he could tell us why. Of course, it is a great relief for a pressing problem to be removed from the class marked "urgent." But the logic of the process whereby it goes into the class marked "never to be heard of again" remains obscure. A so-called intelligent species might expect to hear cries about dwindling resources of any kind whatsoever until it had solved the problem of living within the supply of energy and materials that the earth can provide on a permanent basis. Such an expectation would be only a minimum. The maximum would extend beyond the history of our planet. Probably we can agree to leave that ultimate problem to our grandchildren several degrees removed. And probably we shall agree, whether wisely or no, to hand on to them the more immediate problem that we create by using for our immediate comfort and luxury all the reserves that might be saved for "a rainy day"—or an ice age.

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MODERN HOUSING

Let me compliment you on publishing the article "Toward a Science of Housing," by Theodore Larson, in the October issue. It brings out in excellent fashion a line of thinking that needs sober analysis.

In a subject so old, so vast, and so continually new, it is perhaps impossible to separate the engineering from the social. However, it is obviously essential to recognize not only the presence of both engineering and social sciences, but that each is present in its fullest and most extensive form. Mr. Larson's approach falls pitifully short of covering the magnificent scope of the subject and is therefore closely allied to the crackpot. What little this article contains of social thinking seems to echo the leftish political concepts of regimentation, while his grasp of engineering is at least somewhat befuddled.

Let me be specific. He begins with the revelation that: "Walls need no longer be inert masses of masonry with no function other than to hold up the roof and provide shelter against a harsh external environment. They can be made to do extraordinary things—radiate warmth or coolness, glow with soft light, move about, perform household chores. . . ."

That Mr. Larson's walls can be trained like seals is no longer newsworthy, but that his type of thinking shrugs off the value of holding up the roof and keeping out the weather and then completely ignores the social and esthetic values of architectural beauty and repose is typical of the hysteria of those who would herd us into enameled trailer camps. I am not a religious man but within the comfortable shelter of my own stone and hewn-beamed house I find a soul satisfaction which could not exist in Model 13-X, unit Number 6327, of the F.O.B. Detroit Corporation's aluminum masterpiece.

Mr. Larson holds up to unthinking scorn the fact that our present houses have a median age of twenty-five years. Conditioned by the advertising of automobiles and other new mechanical gadgets, this hoary age would seem to condemn itself. But has he ever wandered through the gracious rooms of Williamsburg, or Mount Vernon, or any of a thousand other fine old dwellings? Has he not seen even the smallest of many old-time cottages whose inner comfort and outer charm easily outbids much of both contemporary and futuristic so-called housing? By the addition of only accessories—heating, plumbing, and lighting—the old house with basic architectural merit remains a family comfort and a community asset.

To his 14 percent of all houses that need major repairs and to those without modern conveniences could also be added the thousands of dreary shacks devoid of artistic or suitable design, but in this connection it must be noted that their owners seem to be well supplied with eight-cylinder machines for getting away from their uninspired homes. It is a fact that automobile paper is more gilt-edged than first mortgages on houses. These people have made their choice of values, and home is not among the titles on the hit parade. To this group the steel panel prefab would appeal, provided it could be occupied on weekly payments smaller than those required for a shack or a tenement in the slums.

However, this hope for a lower level of cost does not appear to be warranted. All the prefab plans today attack the shell of the house as their basic premise. They grope for novelty in wall construction. I have before me a detailed itemization of every cost involved in the building of my house and I find that the total of all costs pertaining to the exterior walls, which are sixteen inches thick of masonry, insulation, and interior paneling, is just 18 percent of the total house cost. Frame construction in comparable houses represents only 14 percent. Would a saving of even one-half these figures warrant the complete destruction of a character and beauty universally appreciated? Will ivy cling to, or even a rambler rose cover, the "sanitary baked-on enamel" of walls that any handy man can erect with nothing but a screw driver?

It is true that a house costs much more today, even comparatively, than it did a generation ago. That is largely because it has already ceased to be a simple shell. Bathrooms cost more than privies; wiring, oil burners, and weather stripping are new expenses. But to advance the claim that these desirable features in a single house on a private lot should be made available to the lowest income groups is simply to repeat a hypnotic fantasy. However, these groups do find their modern housing in multiple units. In fact, the apartment house answers the needs of not only low-income families but also of thousands of others who, because of circumstances, do not find the single house expedient. Let the prefab house promoters realize that their costs cannot possibly rival the economies of the well-planned multiple unit.

While it is evident that I find little sense or reason in Mr. Larson's article, I do recognize the underlying need for improvement in housing economics just as in other fields. But I submit that this improvement is a process already under way in a logical manner quite consistent with our civilization. To blame the industry for a housing shortage makes even less sense than to blame General Motors because I do not own a new Cadillac.

The housing industry is not technologically backward. On the contrary, it has made prefabrication and assembly-line production so common that more people than ever aspire to housing luxuries undreamed of a few years ago. Today's carpenter is derided for his lack of skill, but consider that he no longer has the necessity for developing true journeyman status. His doors and windows, carefully packaged with their frames, arrive complete from the assembly line. His cabinets, millwork, flooring, stairs, mantels, and bookshelves are all out of the catalogue. The plumbing, heating, and lighting fixtures are all factory-made. Still larger assemblies are already common, such as kitchen and bathroom units. To deliver at least these two rooms as complete assemblies is but a step and is on the way. This indeed is economic prefabrication.

Were our lending institutions themselves better educated in sound design rather than in quick financing of nondescript and uninspired "housing," our twenty-five-year-old structures would radiate even increased charm instead of the dejection of the slum. After all, it takes twenty-five years to achieve fully landscaped grounds. The depreciation of good houses is more apt to be forced by changed surroundings than physical decrepitude, but as this normal reduction in relative value occurs, these houses retain much that is basically good and become available to those who cannot afford new construction. The average ignorant banker, schooled only in the fine art of foreclosure, lends most of his money on houses to which competent architectural design has never been applied. He thus fosters the blight miscalled obsolescence.

The new type of house that Mr. Larson mentions as a possible goal, one which blends its indoors with outside space, is a delightful objective, but to build even two alike would require two identical landscapes.

Let these charlatans at housing turn their resources toward (1) better design for more of the houses that will be built in the immediate future; and (2) the trend toward prefabrication in subassemblies that is already rolling and forget the tin-pan-alley wall sections.

Lindsay Lord

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HONEYBEES AND RED CLOVER

As an amateur beekeeper, who is more interested in knowledge of bees than in the delectable honey, I wish to comment on Mr. McAtee's "Cats-to-Clover Chain" (September SM). Undoubtedly, honeybees do pollinate red clover. A few years ago the 30-acre field beside my few hives was in red clover (*Trifolium pratense*). The late summer and fall were very dry that year, and my honeybees found their chief employment among the blooms of the clover. Standing anywhere in the field, I could count from six to twelve workers among the blossoms. Some of these bees were collecting pollen, but the majority seemed to be nectar hunters, since their pollen baskets were empty and their abdomens appeared distended. Slowly during August and September their stores of honey increased, although the smartweed (*Polygonium*) flow of early fall was nearly a failure that year.

This year the same field is in red clover, but my bees are not interested. I walked slowly through the field for a hundred yards today [September 20], but the one honeybee I encountered was working the few scattered flowers of the alsike clover (*T. hybridum*). Yet, other species of insects, such as the bee flies (Bombyliidae), clover butterflies (*Eurymus philodice*), and the monarch butterflies (*Danaus archippus*), are common in the field and give every evidence of satisfaction with the clover's bounty. Probably these insects, for all their abundance, are not enough to pollinate a satisfactory percentage of the clover flowerets. The bumblebee, the universally acknowledged effective, is busily present, but only in very small numbers. Casually estimating the numbers of these four insects, I think they are about as numerous this year as in former years. Only the honeybee has sufficient numbers to do the job of pollination, and it alone is absent. Why?

I haven't the slightest idea. My apiculturist friend, Dr. Dunham, states: "The response to red clover varies with the genetic background of the plants, their physical condition, the soil types on which they are grown, weather conditions, and many other factors." Many of the conditions affecting my bees are identical, but the weather this past summer has been unusually wet instead of dry. Possibly this is somehow a factor. Yet a visit to my hive shows the bees are not busy. The smartweed-goldenrod flow is over, the asters are not yet in bloom. Although other insects appear to enjoy the red clover, the honeybee will have none of it this fall. Even the alsike and Ladino clovers, which are also in bloom in other fields at no great distance, are only moderately attractive. All summer long my bees have found but little of interest in the abundant clover blooms of several types. The little excess honey they have cured has come from other sources, such as the smartweed-goldenrod complex. Why this dearth of nectar when clover blooms were plentiful?

Darwin, on a later page of the *Origin* returns to the subject of the honeybee and the red clover. His words are interesting in view of McAtee's criticism:

> The tubes of the corolla of the common red and incarnate clovers (Trifolium pratense and incarnatum) do not on a hasty glance appear to differ in length; yet the hive-bee can easily suck the nectar out of the incarnate clover, but not out of the common red clover, which is visited by humble-bees alone; so that whole fields of red clover offer in vain an abundant supply of precious nectar to the hive-bee. That this nectar is much liked by the hive-bee is certain; for I have repeatedly seen, but only in the autumn, many hive-bees sucking the flowers through holes bitten in the vase of the tube by humble-bees. The difference in the length of the corolla in the two kinds of clover, which determines the visits of the hive-bee, must be very trifling; for I have been assured that when red clover has been mown, the flowers of the second crop are somewhat smaller, and that these are visited by many hive-bees. I do not know whether this statement is accurate; nor whether another published statement can be trusted, namely, that the Ligurian bee which is generally considered a mere variety of the common hive-bee, and which freely crosses with it, is able to reach and suck the nectar of the red clover. Thus, in a country where this kind of clover abounded, it might be a great advantage to the hive-bee to have a slightly longer or differently constructed proboscis. On the other hand, as the fertility of this clover absolutely depends on bees visiting the flowers, if humble-bees were to become rare in any country, it might be a great advantage to the plant to have a shorter or more deeply divided corolla, so that the hive-bees should be enabled to suck its flowers. Thus I can understand how a flower and a bee might slowly become, either simultaneously or one after the other, modified and adapted to each other in the most perfect manner, by the continued preservation of all the individuals which presented slight deviations of structure mutually favourable to each other.
>
> I am well aware that this doctrine of natural selection, exemplified in the above imaginary instances, is open to the same objections which were first urged against Sir Charles Lyell's noble views. . . .

Clearly, Darwin did not regard the clover-bee situation as necessarily static, for the clover might adapt to honeybee pollination; nor did he regard his words as absolute or final. Rather, he regarded the situation as plastic, his interpretations as imaginative and possible, instead of precise and immutable.

McAtee need not have apologized for reopening the question, for outside Russia few believe Darwin infallible. Many, and among them the writer, admire his truly marvelous degree of inerrancy, and for that reason some of us delight in spotting those few errors to be found in the *Origin*.

Furthermore, questions of considerable economic importance are forcing us to reconsider today the bee-clover relationship, for we find that yields of legume seeds, including red clover, have declined until the situation is acute. Large research programs have been established to discover the reason for these falling yields. It is nearly a hundred years since Darwin found, quite correctly, that 20 heads of insect-pollinated clover yielded 2,290 seeds, whereas 20 protected heads yielded not one. Investigators are seeking ways to obtain consistently those high yields. Prominent in the research program is the relationship of the bee to the clover, for that relationship is not wholly understood, even today. In the absence of sufficient wild insects to pollinate the clover, it is of critical importance that we understand better the erratic behavior of the honeybee. Darwin's observations on the subject are still valid, although his interpretations may fall a little short of perfection.

PAUL D. HARWOOD
Ashland, Ohio

THE BROWNSTONE TOWER

OUR regular readers, we hope, noticed the resumption of Science on the March in the January issue of the SM. This section had been discontinued during 1947 while a new and larger staff of official reporters was being appointed by societies affiliated with the A.A.A.S. The response of officers of the affiliated societies to our request for the appointment of reporters representing these societies was most gratifying. Evidently the officers realized that our proposal would provide an outlet for authentic popular information about significant advances in their sciences. From our point of view the appointment of professional scientists, carefully selected on the basis of their professional standing and ability to write for intelligent laymen, would give us a greater number of factual articles of current significance than we have been receiving, thus increasing the interest and value of the SM for our readers.

The first article by an official reporter appeared in the January issue. It was written by Dr. Charles F. Richter, of the Balch Graduate School of Geological Sciences, California Institute of Technology. Dr. Richter, representing the Seismological Society of America, wrote a truly global account of international recovery in seismology since the war. In the present issue Dr. K. Starr Chester, representing the American Phytopathological Society, and Dr. S. E. Luria, representing the Genetics Society of America, make their appearance. As time goes on more and more sciences, both pure and applied, will be heard from in Science on the March through their official reporters.

Appointment of all reporters has not yet been completed, nor have all reporters who have accepted appointment submitted their first manuscripts. We express the hope here that those societies that have not yet taken action will soon do so and that reporters who have been appointed will submit copy promptly without further word from the editor. On pages 162 and 182 of this issue is a list of present reporters and their respective societies.

The question has been raised by some reporters whether it is necessary for them personally to write all contributions for Science on the March in the fields of science covered by their societies. The answer is "no." A reporter may request a colleague to write an article on a subject that he knows better than the reporter. If that is done, we should want the article to be approved by the reporter and submitted by him to the editor. Of course the reporter not only may, but should, consult his colleagues in the preparation of his own articles, even when he is writing about his own work, for everyone has blind spots toward his own writing. Any manuscript that appears to be suitable for Science on the March and is written and submitted to the editor without the knowledge of a reporter will be referred to the appropriate reporter by the editor. If satisfactory to the reporter, such independent articles may be published. Longer manuscripts intended for publication as principal articles may be referred to reporters for review and criticism when their advice would be helpful to the editor. Also, reporters may help the editor by writing principal articles and suggesting to the editor subjects and possible authors for principal articles.

The editor cannot overemphasize the desirability of writing for the SM in a popular but dignified manner. Articles for the SM are to be read, not studied; and they are to be read largely by those who are not specialists in the subject of the article. Therefore, the customary rules for technical writing do not apply. More background is needed than is required for specialists, technical terms of limited use should be translated or defined, bibliographies are not wanted, mathematical equations are to be avoided. Although lacking the appearance of precision and scholarship, factual articles for the SM may be and should be just as reliable and authentic as the original technical articles from which they may be derived. Our new staff of reporters will bring us closer to that goal.

F. L. CAMPBELL

PHOTOGRAPHY IN SCIENCE

Announcing the Second International Competition

Sponsored by

Smithsonian Institution and THE SCIENTIFIC MONTHLY

IN THE previous issue of THE SCIENTIFIC MONTHLY (January 1948) appeared a report on the First International Photography in Science Competition sponsored by the Smithsonian Institution and THE SCIENTIFIC MONTHLY. Lt. Alexander J. Wedderburn, its author, presided at the judging of the entries in his capacity as associate curator of the Division of Graphic Arts, Smithsonian Institution, and he was in charge of the exhibition of the accepted entries in the Smithsonian during November 1947. Having been pleased by the number and character of the entries and by the attention given to the first salon by scientists and the press, Lt. Wedderburn and the editor of the SM proposed that a second competition and salon be held in 1948. Approval of a second event was given by the Executive Committee of the A.A.A.S. for the SM and by Dr. Alexander Wetmore for the Smithsonian. At the same time Dr. Wetmore reserved exhibition space for the second salon in the gallery on the ground floor of the Natural History Building, U. S. National Museum—a better place for the exhibition than that available in 1947. So the second salon is scheduled for the first three weeks of September 1948, and within that period the Centennial Meeting of the A.A.A.S. will be held in Washington, September 13–17.

Although the first salon met the expectations of the sponsors, the second can and should be improved as a result of experience gained in the first. Unlike a pictorial salon in which subject interest, composition, and photographic technique are predominant factors in judging entries, "Photography in Science" emphasizes the utility of the entries in scientific research. Entries that provide scientific information unattainable except through the use of photography are most appropriate, and if, in addition, novel photographic methods have been devised to get the desired information, still more weight will be given. Therefore, it is important for the judges to be able to distinguish those entries that have an element of scientific and photographic novelty from those that are merely excellent examples of conventional scientific photography. Those who view the pictures later will also want information about the significance of the exhibits. Entrants will therefore be asked to paste on the front of the mount under the print not only a neatly lettered or typed title but also supplemental information on the significance of their entries.

The rules for the first salon stated that "only professional scientists . . . are eligible to enter the exhibition" and that "all prints must be entirely the work of the individual contributor or co-workers." We fear that this rule could have been interpreted too strictly. It was not intended to bar cooperation between scientists and photographers, nor to prevent specialists in photographic research from submitting independent entries. When a proposed entry embodies the ideas and skill of both a scientist and a photographer, it would be appropriate to submit it as a joint contribution. The ordinary operations of developing and printing, which may be done for scientists by photographers, would not seem to require special recognition of the photographer.

In the 1947 rules it was stated that the pictures would be returned to the contributors within two weeks after the close of the Chicago meeting of the A.A.A.S. It was not then anticipated that various institutions would request the privilege of exhibiting the pictures after they had been shown by the Smithsonian and the A.A.A.S. At this writing five such requests have been received. The pictures will be shown elsewhere, and return of the entries to the contributors will therefore be delayed. It will be understood that all or part of the entries for the second salon may be borrowed for showing in other cities after the close of the exhibition in the Smithsonian on September 21, 1948.

The following men have expressed their willingness to serve as judges of the second competition:

For the physical sciences: Dr. Wallace R. Brode, recently appointed associate director of the National Bureau of Standards. He has done outstanding work in spectroscopy and organic chemistry.

For the biological sciences: Professor Charles T. Brues, Harvard University. A scholar in entomology and related sciences, Professor Brues has made much use of photography in biology.

For the medical sciences: Dr. Edward J. Stieglitz, a physician practicing in Washington, D. C. He is known for his books and articles on gerontology.

For photography: Dr. Sidney S. Jaffe, Washington, D. C., a member of the National Photographic Society.

The foregoing committee will be headed by Lt. A. J. Wedderburn, who again will be in charge of the salon.

SUMMARY OF INFORMATION AND RULES

Purpose. To show examples of the uses of photography in scientific research.

Sections. (1) Black-and-white prints; (2) colored prints and transparencies.

Who are eligible. All scientists everywhere, including those engaged in photographic research. Scientists may collaborate with photographers, and when the photographer's contribution to the final product is substantial, the entry may be submitted as a joint contribution from the scientist and the photographer.

What are eligible. All photographs taken for scientific purposes. Purely pictorial photographs and hand-colored prints are not eligible.

Dates. Entries will be received by the editor of THE SCIENTIFIC MONTHLY from July 26 to August 16, 1948, inclusive. Entries will be judged on August 21, 1948. Those accepted will be shown in the Natural History Building, U. S. National Museum, Washington, D. C., from September 1 to September 21, 1948. All or some of the pictures may be exhibited later in other cities by institutions requesting the privilege.

Judges. Dr. Wallace R. Brode, Professor Charles T. Brues, Dr. Sidney S. Jaffe, Dr. Edward J. Stieglitz, and Lt. Alexander J. Wedderburn, chairman.

Criteria. The greatest weight will be given to entries that show novelty of application of photography to scientific research and originality of photographic technique. Good examples of conventional photography applied for ordinary scientific purposes will be acceptable, but will be given less weight than those that show scientific originality. Although photographic quality of entries will have the least weight, it should not be neglected.

Awards. There will be first, second, and third awards and five honorable mentions in each of the two sections, black-and-white and color. Awards will take the form of certificates suitable for framing. To each print accepted for exhibition, the label of the Smithsonian Institution will be affixed.

Number and preparation of entries. Each contributor may submit up to eight entries, four in color and four in black-and-white. Each print must be mounted on stiff white cardboard, 16 x 20 inches. One mount may carry more than one print, if necessary to show progressive steps, and each mount will be regarded as a single entry. The dimension of the prints is immaterial, provided space remains on the front of the mount under the print or prints for a neat label giving the following *essential* information: Name, highest degree, official title, and business address of the contributor, including the name of the company or institution employing him; title of the entry and magnification; a concise statement of the scientific significance of the entry. For the benefit of the judges, additional technical data may be given on the back of the mount, and to assist in identification of the entry, the contributor should give its number on the back of the mount; e.g., "This is entry No. 2 of four entries in the black-and-white section."

Color slides or transparencies should not exceed 3¼ x 4¼ inches and must show the contributor's name and address, and the title and number of entry. Complete information, as specified above for the mounts, must also be given for the transparencies, but on separate sheets of paper properly identified and attached to the entry form. Slides may be in glass or cardboard, but glass is recommended. Each slide must be spotted in the lower left-hand corner. If not bound, transparencies should be protected in cellophane envelopes.

Shipping and handling. All pictures must be packed flat with sufficient protection. Reinforced cardboard cases designed for mailing 16 x 20 inch mounts may be purchased from photographic supply houses. Shipping labels will be supplied with entry forms. Entries should be sent prepaid to The Editor, THE SCIENTIFIC MONTHLY, 1515 Massachusetts Ave., N. W., Washington 5, D. C. When the entries have completed their travels, they will be returned to the contributor by express collect.

The receipt of entries will not be acknowledged, but as soon as possible after the judging a card will be mailed to each entrant indicating the entries accepted. Those not accepted will be returned promptly to the sender.

It may be assumed that all exhibitors will handle the entries with care. The Smithsonian Institution and THE SCIENTIFIC MONTHLY will take pains to prevent damage or loss of entries, but assume no responsibility if such should occur.

Publication: Unless specifically forbidden by the contributor, THE SCIENTIFIC MONTHLY reserves the right to publish any of the entries accepted, and to grant permission to publish them in newspapers and other periodicals.

Inquiries about the Second Photography in Science Salon and requests for entry blanks should be addressed to The Editor, The Scientific Monthly, 1515 Massachusetts Ave., N.W., Washington 5, D. C. Submission of entries is understood to imply acceptance of all the foregoing conditions of entry. The entry form, properly filled in, must accompany entries.

REPORTERS FOR "SCIENCE ON THE MARCH"—*Continued*

SECTION N (MEDICAL SCIENCES)

American Medical Association	Austin Smith
American Psychiatric Association	C. C. Burlingame
American Veterinary Medical Association	R. C. Klussendorf
Society of American Bacteriologists	Monroe D. Eaton Wayne W. Umbreit
The American Physiological Society	John Field

SECTION O (AGRICULTURE)

American Society for Horticultural Science	H. B. Tukey Ora Smith V. T. Stoutmeyer
American Society of Animal Production	C. F. Huffman
Society of American Foresters	William M. Harlow C. R. Lockard
The American Society of Agronomy	C. E. Marshall H. M. Tysdal

THE
SCIENTIFIC MONTHLY

MARCH 1948

THE U. S. NAVAL POSTGRADUATE SCHOOL

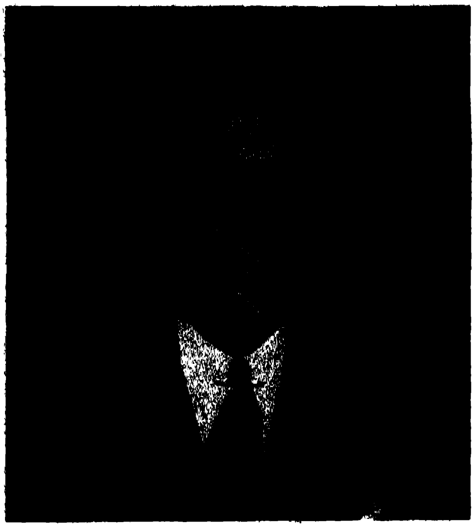
FORD L. WILKINSON, JR.

Dr. Wilkinson graduated from the U. S. Naval Academy in the class of 1918 and served in the Navy until 1927. In March 1947 he went to the U. S. Naval Postgraduate school as its first civilian dean. He had previously held the position of dean of the Speed Scientific School at the University of Louisville.

RECENT legislation by Congress has provided the Navy with an institution that may well become one of the nation's outstanding graduate schools in science and technology. The two acts passed by the Eightieth Congress, and enacted into laws by the signature of the President, provide the U. S. Naval Postgraduate School with statutory authority to conduct graduate and postgraduate instruction in an unlimited variety of fields and to establish the School as an independent unit with its own superintendent and with administrative authority similar to that of the U. S. Naval Academy; they authorize also the purchase of property at Monterey, California, for the future home of the School.

Although the legislation referred to marks an important milestone in the history of naval officer advanced education, its major effect is to recognize by legislation a program that has been under way under executive order since 1909 and to give to that program a permanent place in the education of officers of the Navy, the Marine Corps, and the Coast Guard. In addition, it provides for the purchase of a property of adequate size in a location ideally suited for the conduct of a year-round program of intensive study. Although the legislation authorizes only the purchase of the property and makes it possible to convert the buildings already on the site to immediate usefulness by a limited activity of the School, future plans call for a transfer of all the School's activities to Monterey as soon as funds are available and a suitable physical plant can be constructed. It is believed that the procurement of the Monterey site is the first time in the history of education that an institution of higher learning has been established in a locality selected solely on the basis of environmental conditions.

History of the Postgraduate School. The Naval Postgraduate School was organized in 1909 as a division of the U. S. Naval Academy, with an officer in charge who was designated as head of the School. It has operated under this plan with its own instruction staff since that date, except for two years during World War I. It has occupied its own quarters on the Naval Academy grounds since 1919, and its physical plant has until recently been adequate for the number of student officers enrolled.



Official U. S. Navy Photograph

DR. FORD L. WILKINSON, JR.

ACADEMIC DEAN, U. S. NAVAL POSTGRADUATE SCHOOL.

The separation of the School from the Naval Academy has been necessitated by the tremendous increase in demand for scientific and technical graduate education of naval officers and because of the inability of the School to expand its present physical plant. Hereafter, graduate and advanced education will be a function separate from undergraduate education.

The School consists of three major divisions: the Engineering and Scientific Division, now at Annapolis; the General Line School at Newport, Rhode Island; and the Naval Intelligence School at Anacostia, D. C. Each of the three units is relatively independent at present, but under the general administrative direction of the superintendent of the U. S. Naval Postgraduate School. It is expected that all three of the major divisions will be combined in one location when facilities are made available at Monterey.

In addition to the three separate schools for which the Navy supplies complete faculties and physical facilities, the superintendent of the U. S. Naval Postgraduate School directs the educational activities of naval officers conducted entirely at civilian institutions. These programs, usually involving small groups of officers, are at present in the fields of naval construction, civil engineering, law, theology, textile engineering, business administration, personnel administration and training, and industrial accounting. The programs extend over periods of a few months for certain special subjects to three years for naval construction. Each is given under the specific curriculum of the institution to which officers are assigned and is conducted under the educational supervision of the faculty of that institution. The program in each of the participating colleges and universities is reviewed by the administration of the Postgraduate School in conjunction with the naval activity initiating the program and the Division of Training of the Bureau of Naval Personnel.

Inasmuch as the largest concentration of graduate and professional studies is within the three major divisions operated under their faculties, the organization and activities of these divisions will constitute this story of the U. S. Naval Postgraduate School.

General Line School. For some years prior to World War II, junior officers were ordered to the Naval Postgraduate School at Annapolis for a one-year course in purely professional subjects. It was the intent of the Navy Department to utilize this period in the conduct of an intensive program of professional training in all activities of the line officer, with limited applications in the technical fields of marine engineering and communications. It was undertaken to offer to each young officer on his first tour of shore duty an opportunity to enhance his educational background and confirm his experiences as a junior officer at sea.

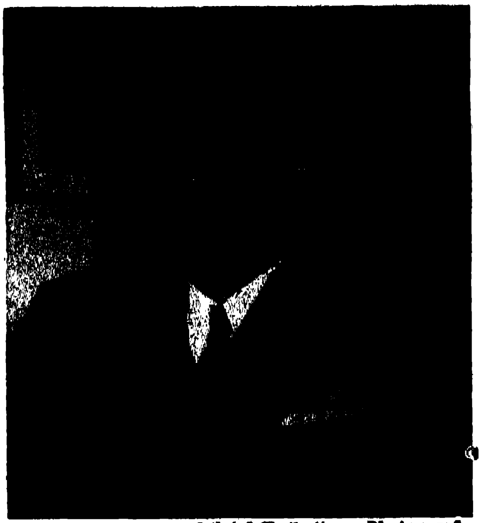
An effort was made to ensure that each junior officer received one year of General Line School training within about five years after receiving his commission. Since the input of commissioned officers consisted almost exclusively of graduates of the Naval Academy, and the classes were in those days small, this work could be carried on without undue strain on the physical facilities of the Annapolis School. The staff was composed largely of naval officers teaching purely professional subjects, with members of the civilian faculty of the engineering division assisting in refresher courses in mathematics, electrical engineering, and fundamental sciences.

The General Line School was discontinued just prior to the war because of the need for officers to man the rapidly growing fleet and because of the increased need for technically educated officers from the technical school. The physical facilities could not accommodate both programs.

The School was revived in 1946 in temporary quarters at Newport, with a new purpose and objective. The Navy Department had agreed to provide each Naval Reserve officer who transferred to the Regular Navy a year of professional training in the General Line School. Its revival in 1946 was for the purpose of carrying out this agreement. The present enrollment of approximately 600 student officers consists mainly of those officers who have been performing duties at sea of a highly specialized nature but who have had little or no training in the general-line duties of a naval officer.

It is expected that a second school for 500 additional officers will soon be established in Monterey. It is anticipated that by 1954 the tremendous backlog of transferred officers will have gone through these schools. When that is accomplished, the Newport school may be discontinued, and the General Line School at Monterey will be integrated with the U. S. Naval Postgraduate School on the prewar basis, serving graduates of the Naval Academy and those of the fifty-two NROTC units who transfer to the regular service. Its eventual capacity at Monterey will be approximately 1,250 junior officers annually pursuing a one-year curriculum.

Naval Intelligence School. Experience during the war definitely determined the need of a training school for naval intelligence, and the present school was organized and its first class started in July 1946 at the Naval Barracks at Anacostia. There are two major divisions of the Intelligence School: one is concerned with professional and strategic intelligence training, which consists of approximately nine months at the school and in the field; this is followed by concentrated foreign-language instruction extending over periods varying from three to twenty-one months, depending upon the language being studied.



Official U. S. Navy Photograph

CAPTAIN HERMAN A. SPANAGEL, USN

SUPERINTENDENT, U. S. NAVAL POSTGRADUATE SCHOOL.

The language division of the school follows the methods employed by the Navy's wartime Japanese Language School. Because of the unique methods of instruction and the high degree of concentration on the languages taught, the School has been successful in graduating a majority of its student officers with a proficiency in the language studied equal to that of a native who has an educational background at the equivalent level of a second-year college student. The success of the language division of the Intelligence School is attested by the number of governmental agencies utilizing its facilities and faculty for language training for foreign service.

As soon as is feasible, the Naval Intelligence School will also move to the Monterey location.

The Technical Division. Of major interest to readers of THE SCIENTIFIC MONTHLY is the program of the graduate division in science and engineering. The work of this division has been the backbone in the development of engineering and of scientifically trained officers since the School was first established in 1909.

Because of the necessity of having all engineering and scientific laboratories well established and equipped before moving to a new location, it is expected that the Technical Division will remain in its present location at Annapolis for several years. It, however, will follow the General Line School and the Naval Intelligence School as early as is convenient and thus complete the establishment of the U. S. Naval Postgraduate School in Monterey.

From the very beginning of naval postgraduate education, the basic reason for operation of the Postgraduate School has been the need for a relatively large number of naval officers who possess scientific and engineering education beyond that encountered in their undergraduate work. Officers so trained have for years been directing the operation and maintenance of machinery aboard ships, directing the design and procurement divisions of the material bureaus of the Navy, initiating new technical developments, and serving as the directors of Navy tests and research. The areas and fields of study are determined by the technical bureaus of the Navy, and representatives of those bureaus assist in the development of the curriculums offered by the Postgraduate School.

Until relatively recent years, the Postgraduate School at Annapolis drew its students from the ranks of Naval Academy graduates who, after a number of years at sea, were selected for advanced educational programs under the sponsorship of the Bureau of Ships, Bureau of Ordnance, and Bureau of Aeronautics. The courses consisted mainly of a one-year curriculum designed for the purpose of bridging the gap between the professional program of study at the Naval Academy and the requirements for entrance to a graduate school of engineering at selected civilian institutions. The work at the civilian institutions was usually of a broad nature in a general field of engineering, such as aeronautical, electrical, or mechanical. It was customary on the part of most civilian institutions to award the Master's degree after one year of study in the specified subjects.

As the technical complexity of naval operation increased and the needs for officers with more specialized and scientific training became imperative, the program became more flexible and expanded in scope. Today it offers, either wholly at the School or in cooperation with civilian colleges and universities, approximately twenty-two scientific and engineering curriculums leading to the Master's degree, granted by either the Naval Postgraduate School or one of the nine cooperating institutions.

The criterion used to decide whether an entire graduate curriculum shall be offered at the School or at a selected civilian institution is based on an exhaustive study of the requirements of the course of study, and the decision is reached on the answer to the question, Where can the curriculum be taught with the greatest effectiveness?

Most of the graduate engineering and scientific curriculums are conducted over a period of three calendar years. Certain of these, such as naval construction and civil engineering, are conducted wholly at the cooperating civilian institutions. Others consist of preliminary study of one year at the Postgraduate School followed by two additional years at the School or elsewhere.

The curriculum of the first year is devoted to refresher work in mathematics, physics, and chemistry, with additional work of an advanced undergraduate nature designed to supply the deficiencies of the students' undergraduate technical and scientific studies. Because of the maturity of the students and the high degree of selection, it is possible to carry this program in the third and fourth terms of the first year to a somewhat higher level than is common in undergraduate study.

The second and third years deal rather specifically with the field of specialization undertaken by the student officers. In some fields of study these years are spent at the cooperating institutions, in others the second year is continued at the School with only the third year away, and with still others the entire three-year program is conducted at the School.

The need for specialized study is determined mainly by those divisions of the Navy dealing with design, construction, and operation of machinery, and with research and de-

Official U. S. Navy Photograph

U. S. NAVAL POSTGRADUATE SCHOOL

THIS IS THE PRESENT BUILDING ON THE GROUNDS OF THE NAVAL ACADEMY, ANNAPOLIS, MARYLAND.

velopment. Assignment to study is based on the projected needs of these divisions for specialized education beyond the undergraduate level. In addition, there are a few programs of an applied nature seeking to develop trained officers in a short period for certain operational billets at sea and ashore. These latter are called "applied curricula," are definitely at the training level, and are not recognized as involving graduate study. At present these programs, covering periods of from one to two years, are in the fields of applied communications, applied naval engineering, and applied aerology.

The distribution of student officers for the year 1946–47 under the direct supervision of the staff of the Postgraduate School was approximately as follows: 543 enrolled in curriculums conducted in whole or in part at the Annapolis School, of which 328 were taking first-year work at Annapolis, 137 taking advanced work at civilian institutions, and 78 taking advanced work at the School. The number taking advanced work at Annapolis in 1947–48 has increased to approximately 190 enrollees, with a total enrollment of all students in the engineering and science courses, either at Annapolis or elsewhere, of 570. The 570 student officers are enrolled in twenty-eight scientific and engineering curriculums, of which all but three are graduate in character and lead to at least a Master's degree granted by the Postgraduate School itself or by a cooperating civilian institution.

At present, the Postgraduate School conducts complete curriculums at the School leading to the Master's degree in engineering electronics, aerology, and naval engineering.

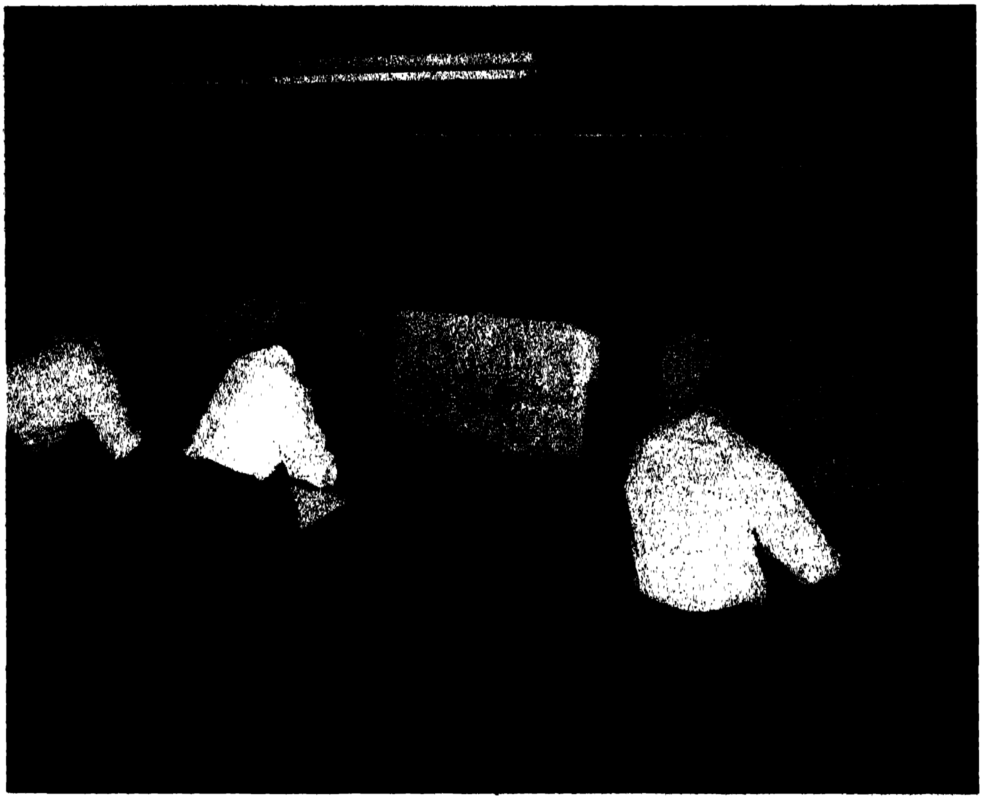
The staff of the School at Annapolis is composed of both naval officers and civilians, the officers conducting courses in purely naval professional subjects, generally in the applied curriculums, and the civilian faculty in science and nonnavel technical subjects. At present there are approximately 30 naval officers and 59 civilian faculty members attached to the staff.

The division of work between the naval school and the cooperating institution is not fixed, but is determined on the basis of best utilization of the nation's graduate educational facilities. It is believed that the programs conducted wholly at the School can best be handled there because of the relatively large numbers involved and the particular marine emphasis required in the graduate program. In other fields of a high degree of specialization, involving a relatively small number of student officers, it is believed that the objectives of graduate education can best be attained by drawing on the facilities of those schools whose faculties have demonstrated outstanding competence in the field of study to be pursued. In many cases two or more institutions are selected for similar programs. The results of graduate instruction are continuously reviewed by the Postgraduate School staff, and shifts in cooperating institutions are made from time to time in order that the most effective programs may be obtained.

In the spring of 1947, the Navy Department requested the American Council on Education to appoint a Committee to "Report on the Educational Program of the United States Naval Postgraduate School." That Committee consisted of Henry T. Heald, president of Illinois Institute of Technology, chairman; Edward L. Moreland, executive vice-president of Massachusetts Institute of Technology; Baldwin M. Woods, director of University Extension and profes-

Official U. S. Navy Photograph

A SECTION OF THE ELECTRICAL ENGINEERING LABORATORY



Official U. S. Navy Photograph

METEOROLOGICAL MAPPING IN THE AEROLOGY DEPARTMENT

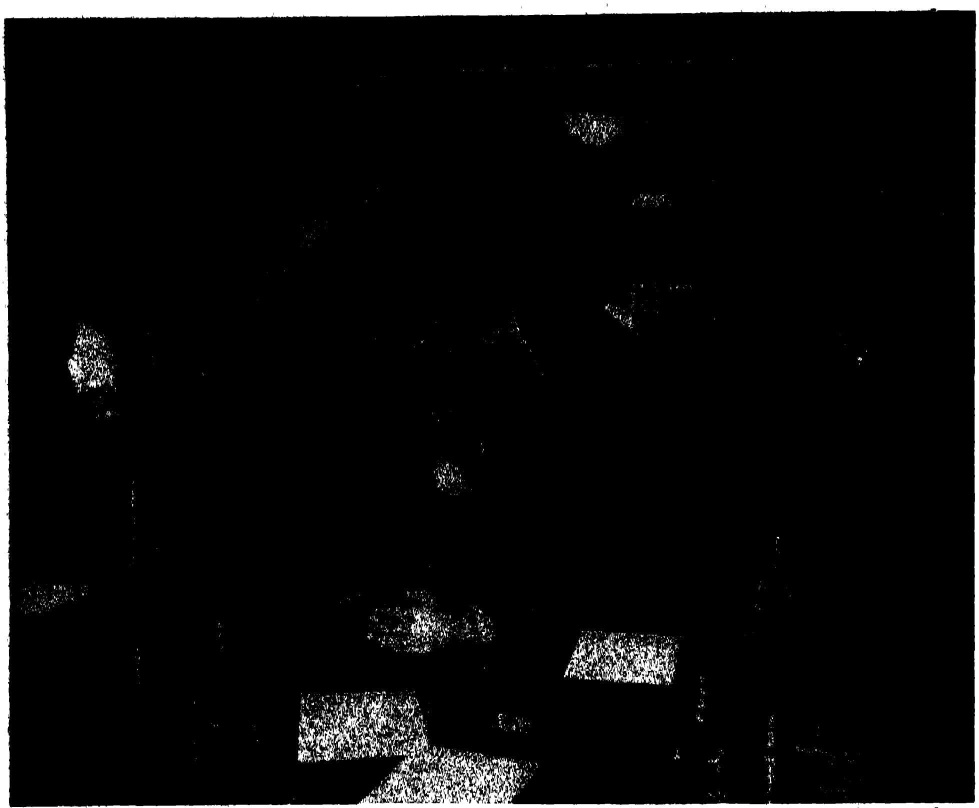
sor of mechanical engineering at the University of California; with Herbert A. Simon, professor of political science at Illinois Institute of Technology, as secretary.

The report of the Committee has this to say about the program of graduate study:

> We believe that the Navy has been wise in not attempting to develop completely self-contained educational facilities to meet all these training needs, but in drawing, instead, upon the resources of established civilian institutions for a substantial part of the program. . . . No general criteria appear to have been laid down to determine where the work of the Postgraduate School should leave off, and where the work of cooperating institutions should begin. However, the committee realizes that the most effective division must be determined separately for each curriculum in view of the needs of the program and the facilities both of the School and civilian institutions. Probably no single pattern will be suitable for the program as a whole.

As a general policy, the Postgraduate School has attempted to develop strong faculties in a few fields for which there is a constant and stable demand for graduate and advanced undergraduate education. These are largely in the scientific fundamentals of mathematics, mechanics, physics, and chemistry, and in the specialized fields of electrical engineering, electronics, metallurgy, mechanical engineering, meteorology, and aeronautics. Its faculty is selected on the basis of the strength of each member's training in fundamental science and mathematics, with emphasis on proved ability in creative endeavors. Members of the faculty are given privileges for research and consulting practice similar to those of an enlightened civilian institution, and they are employed under the tenure rules prevailing in American educational institutions. They are not classified civil service employees but are in an excepted class of government service.

The principal officials of the U. S. Naval



Official U. S. Navy Photograph

MICROSCOPIC EXAMINATION OF METALS IN THE METALLOGRAPHY LABORATORY

Postgraduate School at Annapolis are the superintendent, who by law must be a naval officer of rank not below that of captain; an executive officer; and the academic dean, who is a civilian.

All officer and civil service personnel are under the control and supervision of the executive officer, who is responsible for the execution of the plans and policies of the superintendent. The academic dean exercises control and supervision over all civilian faculty members and is responsible for all instruction activities except in strictly naval professional subjects.

As stated elsewhere, the staff of the U. S. Naval Postgraduate School includes both naval officers and civilians. Some of the naval officers instruct in naval subjects, and in addition there is an officer in charge of each of the curriculums of instruction. In a sense, the officer in charge of a curriculum is a liaison officer between the School and each material bureau of the Navy, but under the direct authority of the superintendent. It is his specific duty to see that the curriculum or curriculums under his supervision are so organized and conducted as to meet the educational needs of the sponsoring material bureau. Student officers assigned to a curriculum of study are under the direct supervision of the officer in charge.

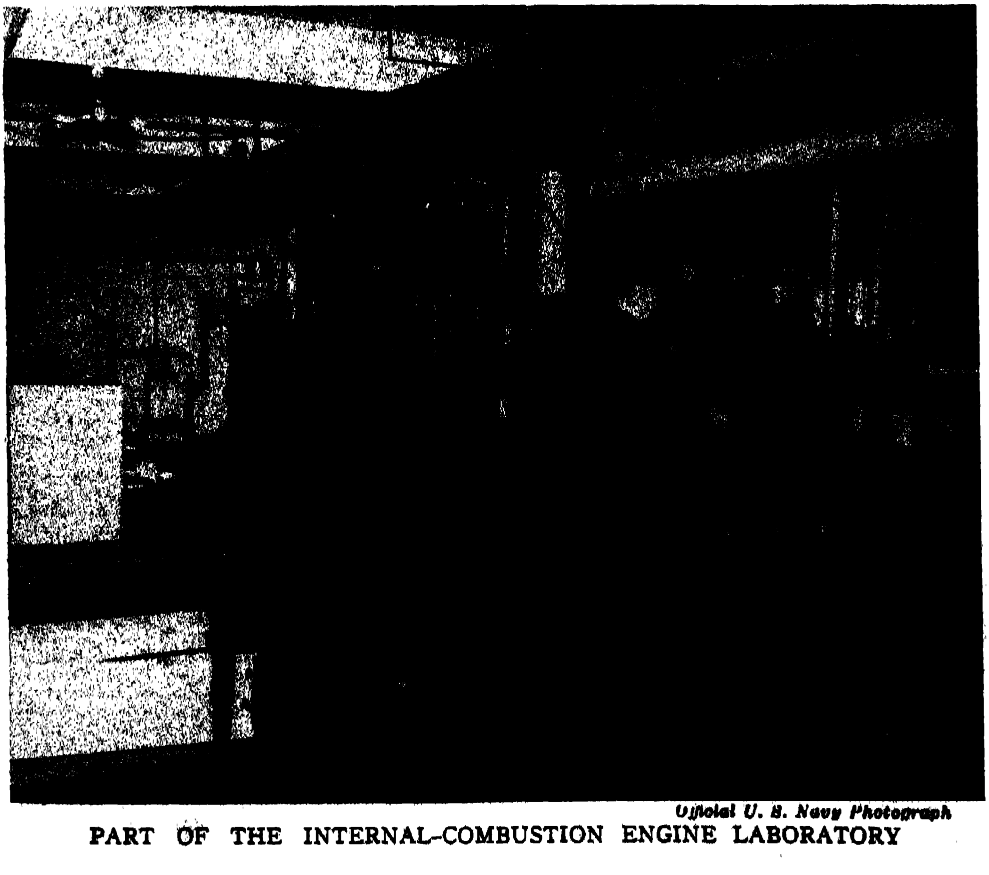
The civilian faculty members are grouped into service departments of mathematics and mechanics, chemistry and metallurgy, physics and electronics, aerology, aeronautics, electrical engineering, and mechanical engineering, with a chairman, designated by the superintendent upon the recommendation of the academic dean. A civilian faculty member, who may or may not be a department chairman, is designated as an academic associate with each curriculum officer. He has joint responsibility with the officer in charge of curriculum for the formulation of curriculum course content and sequence at the School and at the cooperating institution.

Curriculum building is in reality a joint endeavor of naval officers, civilian faculty, and cooperating institutions. Each curriculum is reviewed by the academic dean and approved by the superintendent. The strong participation of the civilian faculty in the design of curriculums and the conduct of courses provides the needed continuity that might be lost were these functions left wholly to members of the naval officer staff, whose tour of duty cannot extend beyond a limited number of years.

The civilian faculty is an organized body under the chairmanship of the academic dean and functions as an advisory body on academic matters. An Academic Council, consisting of the academic dean, chairmen of service departments, and the senior officer in charge of curriculum, has complete authority over policies and standards with regard to the granting of degrees.

The following excerpts from the American Council on Education report relating to academic policies and faculty organization will be of interest:

> We must emphasize again that the purpose of the School is to meet the training requirements of the Navy. There is no need for the School to be concerned with civilian academic standards unless the maintenance of such standards will contribute to the training objectives of the institution. For several reasons, however, this Committee believes that the School can accomplish its objectives in the field of scientific and engineering education only by adopting the general educational outlook and approach of a good graduate school.
>
> First of all, while very few of the men who pursue these curricula will ever be engaged in the actual conduct of research, many of them, in their naval career, will be responsible for the administrative direction of research organizations, or for



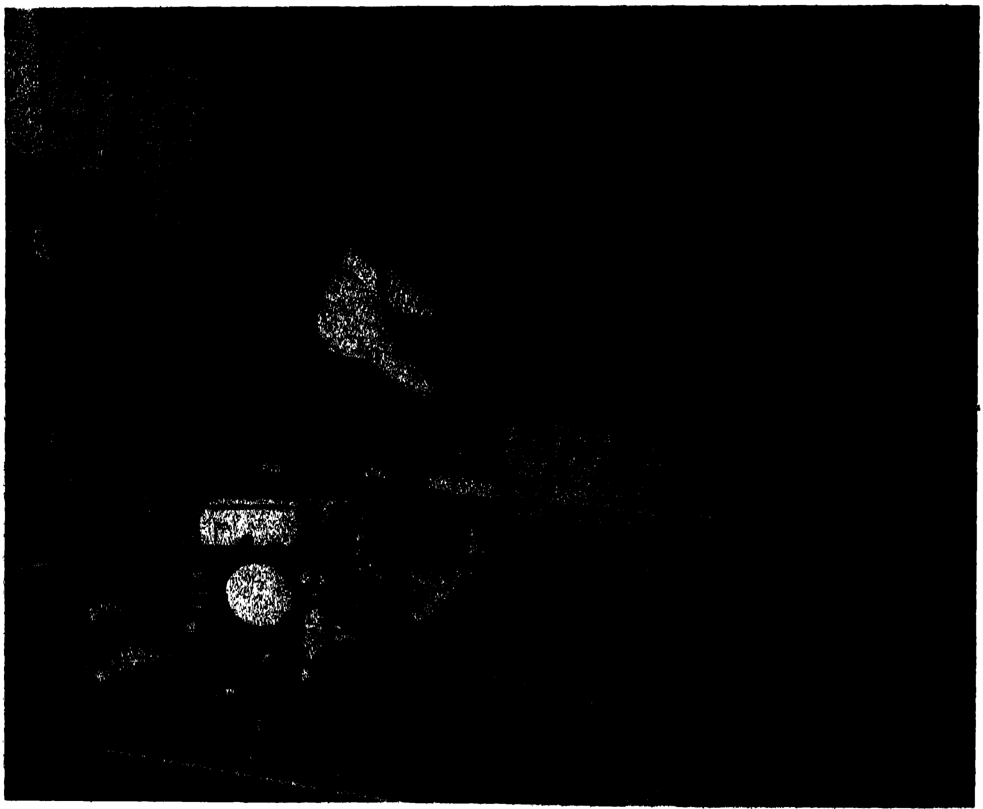
Official U. S. Navy Photograph

PART OF THE INTERNAL-COMBUSTION ENGINE LABORATORY making basic decisions with respect to the research and development policies of the Navy. They will be in strategic positions of liaison between scientific activity and its application to naval problems. On them will depend heavily the Navy's ability to make use of developments in American science and technology. In addition to their naval professional qualifications, these men will need to be possessed of three abilities: administrative capacity, sufficient scientific and technological training to understand the language of the scientist and to appreciate the applications of his work, and a thorough appreciation of the nature of scientific research, of the attitudes of mind and methods of work of the research man, and of the conditions under which research and development work can most fruitfully be carried on. Academic work, as it is conducted in graduate schools is the most effective method—perhaps the only method—of developing the second and third of these abilities.

Secondly, the only way in which the Postgraduate School can secure and retain a Faculty competent to conduct scientific and engineering instruction at an advanced level is by providing the kind of working environment that attracts such men to an academic career.

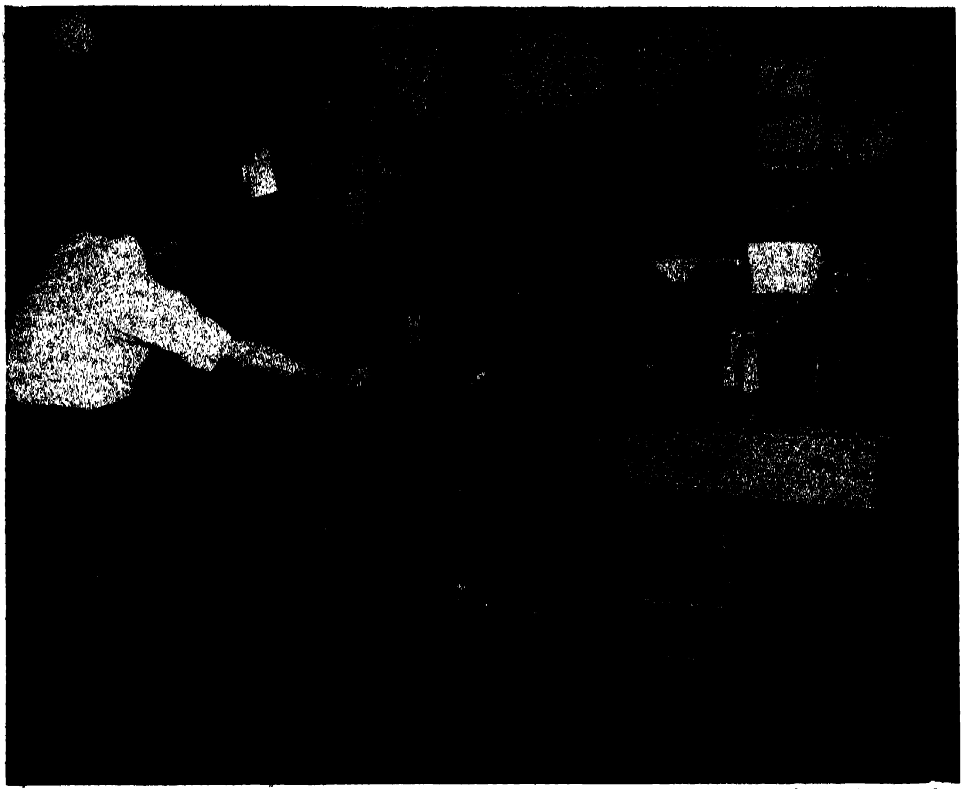
[Concerning the organization of the School, the Committee further states]: The Committee, therefore, believes that the . . . scheme of organization is entirely appropriate, and that the civilian faculty members will have as much freedom and as broad authority and influence over academic policy as can be expected in a school whose primary function is the training of officers for professional duty.

THE present organization and faculty policies have been established by Captain Herman A. Spanagel, USN, formerly head of the Naval Postgraduate School, and at present superintendent of the newly authorized U. S. Naval Postgraduate School. He early recognized the demands that will be made on the School during the postwar period and forecast with unusual foresight the character of school necessary to bring this about. It was fortunate indeed that the Navy Department and its technical divisions had the strong support of as persevering a person as



Official U. S. Navy Photograph

A CORNER OF THE RADAR LABORATORY



Official U. S. Navy Photograph

STUDENT OFFICERS ENGAGED IN HIGH-FREQUENCY RESEARCH PROJECT

Captain Spanagel in launching its expanded program.

An analysis of the postwar continuing needs for graduate training by the material bureaus indicated in early 1945, even before the end of hostilities, that the physical facilities of the existing School could not possibly fulfill the requirements. Captain Spanagel set machinery to work in planning for the future. In this he had the wholehearted support of the Secretary of the Navy and virtually every chief of bureau in the Navy Department. Where legislation was needed to ~~implement plans, the~~ Navy Department sought it of Congress. It is interesting to note that the entire program of the Navy Department for postgraduate education, as proposed by a Board of which Captain Spanagel was Chairman, has been enacted into law by either the present or preceding Congresses.

It is recognized that several years will ensue before the full fruition of the plans laid down. For instance, the present plant facilities at Annapolis will accommodate an uncomfortable maximum of 500 student officers. The ultimate plans, based on the established needs for graduate education in science and technology, call for an annual enrollment, not including those at cooperating civilian institutions, of approximately 800 student officers.

It is planned that the physical plant to be constructed at Monterey will accommodate about 2,900 student officers, distributed as follows: 1,250 in the General Line School, 300 in the Naval Intelligence School, 800 in the graduate scientific and engineering curriculums, and 550 in several forms of applied engineering.

Laboratories will be established in all the fundamental sciences and in the several fields of technology in which both graduate and undergraduate instruction will take place. A staff of several hundred naval officers and civilian instructors will be required.

In order that the faculty may engage in creative work, the Postgraduate School at present offers a rapidly strengthening program of fundamental research and development work for its staff and student officers. The facilities for this work will be greatly expanded in its future home, and it is confidently expected that a reasonable portion of the Navy's vast research program will be allotted to the staff of the Postgraduate School.

Theses are required of all degree candidates either at cooperating institutions or at the U. S. Naval Postgraduate School. These are of an experimental or theoretical nature and are undertaken in an attempt to solve some of the research and development problems of sponsoring technical bureaus or as supplemental solutions to parts of research projects assigned to the School by activities of the Navy Department. Allocation of funds for long-range research programs will be made on a contract or project order basis with the School in much the same manner as the technical bureaus and the Office of Naval Research contract for the services of civilian colleges and universities.

It is anticipated that future appropriations for operation of the U. S. Naval Postgraduate School will carry adequate funds for the conduct of independent research, thus ensuring continuing support of this important function of a graduate school.

The standards for awarding the Master's degree are somewhat stricter than those in a civilian institution. They were purposely made strict in order that there may be no question as to the character of the degree awarded. Authority has been granted by Congress to award the doctorate degree to worthy candidates. While this degree has never been awarded by the School, and if ever awarded will undoubtedly be restricted to only a few special students, much of the work available may be considered of a character that would be sufficiently advanced for doctoral candidates.

The American Council on Education Committee, while advising against the extension of the program beyond the Master's requirements, has this to say about the graduate program conducted at the School: "The Committee finds the regulations governing the award of the Master's degree generally acceptable." Its recommendations concerning the award in general require no adjustment of the standards previously established. In fact, the present requirements might be lowered in certain respects and meet with the recommendations made.

On the other hand, the Committee found in its study that the physical facilities of the present school materially hampered the School in the implementation of its scientific and technical program, and it made many constructive suggestions that, when carried out, will unquestionably give the U. S. Naval Postgraduate School an unusually strong opportunity to lead in the field of advanced and graduate instruction.

It is not anticipated that the growth of the physical plant and the expansion of the faculty of the School will curtail the utilization of cooperating civilian institutions in the Navy's program of graduate training. The contemplated expansion will result, however, in an increase in the number of students and of graduate programs, and in greater versatility in meeting the increasingly diverse needs of the Navy.

GEORGE BROWN GOODE (1851-1896)*

PAUL H. OEHSER
Assistant chief, Editorial Division, Smithsonian Institution.

POSSIBLY no other man connected with the Smithsonian Institution has ever possessed such a marked ability to pick sound and capable assistants as Spencer Fullerton Baird. He was quick to perceive the latent worth of a man. Just as at Cambridge Louis Agassiz, by virtue of his magnetic personality and unique teaching methods, attracted a whole school of American students of natural history, so Baird at Washington gathered around him a coterie of young and promising talent. Baird, of course, was their master, and together they became known as "the Bairdian school," characterized by exactitude in scientific statement, conciseness in deduction, and careful analysis of data, in contrast to many of the older European naturalists whose statements often had to be taken without benefit of documentary substantiation.

There was one man who came to the Smithsonian under Baird's aegis who turned out to be a genius, especially in his broad and humane interests and in the quality and quantity of scientific and literary work that he performed in his brief forty-five years of life. If George Brown Goode had lived, quite likely he would have become secretary of the Smithsonian. David Starr Jordan said: "Of all American naturalists, Doctor Goode was the most methodical, the most conscientious, and, in his way, the most artistic. And of them all no one was more beloved by his fellows." It was, as Secretary S. P. Langley remarked, "incomprehensible that one man could accomplish so much in one single life."

Goode was born at New Albany, Indiana, on February 13, 1851, of sturdy American stock. His mother, Sarah Woodruff Crane, died in the boy's infancy. He passed his early youth at Amenia, in eastern New York, where his father had moved after retiring from his mercantile business. We are told that Francis Collier Goode, the father, was of studious habits and keen observation and that he bore a strong facial resemblance to Henry Clay. Though he lacked a professional education, he was well informed. A friend said of him: "Few public men are better acquainted with the political history of the United States, and few naturalists more enthusiastic lovers of nature, or more familiar with the habit and appearance of plants." These characteristics he passed on to his son. It is of interest, too, that in his library he had a set of "Smithsonian Reports," and from these young George first became attracted to science and learned of the Institution he was later to serve. It seemed to be an instance of boyhood dreams come true.

In 1870, when only a little past nineteen, Goode was graduated from Wesleyan University at Middletown, Connecticut, with a decided penchant and talent for natural history. The same year he began postgraduate work at Harvard College under Louis Agassiz and so distinguished himself with the professor that he was recommended for the position of curator of the university museum in the Judd Hall of Natural Science at Middletown. This building had just been presented to Wesleyan University by Orange Judd, the pioneer agricultural journalist and Goode's future father-in-law. Goode undertook the arrangement of the natural-science collections and gained therefrom an experience in museum administration and techniques that was to prove a solid foundation in later years. He was then only about twenty years old, a stout and healthy lad, prodigiously doing the work of an older man and doing it well.

*** This article will form part of a chapter in the author's book, a biographical history of the Smithsonian Institution, to be published by Henry Schuman, Inc., New York, in "The Life of Science Library." Photographs, courtesy of the Smithsonian Institution.**

His first appearances in print consisted of various short notes in the *College Argus* and *College Review*, but his maiden scientific paper, "The Billfish in Fresh Water" (i.e., the Connecticut River), appeared as a brief note in the *American Naturalist* in September 1871. At the Portland meeting of the American Association for the Advancement of Science in 1873 he presented a paper on the question "Do Snakes Swallow Their Young?" and this, Goode's first important contribution to zoology, was published the next year in the *Proceedings* of the Association.

In 1872, through the influence of his stepmother, he met Professor Baird at Eastport, Maine—a meeting that proved to be a turning point in his life. The following year, at the Portland meeting of the A.A.A.S., the two met again, and Baird, at once recognizing Goode's genuine potentialities, invited him to participate in the work of the United States Fish Commission, which was then beginning to take shape under Baird's organizing genius. The result was that the young man began a volunteer service for the Commission that lasted several years, during which he collected fishes along the eastern coast, wrote papers on what he was learning about them, and became not only a specialist in technical ichthyology but also a student of the economic problems relating to fisheries and the fishing industry. Upon Baird's invitation he spent the winter of 1873 in Washington arranging the fish specimens at the Smithsonian. Here he met Secretary Joseph Henry and became one of the small group of young scientists who lived in the Smithsonian Building and partook of the Henrys' hospitality and encouragement.[†]

Although Goode kept his official connection with Wesleyan University until 1877, the Fish Commission and the Smithsonian gradually claimed him. He was given the title curator of the museum. In 1876 appeared his first publication under the Smithsonian imprint—*Catalogue of the Fishes of the Bermudas*, a bulletin (No. 5) of the National Museum. This was the beginning of two decades of unremitting hard work for the

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[†] In the old days, during the Joseph Henry-Spencer Baird regime, the hospitality of the Smithsonian Institution extended even to furnishing living quarters for scientific workers it wished to aid and encourage. Especially if the man had no family, he would be allotted one or more of the unused rooms, perhaps in one of the towers, of the then new, brownstone, Gothic-like Smithsonian Building. The geologist Fielding Bradford Meek, for example, lived in a north-tower room from the time he went to Washington in 1858 until his death in 1876. Frank Hamilton Cushing, the eminent ethnologist, joined the Smithsonian staff in 1875 and was assigned to rooms in one of the south towers. His friend George Kennan has provided a firsthand account of the sometimes exciting life in the building. He writes (in the *Medina* (N. Y.) *Tribune*, December 27, 1923): "In 1878 when I went to Washington to enter the service of the Associated Press, Frank was living near the top of the south tower of the Smithsonian building south of Pennsylvania Avenue. As he had two rooms and two beds, he asked me to come and stay there until I should find a boarding place. . . . I stayed with Frank for several months, partly because it was a pleasure to be with him again, and partly because Dr. Bessels, the scientist of the Hall Arctic expedition, also had a room in the building, and he was another interesting character. Frank and I got our dinners at the restaurant on Pennsylvania Avenue widely known to old Washingtonians as 'Harvey's'; but our other meals we took in our rooms. . . .

"The room under ours at that time was the workroom of the well known ornithologist Robert Ridgway. It contained hundreds of skins of birds which had been treated with benzene to protect them from insects, and the floor was covered with fragments of wooden boxes, wrapping paper, excelsior, and other combustible material used in packing. The whole room was as inflammable as a powder magazine. One morning Frank happened to want more heat than he could get over a mere cooking lamp, and he thought he would go down and make a fire in Mr. Ridgway's stove. Not noticing on the floor an open can of benzene which the ornithologist had been using, he struck a match. Instantly the vapor in the air flashed out and set fire to the benzene in the can, which sent up a column of flame four or five feet high. In half a minute the can would have been melted apart and the burning benzene would have run all over the tinder-like stuff on the floor. Frank, with perfect self-possession, lifted the blazing can between his hands, carried it steadily across the room without spilling a drop, and set it down in a solid iron coal-scuttle, where it burned itself out without setting fire to anything else."

It may have been that Cushing's miraculous presence of mind was inspired by stern fire warnings, for it had been but a dozen or so years before that there had occurred a disastrous fire in the Smithsonian Building which destroyed many of James Smithson's personal relics, valuable records and files, and most of the John Mix Stanley collection of Indian paintings.

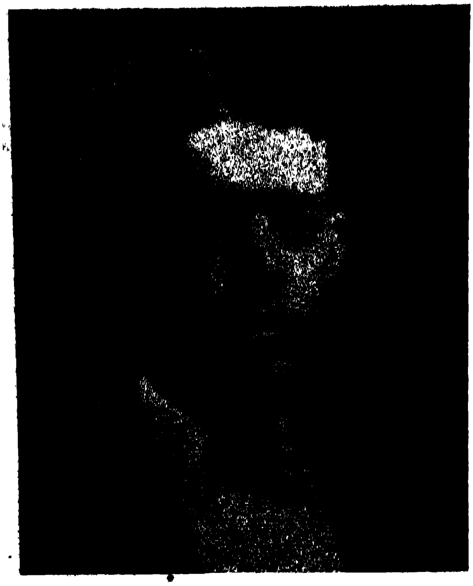
Institution to which he became singularly devoted.

On November 27, 1877, he was married to Sarah Ford Judd, only daughter of Orange Judd and a woman who shared his love of nature. Their marriage was a happy one. As Secretary Langley has recorded, Goode was "eminently a domestic man, finding the highest joys that life brought him with his family and children." This devotion to family extended to a keen interest in genealogy, especially that of the Goode family, and impelled him to work twenty-five years preparing his book *Virginia Cousins,* which, as the title-page painstakingly explains, was "A Study of the Ancestry and Posterity of John Goode of Whitby, a Virginia Colonist of the Seventeenth Century, with notes upon Related Families, a Key to Southern Genealogy and a History of the English Surname Gode, Goad, Goode, or Good from 1148 to 1887." This work, begun as a hobby, turned out to be a fine and model piece of genealogical scholarship. He saw the sociological and historical significance of genealogies, and accordingly was determined that the history of the Goode family should be as nearly perfect as it was in his power to make it. "Every man," he quotes Emerson as saying, "is a bundle of ancestors," and then inserts a special excursus "for the future use of the four little bundles at this moment sheltered beneath his roof-tree," referring to his four children, a daughter and three sons, the eldest then about nine years of age. In Washington the family had a home on Lanier Heights, on a street overlooking the area of the Rock Creek Valley that was then being developed into the National Zoological Park.

One of the first big jobs that Goode undertook after becoming established at the Institution was to help Secretary Baird organize and execute the Smithsonian and Fish Commission exhibit at the Philadelphia Centennial Exposition in 1876. They saw in this Exposition a great opportunity to bring the scientific work of the Federal government to the attention of thousands of people. They wanted to show especially what a great national museum could mean to a nation, culturally as well as scientifically. For this was, in fact, a crucial time for the National Museum. Although the Smithsonian from its very beginning had been delegated to provide for a national museum, Congress had not been overgenerous in appropriating funds for its upkeep. This treatment was particularly unfortunate in view of the great influx of material to be cared for resulting from the United States Exploring Expedition under Captain Charles Wilkes and from the explorations of our Western Territories. Only about $4,000 a year was being appropriated to care for the collections; there was therefore little chance of expansion; and in 1858 Secretary Henry said: "It will, therefore, be the policy of the Institution, unless other means are provided, to confine the collections to illustrations of the products of the North American continent." In 1859 a guidebook was published with the title "Guide to the Smithsonian and National Museum," and about the same time the words "National Museum of the United States" were painted over the door of the exhibition hall. But actually the name "National Museum" had never been given legal sanction by Congress,

GEORGE BROWN GOODE

THIS PICTURE WAS TAKEN WHILE HE WAS ASSISTANT SECRETARY OF THE SMITHSONIAN INSTITUTION.



GEORGE BROWN GOODE

DURING STUDENT DAYS AT WESLEYAN UNIVERSITY.

and this was not to happen until the erection of the new museum building following the close of the Philadelphia Exposition.

The future of the National Museum was close to the hearts of Baird and Goode, and they were quick to take advantage of the publicity afforded by the Centennial Exposition. As Langley has recorded, Baird was—

> determined that the exhibition made by the United States should bear witness to what the Museum could do. . . . The Government made a loan of several millions to the Exposition, which no one then supposed would ever be repaid. Members of the appropriations committee felt quite safe in jokingly assuring Professor Baird that if the money was repaid an appropriation for a National Museum building should not be withheld. The entire staff of the Museum, including several unpaid volunteers, with Goode at their head, gave all their energies for nearly a year to make the Government and especially the Museum exhibit a success, feeling that the future of the Museum was at stake. Individuals all over the country were called upon to assist by advice or material in their special lines. Thousands of letters were written and thousands of exhibits gathered. Here Goode had his first training in the arts of exhibition, in which he finally became the acknowledged master. Many were the discussions as to system, selection of exhibits, causes, labels, and method in general. It was indeed a liberal education to those engaged in the work. . . . The result was a glorious success, acknowledged by all beholders.

The part of the exhibit that Goode directed was unified under the general theme "Animal Resources of the United States." He prepared an elaborate classification and catalogue of all the objects that made up the display. In addition, there was an exhibition of minerals and another of ethnologic materials.

Important, too, was the fact that Goode was kept thinking seriously and deeply on the problem of museum organization and administration, so that when the new museum building was completed in 1881 he was ready with a well-considered plan of what a public museum ought to be. By that time he had been made assistant secretary of the Smithsonian, in direct charge of the United States National Museum, and was recognized as a leading museum authority. And Secretary Baird gave him free rein in developing his ideas. A national museum, he believed, should serve a threefold function. First, it should be *a museum of record,* "in which is preserved the material foundation of an enormous amount of scientific knowledge." Second, it should be *a museum of research,* "which aims to make its contents serve in the highest degree as a stimulus to inquiry and a foundation for scientific investigation." Third, it should be *an educational museum,* "through its policy of illustrating, by specimens, every kind of natural object and every manifestation of human thought and activity." This was the ideal he set up for the National Museum, which he directed.

But he went farther than that. He developed a philosophy of museum administration that was broad and sound and made the conduct of a museum virtually a science in itself. And in this he demonstrated his originality, thoroughness, foresight, and organizing ability that made him a genius in his line. Many of the principles he laid down and the organization plans he codified are still effective today. The secret perhaps was, as it has been in many another man's success, that he believed wholeheartedly in what he was doing—he was certain of the efficacy of the museum as a cultural agency. He summed up his principles in these positive words: "The degree of civilization to which any nation, city, or province has attained is best shown by the character of its public mu-

THE APPROACH TO THE SMITHSONIAN BUILDING IN GOODE'S DAY

seums and the liberality with which they are maintained."

Possibly we need to be reminded that museum exhibition techniques and methods have changed considerably since Goode's day. As Clifford C. Gregg recently wrote in the *Museum News:*

> Our emphasis was then on cataloging and arranging materials, classifying them and describing them. We achieved orderly and, in many cases, attractive displays. Our interpretation was through descriptive labels in our exhibition collections, and through publication of our scientific studies. This procedure was, I believe, in keeping with the times, and in certain instances perhaps ahead of its time. However, times change. The cultural level of the general public and its knowledge within the fields of our operations have increased considerably.

The aims and principles of museums, as formulated by Goode, are still the same. The program is still "research, record, and the dissemination of knowledge."

Goode differed from many museum administrators in being able to bring to bear upon his work a phenomenal breadth of learning and variety of interests. He was a true scholar, one who was not only able to apply himself with distinction to a specialized field (in this instance ichthyology), but also to see the necessity for the integration of all knowledge of whatever kind and from whatever source; and certainly in him the aspirations of James Smithson found a staunch protagonist. To quote Langley again:

> He had a strong interest in literature, and wrote in an excellent English style—clear, direct, and unpretentious. I have never met a mind in touch with more far-away and disconnected points than his, nor one of the same breadth and variety of writing, outside the range of his own specialty. He had fine aesthetic tastes and derived keen enjoyment from everything that was beautiful in nature or in art. He knew all natural sights and sounds, and recognized the note of every bird. . . ."

I have talked with men who knew George Brown Goode, and with one accord they have characterized him in the highest terms. They remembered him, after fifty years, as a man of great and generous spirit, of sterling and loyal character. He apparently possessed that none-too-common ability to make people like him and at the same time to maintain firm control over those under him. "He had a hearty way of meeting associates . . . ," said Theodore N. Gill, a colleague at the Institution, "but in spite of his gentleness, firmness and vigor in action became manifest when occasion called for them."

GOODE'S contributions to science were manifold. His first loyalty in line of research was to the study of fishes, and in spite of his increasing administrative duties he turned out dozens of substantial ichthyological papers, many of them published in the *Proceedings* and *Bulletins* of the National Museum and many of them written in coauthorship with Tarleton Hoffman Bean, who was then the Museum's curator of fishes.‡ The culmination of this collaboration was the publication of *Oceanic Ichthyology,* a treatise on the deep-sea and pelagic fishes of the world, based chiefly on the collections made by the steamers *Blake, Albatross,* and *Fish Hawk* in the northwestern Atlantic. This work comprised two large quarto volumes, which appeared the year of Goode's death, 1896, as a *Special Bulletin* of the National Museum. It was, as David Starr Jordan said, "Goode's most important scientific study" and showed "a distinct advance over all other treatises of abyssal fishes. . . ." In it 156 new species and 55 new genera of fishes belonging to the Atlantic abyssal fauna were named for the first time.

Only once did Goode try his hand at purely popular writing on fishes. This was his book *American Fishes: A Popular Treatise upon the Game and Food Fishes of North America, with Special Reference to Habits and Methods of Capture,* published in 1888 and widely

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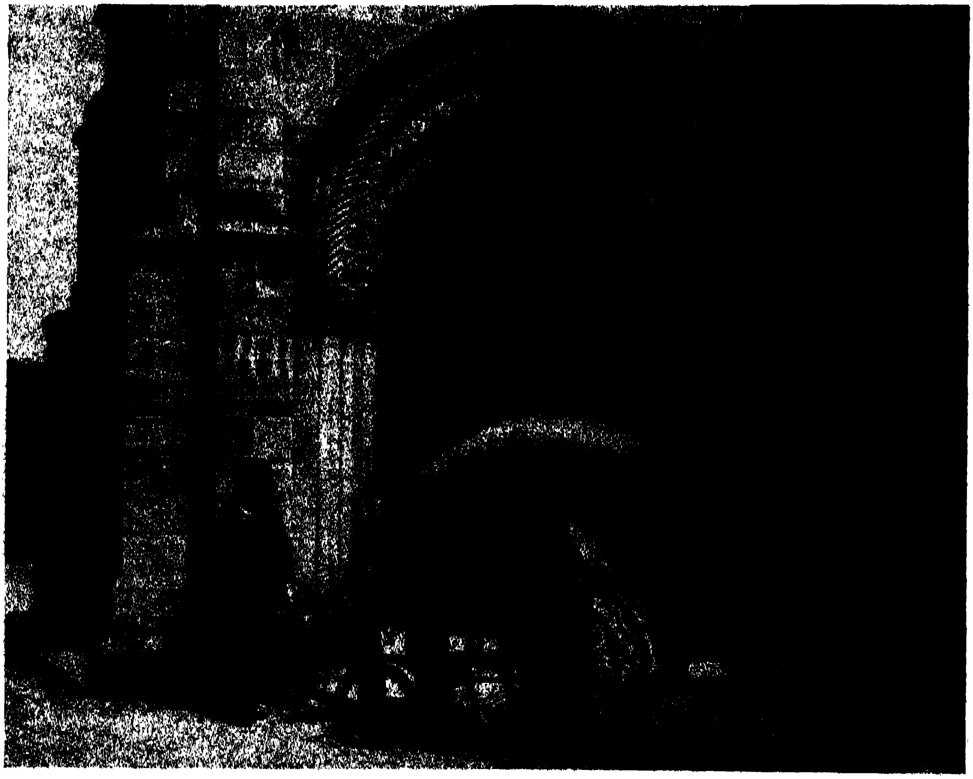
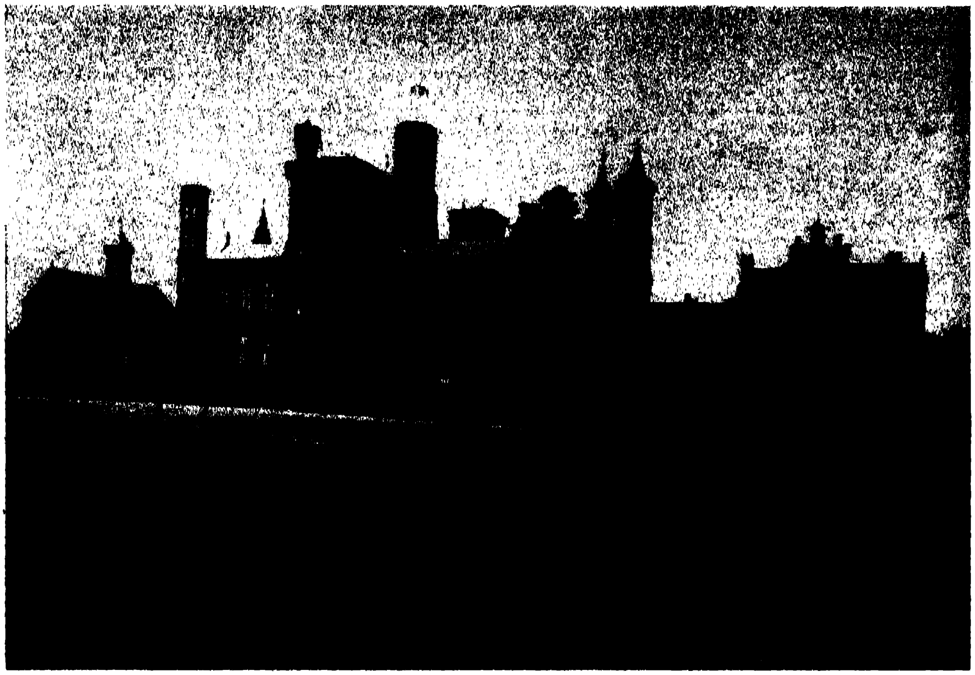
‡ In Dean's *Bibliography of Fishes* I count 124 titles by Goode alone, 39 by Goode and Bean, and 14 by Goode and others.



PORTE-COCHERE OF THE SMITHSONIAN BUILDING, ABOUT 1890

THE VEHICLE SHOWN IS THE COPELAND STEAM TRICYCLE, THE ONLY MACHINE OF ITS KIND EVER BUILT.



AN OLD PICTURE OF THE BACK OF THE SMITHSONIAN BUILDING

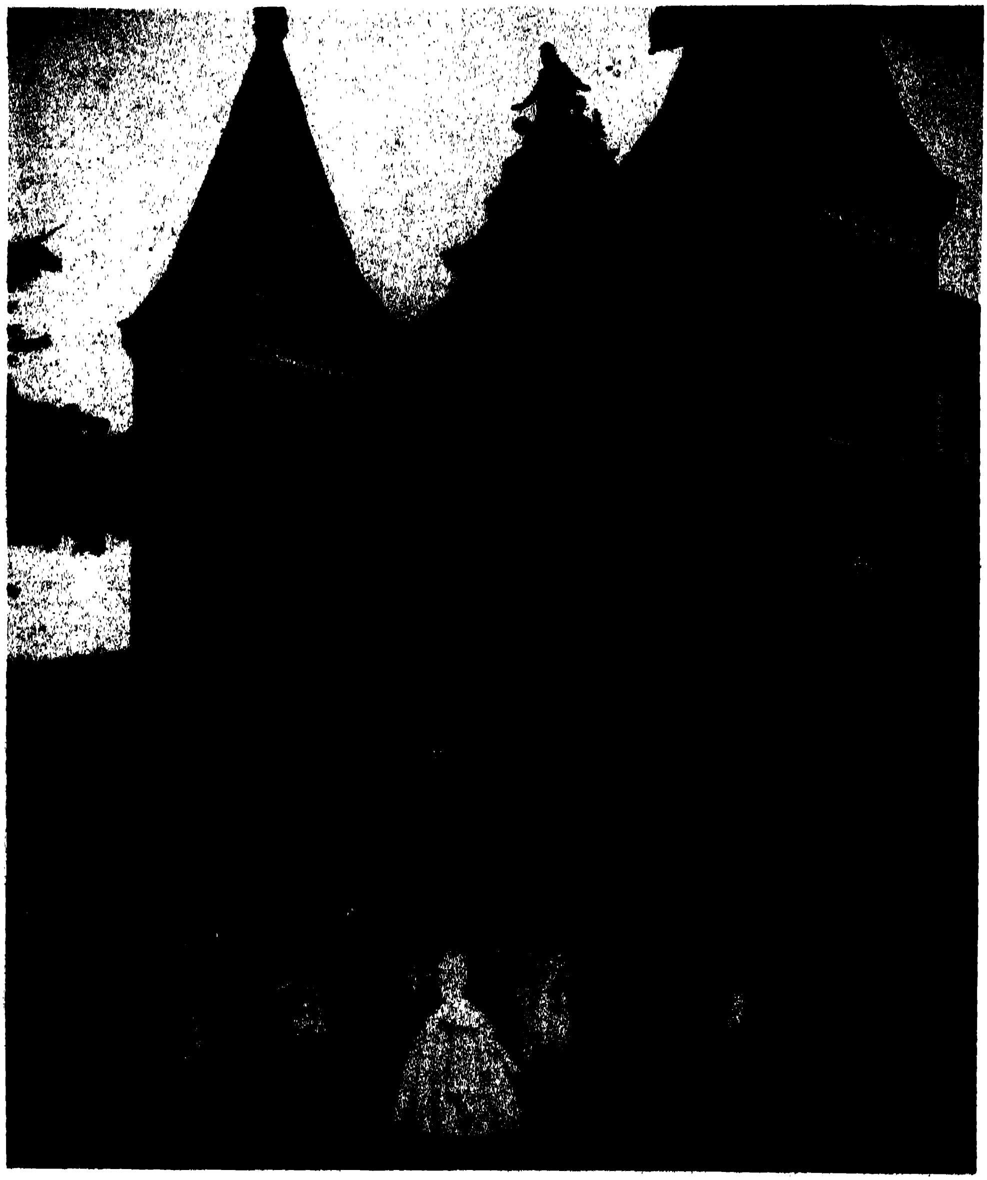
IN THE PENS IN THE CENTER OF THE PICTURE LIVE ANIMALS WERE KEPT BEFORE THE NATIONAL ZOOLOGICAL PARK WAS ESTABLISHED. TODAY THE OPEN AREA OF THIS PICTURE IS OCCUPIED BY SMALL BUILDINGS.

reviewed at home and abroad. In this book he showed himself a literary artist as well as a scientist and succeeded in his expressed ambition to write a book that "people would buy and read" rather than technical reference works, however fundamental, for specialized and esoteric readers.

But Goode ultimately perhaps will be remembered as a historian and organizer of science rather than as a scientist. It was well enough, he thought, to catalogue biological specimens, to work over collections and prepare taxonomic treatises, to map the distribution of forms of animal life over the earth, or in other ways to add little by little to the sum total of our knowledge of natural phenomena, but science too needs a synthesis. It must be correlated with the past, and the record of past achievement must be preserved and interpreted for the future. "The history of a science is the palladium of its freedom; it prevents it from being tyrannized over by narrow bigoted viewpoints." Goode would have concurred in this motto, which appeared with a lithographed portrait of Ludwig Choulant in 1842; and for the epigraph of one of his essays he quoted Herbert Spencer: "Is not science a growth? Has not science, too, its embryology? And must not the neglect of its embryology lead to a misunderstanding of the principles of its evolution and of its existing organization?" Goode was the one of his generation of American scientists who saw this most clearly.

On January 12, 1887, Goode was made assistant secretary of the Smithsonian, with virtually full responsibility for the direction of the National Museum; and in the nine brief years that were left to him he was to bring out (in addition to voluminous other material) six or eight papers on the history of science that were to prove classic. The first of these, "The Beginnings of Natural History in America," he had already delivered the previous February as a presidential address before the Biological Society of Washington. Beginning with Thomas Harriot, the earliest English naturalist in

America, he traced the development of American natural science to the end of the Revolutionary War, a period of nearly two hundred years, and put on record a mass of historical data indispensable to later historians of science. In a second paper, "The Beginnings of American Science: The Third Century," delivered before the Biological Society in 1887, he carried forward this history to his own day. The next year he read his essay "Museum-History and Museums of History" before the American Historical Association meeting in Washington, which was followed in 1889 by "The Origin of the National Scientific and Educational Institutions of the United States," presented to the same As-



THE OLD NATIONAL MUSEUM BUILDING

IT WAS BUILT IN 1879 DURING GOODE'S FIRST YEARS WITH THE SMITHSONIAN INSTITUTE

THE OLD NATIONAL MUSEUM BUILDING DURING CONSTRUCTION

From left to right: GENERAL M. C. MEIGS, U.S.A.; GENERAL WILLIAM T. SHERMAN, SMITHSONIAN REGENT AND CHAIRMAN OF THE BUILDING COMMITTEE; PETER PARKER, REGENT; SPENCER FULLERTON BAIRD, SECRETARY OF THE SMITHSONIAN; ADOLF CLUSS, ARCHITECT; W. J. RHEES AND DANIEL LEECH.

sociation. Finally he published three essays which contained his mature ideas and philosophy with respect to museums—"The Museums of the Future," in 1889; "The Genesis of the United States National Museum," in 1893; and "The Principles of Museum Administration," in 1895.[§] After Goode's death, all these papers were collected and reprinted in the *Goode Memorial Volume*, published (in 1901) as Part 2 of the "Report" of the National Museum for 1897. This volume, because of Goode's contributions, is one of the most important ever published on the history of American science.

Goode's devotion to science history manifested itself in one other respect—his unusual interest in bibliography. He knew as well as anyone the drudgery connected with the compilation of indexes and bibliographies, yet he realized that they are the indispensable tools of the research worker and that there is an obligation laid upon every generation of scientists and scholars to provide works of this character. He would have agreed wholeheartedly with Pope's oft-quoted couplet:

> Index-learning turns no student pale
> Yet holds the eel of science by the tail.

As early as 1874 he began collecting materials for "An Index Bibliography of American Ichthyology," in which he planned to "enumerate by title every writing ever published which refers to American fish or fisheries." This was indeed a gigantic undertaking, and he did not live to see it completed.[**] He inaugurated a series called "Bibliographies of American Naturalists," several of which were published as bulletins of the National Museum. Three of them—

[§] L. V. Coleman, director of the American Association of Museums, tells me that this essay has recently been reprinted in South Africa.

[**] Goode's labors on this index were not wasted, however, for in 1910 when Bashford Dean was compiling his monumental *Bibliography of Fishes* the Smithsonian made Goode's index available to him for incorporation in his work. Dean's *Bibliography* was published by the American Museum of Natural History: Vol. 1 in 1916, Vol. 2 in 1917, and Vol. 3 in 1923.

Photo by T. W. Smillie

THE INDIAN EXHIBIT AT THE PHILADELPHIA CENTENNIAL EXPOSITION OF 1876

bibliographies of Spencer Fullerton Baird, Charles Girard, and Philip Lutley Sclater—appeared under Goode's name, and he had begun work on two others, those of Theodore Nicholas Gill and David Starr Jordan, both renowned ichthyologists. These bibliographies were no mere trifling lists, but were scholarly, annotated, and well indexed. The one of Baird, published in 1883, alone aggregates nearly 400 printed pages and still stands as an invaluable source in the study of the career of the Smithsonian's second secretary.

Goode's interest in history even outside the realm of science was not that of a dilettante. Besides his great book on genealogy, *Virginia Cousins,* already mentioned, he wrote "The Literary Labors of Benjamin Franklin," delivered as an address before the American Philosophical Society. He was a member of the council of the American Historical Association and was instrumental in getting through Congress the Act of its incorporation (1889), under which the Association's reports are published by the United States Government. According to law, these reports are transmitted to Congress through the secretary of the Smithsonian Institution. He held membership in the Virginia Historical Society, the Columbian Historical Society of Washington, and the Southern Historical Society. He helped to organize the Sons of the American Revolution. He assisted also in the organization of the Daughters of the American Revolution and designed their seal. Furthermore, he wrote extensively on the history of the Smithsonian itself, and edited and wrote much of the large volume published the year after his death commemorating the fiftieth anniversary of the founding of the Institution.

All these interests and talents added up to make G. Brown Goode the consummate museum director and a godsend to Secretary

Baird, who was able to place full trust and responsibility in his assistant secretary. Every museum man ought to have the collecting instinct, and Goode had it to a remarkable and refined degree. But he collected always against the background of his broad museum philosophy, that a museum should not be a mass of collections per se, but "a house full of ideas and a nursery of living thought." And it was not only in his own field of natural history that his genius for collecting displayed itself.

> He would [said Langley] bring back from every exposition which he attended methodical collections, frequently overlooked by others . . . not of miscellaneous objects, but of a series which could themselves be placed on exhibition. These might be musical instruments, ecclesiastical art, early printed books, medals, or ivories,

all selected with taste and discrimination. During his regime the number of specimens in the Museum increased from about 200,000 to more than 3,000,000. This increment might well have resulted in chaos had it not been for Goode's organizing ability and his definite aims for his organization.

Honors and responsibilities came to Goode one after another. He served as commissioner for the United States government at the International Fisheries Exhibition in Berlin in 1880 and in London in 1883. He represented the Smithsonian at all the great expositions in this country in the decade from 1884 to 1895 and was a commissioner and for a time acting commissioner-general to the Columbia Exposition at Madrid in 1892.

Goode's method of assembling an exhibibition, described by Langley, is revealing:

> The plans of the floor space, the cases, the specimens were all carefully arranged in advance. Boxes were especially made of lumber which could be utilized for cases or platforms. Cases were marked, and not very long before the opening of the exposition the entire mass would be deposited on the bare space assigned to the Smithsonian exhibit. Usually other exhibitors had their material half arranged by this time, and the fear was expressed by sympathetic bystanders that the Smithsonian would not be ready. The cases would be unpacked and the specimens put in them in whatever position they happened to stand, and up to the last day all would seem to be in confusion; but Doctor Goode knew his resources and his men as a general knows his army. Suddenly all detailed work would come to an end, and in the course of a few hours, as if by magic, the entire exhibit would be put in place.

Goode seemed to take great pride and delight in thus making order out of chaos. He never repeated an exhibition. He received many awards, diplomas, and medals at the various expositions, including the Order of Isabella the Catholic from the Spanish government for his work at the Madrid Exposition.

He served as president of the Biological Society and the Philosophical Society of Washington and was a member of the American Philosophical Society, the National Academy of Sciences, and several foreign scientific societies. He was one of the founders in 1888 of the National Geographic Society, which was to grow in fifty years from a small local scientific organization of 200 members to a great national institution of more than a million and a half members. He was elected in 1881 a member of the Cosmos Club of Washington and, like his Smithsonian colleagues Baird and Powell, served a term as its president.

Gradually, however, the burden upon Goode's shoulders became more than one man could bear, and though still a young man he broke under the physical strain. All who knew him have remarked how surely he "burned himself out." On September 6, 1896, he died at his home in Washington, at the age of forty-five years; but into those years had been crowded the work of a dozen ordinary men. With his death a golden age of natural history came to an end. There were, of course, many of the old-time naturalists still alive, but the age of acute specialization in science had set in, and perhaps never again could there be a man like George Brown Goode.

He was buried in Oak Hill Cemetery in Georgetown, D. C., not far from the tomb of Joseph Henry and that of his beloved mentor, Spencer Fullerton Baird. Today his unmarked grave may be found on inquiry from the caretaker; but perhaps he needs no other monument than the solid and enduring part he played in guiding the destiny of the Smithsonian Institution and its ward, the National Museum.

TIME

ADVANCEMENT THROUGH THE YEARS

GEORGE G. ENSIGN

Mr. Ensign, who has had extensive and varied experience in construction and steam-power engineering as well as in precision instrument manufacturing and research, received his Master of Science degree at Iowa State in 1933. He is now director of research for the Elgin National Watch Company.

ANY article about the timing device worn on your wrist or carried in your pocket—whose accuracy more often than not is just taken for granted—must of necessity go back through the centuries to those early scientists whose primary discoveries have been refined and included in the timepiece of today.

Although hardly comparable to watches of the modern era, developments in this field began several hundred years ago. In 1370, the first clock with wheels and pinions was made by a Hollander named Henry de Vick. It was subsequently presented to Charles V, King of France. The clock was a rough, heavy machine driven by a weight of more than five hundred pounds. It was regulated by a bar, which vibrated back and forth in a horizontal plane. There was only one hand on this clock, and it pointed to the hours. The name "clock" itself was taken from the bells, *glocken*, or *cloches*, that were used to strike the hours.

It was not until the sixteenth century that watches, or "pocket clocks," first came into existence. Peter Henlein, a locksmith living in Nuremberg, Germany, is generally credited with making the first watch in 1504. It was spring-driven, made entirely of iron, and stood about six inches high. The famous "Nuremberg Egg" was made about 1550, and many writers have confused this timepiece with the one built by Henlein. There is a difference, however, in that Henlein's timepiece was drum-shaped, whereas it seems likely that the first egg-shaped watches appeared shortly after Henlein's death in 1542. It is interesting to note that these watches sold for about $1,500 in terms of today's currency. The motive power came from a coiled spring. The derivation of the term "watches" seems to come from the fact that night watchmen carried these timepieces when the price was reduced.

Gradual progress was made in improving watches during the next three hundred years. By 1687, watches had become smaller and had acquired minute hands. It took ninety-three more years to achieve second hands. Undoubtedly, the main reason why there were no minute or second hands on timepieces prior to this time was the general inaccuracy of the watches of that period, caused by the fact that lever escapements were not then being used. Watches had no balance spring or hairspring until 1661, when a crude hairspring was manufactured from pig's bristle. Dr. Robert Hooke, of England, made the discovery as a means of applying the pendulum principle to watches. The pig's bristle that he used for this purpose was described as "a tender, straight spring, so affixed that one end thereof plays backward and forward with the balance."

One of the most important advances in watchmaking came in 1704 when Nicholas Facio and Peter and Thomas Debaupre were awarded a patent covering the art of piercing rubies and other precious stones for use as bearings. Another important achievement came in 1740 when Benjamin Huntsman of England developed the crucible steel process for making steel mainsprings. The crucible steel process has since been used by many industries requiring carbon steel of the highest quality.

The watch, considered as an instrument, had reached a stage of high development by

1800. Watches made one hundred and fifty years ago will still compare favorably with many more recently invented instruments in regard to degree of progress. This may be attributed to the interest shown by early scientists such as Hooke, Huygens, Newton, and Airy and the attraction of watchmaking for the most skilled mechanics of that day. The watch was a challenge to these scientists and artisans because it was the most complicated mechanism of that period and required extraordinary workmanship.

The precision required in watchmaking gave the profession the standing of an art, and it has been so considered until the past twenty years. The methods of mass production and the use of interchangeable parts were until recently not applicable to the watch industry. Each watch required individual adjusting and fitting of parts. Through the improvement of gauging techniques and inspection procedures, most modern watch manufacturers have achieved interchangeability of parts, and assembly lines are beginning to appear in the industry. This progress is one of the most significant developments in watchmaking. It is a story in itself, but an idea may be had by considering the problem of the manufacture of more than one hundred tiny parts, which must fit interchangeably into a movement the size of a dime.

THIS has not been an attempt to tell the romantic story of watch development through the years, but merely to present a background against which to consider certain modern developments of this industry. Research in the development and improvement of the watch is at present increasing in scope, so that new and better ideas and improvements are appearing at an increased rate and may be expected to continue through the years.

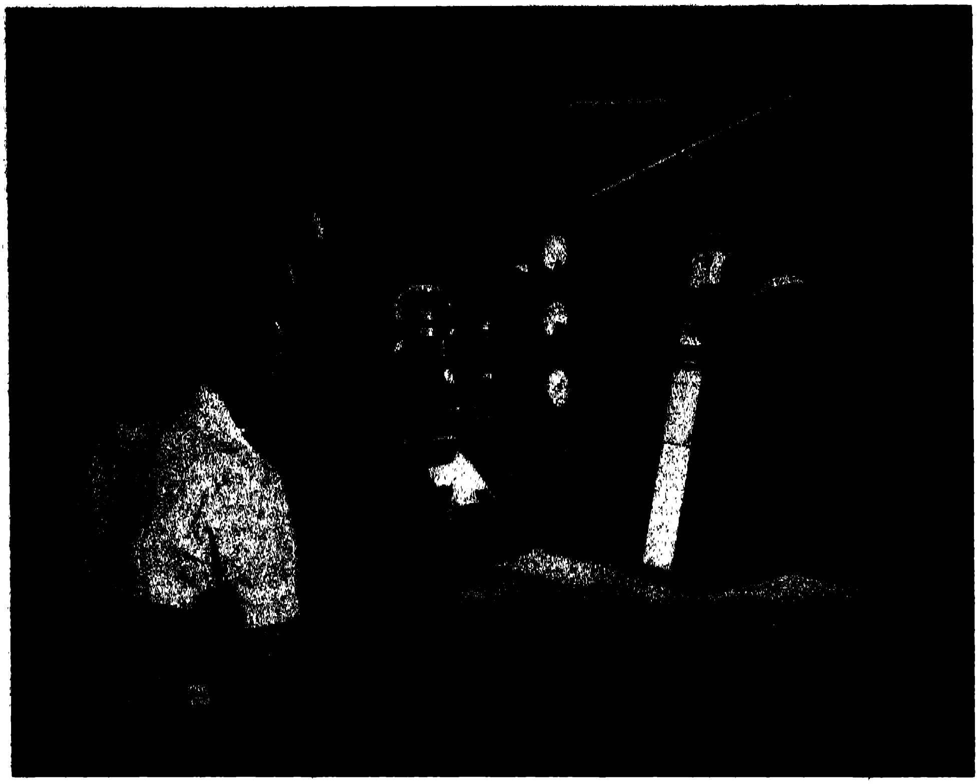
The jeweled wrist and pocket watch as we know it today is composed of well over one hundred parts. Of these, the most important is the hairspring-balance combination, which has often been termed the "heart" of the watch. The development of the first hairspring from pig bristle was considered a radical change when it was announced because, until that time, no device having a definite period of oscillation had been produced for portable timepieces. Before that time, accurate pendulums were used that operated on the principle of gravity, but these were suitable only for stationary clocks. It is easy to see, of course, what a tremendous impetus the development of the hairspring gave to accurate watch performance. If it had not been for this development, the watch would still be a cumbersome, inaccurate instrument.

NEW MAINSPRING MATERIAL

THESE TWO MAINSPRINGS WERE EXPOSED TO WATER-SATURATED AIR FOR 18 DAYS. THE ONE ON THE LEFT, MADE OF A NEW ALLOY, IS RUST-FREE; THE CARBON STEEL SPRING ON THE RIGHT IS RUINED.

The modern developments in watch hairsprings and balances may be summarized in several major steps. First, there was Hooke's pig-bristle spring and solid metal balance. Then came the solid balance, with metal hairspring but without temperature compensation. This was unsatisfactory because, as temperatures rose, the balance expanded and became larger and the hairsprings became weaker, so that the watches slowed down. Conversely, when temperatures went down, the balance became smaller and the hairspring stiffer, so that the watches ran faster. Watches, as a result of temperature changes, thus tended to run slower at high temperatures and faster at low temperatures.

The next major step in the evolution of the



PRETIMING A HAIRSPRING-BALANCE ASSEMBLY

IN THIS VIBRATING MACHINE THE LENGTH OF THE HAIRSPRING IS ADJUSTED TO THE MOMENT OF INERTIA OF AN INDIVIDUAL BALANCE SO THAT EACH UNIT WILL KEEP TIME WHEN PLACED IN A WATCH MOVEMENT.

hairspring balance introduced a balance rim composed of bimetallic elements, similar to those used in thermostats. In the bimetallic balance, the metal with the largest coefficient of expansion is placed on the outside, so that with increases of temperature the ends of the rim will be caused to spring inward toward the staff, thus reducing its moment of inertia and compensating for the loss of stiffness of the hairspring.

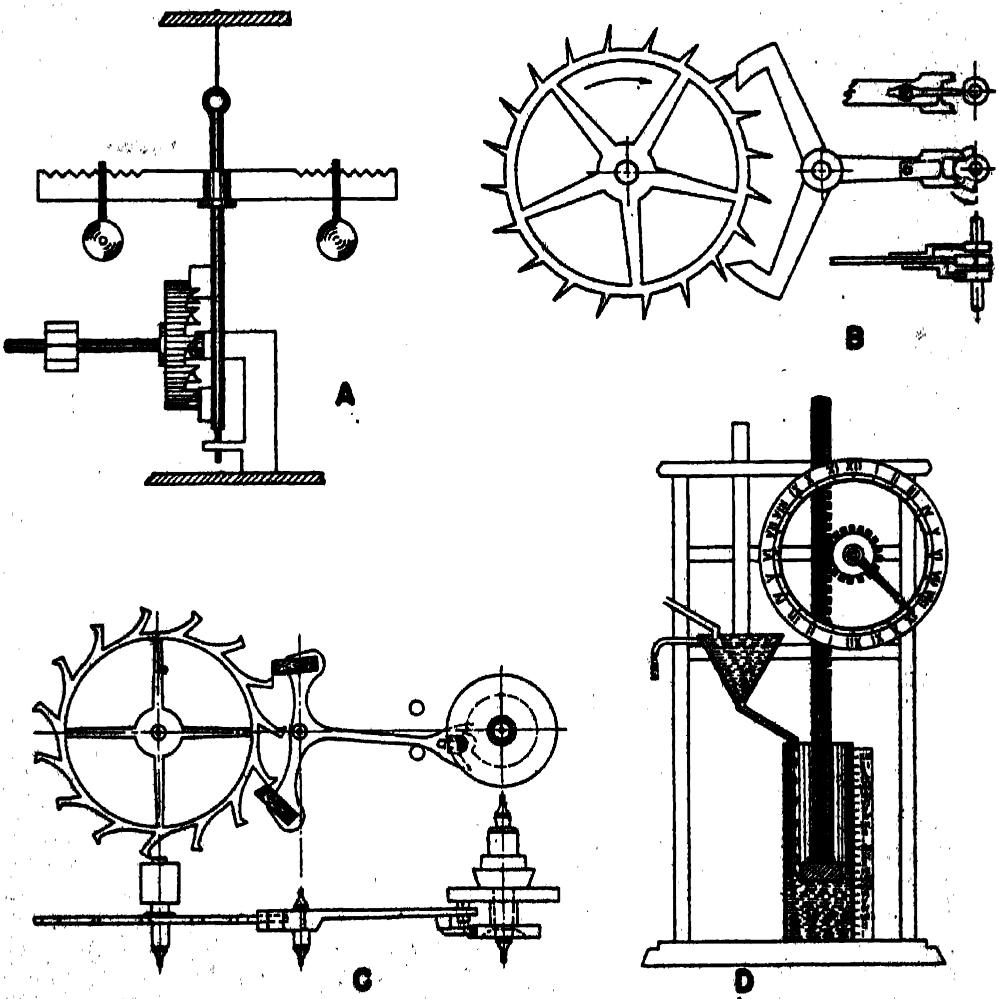
The first bimetallic elements to be used were brass and steel, with later developments substituting invar for steel. Invar was developed in 1896 by Dr. C. E. Guillaume, director of the Bureau International des Poids et Mesures, in Paris. Dr. Guillaume developed his metal to provide a standard measure for the meter that would not expand or contract upon exposure to normal changes in atmospheric temperatures. Its basic composition is 36 percent nickel and 64 percent iron.

In 1913, Dr. Guillaume developed elinvar, the basis of practically all modern compensating hairsprings manufactured today. Elinvar is a refinement of invar, as are Elginite, Nivarox, and other alloys currently used. Some of these variations include heat-treatable materials, thus producing harder and more resilient hairsprings. These materials are characterized by their peculiar ability to become more stiff with an increase of temperature rather than to weaken as does steel.

It is interesting to note that the bimetallic balance was designed to compensate the temperature-stiffness change inherent in the old type hairspring. The modern compensating alloy spring can now be so controlled as to reverse this relationship, and the hairspring can be made to compensate for the change in balance moment. This permits a stability and a simplicity in the modern watch not previously attainable.

A further modern refinement of a compensating balance was recently introduced in the United States in the form of a compensating hairspring and a rigid balance consisting of a stainless steel rim and an invar crossarm. With changes of temperature, this balance will distort to a slightly elliptical shape. This phenomenon, in conjunction with movable weights on the rim, is used to provide an extremely sensitive adjustment of the temperature compensation of a watch in a manner not heretofore possible.

Development of the hairspring and balance to their modern forms now permits watch manufacturers to pretime, or "vibrate," their assembly to an accurate timekeeping rate prior to its installation in the watch. An electronically controlled machine recently developed compares the frequency of vibration of the balance-and-hairspring assembly to an



DEVELOPMENT OF THE ESCAPEMENT

(*A*) THE EARLY "VERGE" ESCAPEMENT RUNNING AN OSCILLATING MECHANISM KNOWN AS THE "FOLIOT." THERE WAS NO HAIRSPRING, AND THE ESCAPEMENT WAS ALWAYS IN CONTACT WITH THE OSCILLATING MEMBER. (*B*) THE FIRST DETACHED LEVER ESCAPEMENT, INVENTED BY THOMAS MUDGE. THIS MECHANISM PERMITS A FREE SWING OF THE BALANCE WITHOUT CONTACTING THE ESCAPEMENT EXCEPT DURING A VERY SHORT INTERVAL. (*C*) A MODERN LEVER ESCAPEMENT. (*D*) AN EARLY WATER CLOCK, THE FUNCTION OF THE ESCAPEMENT AND BALANCE BEING PERFORMED BY DROPS OF FLOWING WATER.

electronic frequency standard. Simultaneously, it adjusts and establishes the length of the hairspring to match the moment of inertia of the balance at the frequency required to keep correct time.

After the pretimed balance assembly is in operation in a watch, another modern electronic machine—the rate recorder, or timer—"listens" to the tick rate by means of a microphone and compares it to a standard frequency. This machine permits a watch manufacturer or repairman to determine and adjust the timekeeping accuracy of a watch in a matter of minutes as contrasted with many days of running on the adjusting racks so indispensable only a few years ago.

LUBRICATION has for centuries been a perplexing problem for the watch manufacturer and repairman. Progress in solving this problem has been rapid within the past few years.

A good watch lubricant must remain fixed in the spot where it is applied. If the lubricant has spreading qualities, it will not stay in the bearings and may even spread to parts of the watch that must be kept free from oil, and thus affect timekeeping values. A watch lubricant must also be nonoxidizing; otherwise, it will become gummy or sticky and hinder rather than help the mechanism to function. Its viscosity must be within usable limits over a wide range of temperatures. Further, a good watch lubricant must not be too sensitive to surface films that may deposit on the bearings. Finally, it must be "oily"—an excellent antifriction agent.

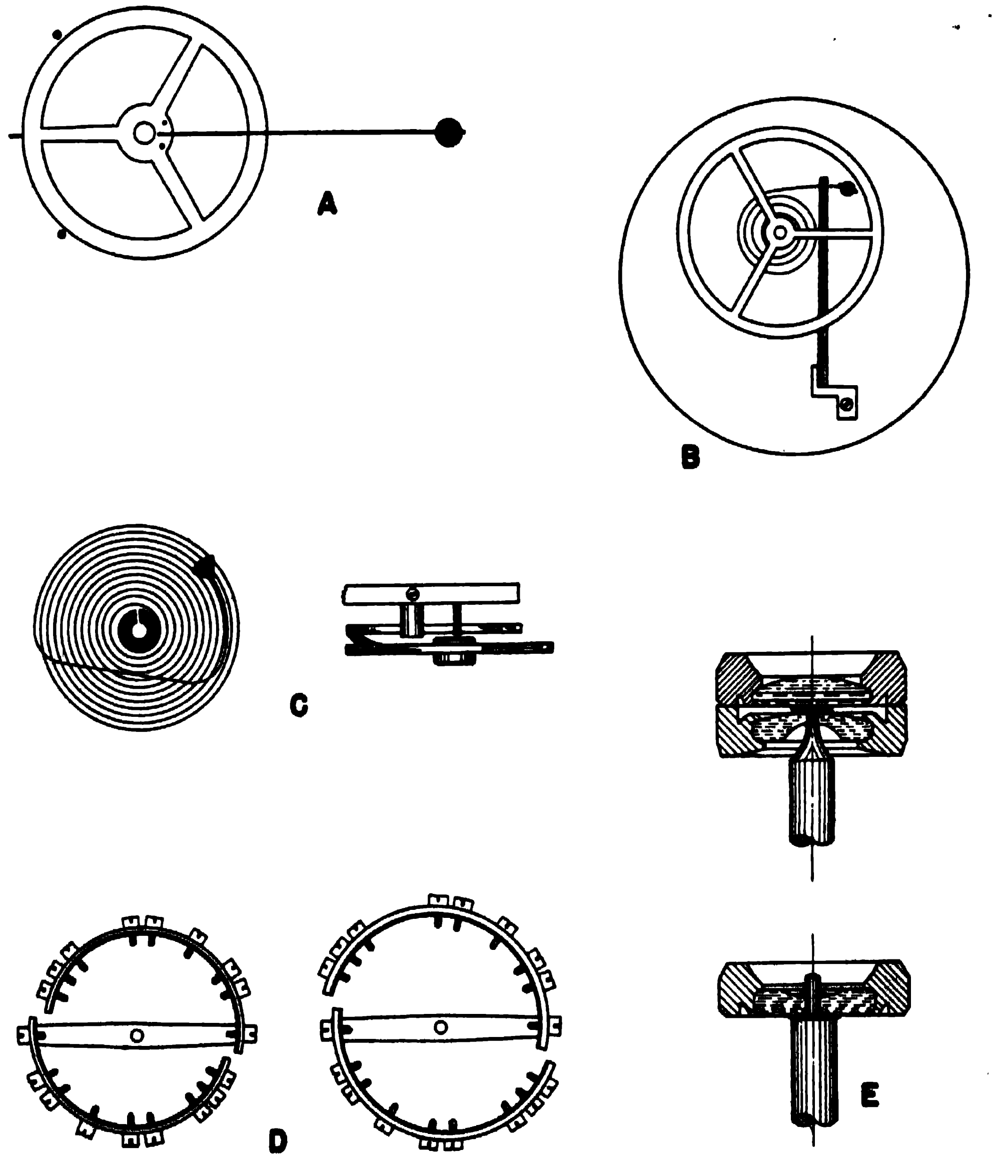
In the early days of large-scale watch manufacture, it was found that the best watch oil was obtained from the porpoise, an aquatic mammal brought back by the whaling ships. The jaw of the porpoise has a small gland containing oil, which, when refined, provided most of the necessary qualities of a good watch lubricant. At one time it was used almost universally. The most serious defect of porpoise-jaw oil was that, upon exposure to light, it rapidly became acid and caused metal parts to corrode.

In the past, practically all kinds of natural oils have been tested and rejected. Mineral oils were tried first and then animal and vegetable oils, such as neat's-foot, walnut, and castor. In many cases, these oils had fine nonspreading qualities, but invariably they were easily oxidized and thus became gummy or sticky. Through the 1920s the only successful forward strides were in refining natural oils so that the acid condition was minimized. Mineral and other oils were blended to obtain better nonspread oils. A German oil was manufactured from an animal oil, such as neat's-foot, blended with mineral oil. This was unsatisfactory because it could not be controlled. The qualities of one batch might be good, the next unsatisfactory. This was true in general of all the natural oils, for they varied from batch to batch and from season to season. In the early 1930s, the French developed a series of oils comparable to the German product in that they were an animal oil blended with mineral oil for proper viscosity. To these was added a combination of organic dyes to absorb the ultraviolet rays of sunlight, thereby reducing their tendency to become rancid. As these oils were of the spreading type, a product called epilame was developed simultaneously to be furnished with each bottle of oil. The epilame was placed on all watch parts that were to receive the oil, providing a very thin film on which the oil would remain without spreading. This was more or less unsatisfactory because, prior to oiling, one could not be sure a sufficient film was present; also, the film was removed each time the instrument was cleaned.

By 1938, most commercial oils had been tested and found unsatisfactory as watch lubricants. The Elgin National Watch Company even sent one of its research scientists to Europe on an extended search to determine whether any oil or oils had been overlooked. At that time the large oil companies were uninterested in doing large-scale research for an industry that used, at the most, only 40 gallons of oil a year. Finally, Elgin referred the problem to the Mellon Institute for study in cooperation with the Elgin Research Department. In 1940, a synthetic lubricant was developed that had the desired qualities. It was given the laboratory designation of M–56a and was marketed under that name.

In 1943, however, Mellon Institute and Elgin developed another oil even more suitable for the commercial trade. This is called M–56b. It is far less sensitive to bearing surface conditions. It therefore gives satisfactory service with far less rigid cleaning standards. M–56b oil will function satisfactorily at minus zero temperatures and will remain effective in unusually hot climates. This oil is now being widely used throughout the watch industry.

A recent development of great importance



VARIOUS PARTS OF EARLY AND MODERN WATCHES

(*A*) EARLY PIG-BRISTLE TYPE OF HAIRSPRING. (*B*) EARLY TEMPERATURE-COMPENSATING DEVICE. DISTORTION OF THE BIMETALLIC ELEMENT SHORTENED OR LENGTHENED THE EFFECTIVE LENGTH OF THE HAIRSPRING TO COMPENSATE EXPANSION OR CONTRACTION OF THE BALANCE. (*C*) MODERN HAIRSPRING WITH "OVERCOIL" TO ENSURE CONCENTRIC WINDING AND UNWINDING. (*D*) EXAGGERATED DISTORTION OF A BIMETALLIC BALANCE AT HIGH AND LOW TEMPERATURES. (*E*) PROPER LUBRICATION OF JEWELED WATCH BEARINGS.

to the watch industry is a highly corrosion-resistant material for mainsprings. This alloy—called Elgiloy—is a result of twelve years of research at Battelle Institute as an Elgin Watch Company project. It has long been a known fact that the major cause of mainspring failure has been the rust that formed on the almost universally used carbon steel, but none of the stainless materials have had mechanical properties that would allow their use as mainsprings without greatly increasing the spring thickness and spring barrel diameter to obtain the necessary torque, or power. Because of style requirements, space is of major importance. A mainspring of Elgiloy not only furnishes power equal to carbon steel, but slightly exceeds it in strength, thickness for thickness. Further, it is not sensitive to surface conditions that cause fatigue failures in carbon steel.

Still another advantage of the new mainspring is illustrated by its name—DuraPower. This spring will deliver the same amount of power after long periods of use as it delivered when first installed. This nearly perfect stability of the new alloy spring allows a watch manufacturer to adjust his watches more closely before they leave the factory, and the constant power source keeps the rate of the watch regular for longer periods.

In contrast, carbon steel begins to lose power as soon as it is wound into a barrel and within a week or two may deliver only 90 percent of its original power. This loss of power, or "set," is easily understood when it is realized that the outer fibers of the mainspring for the average wrist watch are elongated about 0.035 inches per inch when the watch is fully wound. This, of course, places a stress on the material that greatly exceeds the elastic limit of steel, causing a large permanent deformation. Because of its higher yield strength, Elgiloy does not permanently deform to an appreciable degree when subjected to a similar stress.

The developments reviewed above cover only certain innovations in watch technology. No attempt has been made to discuss the many refinements in watch design contributed through constant research and engineering directed toward making a finer product. These improvements are of primary interest only to those who have a special knowledge of the watch mechanism, but it is hoped that the omission of such material will not create an impression that such work is of minor importance. The basic principles of the watch mechanism have not been changed since the introduction of the lever escapement in 1755. The refinements are so numerous, however, that it requires considerable ingenuity to conceive a new one that will afford distinct improvement. It is quite a tribute to the watch as a mechanism that it has withstood the efforts of engineers, watchmakers, hobbyists, and scientists to introduce a new basic principle, such as a mechanism to replace the balance and hairspring. However, anyone inclined to accept this challenge is urged to pause and reflect upon the problem before him. He has chosen to improve an instrument having an error of less than 0.01 percent and representing the best efforts of many great minds and the most skilled mechanics.

TEAMWORK IN INDUSTRY

E. WIGHT BAKKE

Professor Bakke is Sterling professor of economics at Yale and director of the Labor and Management Center recently established there to conduct an extensive program of research in relations between management and unions and workers. He has studied labor problems in the United States, England, and Scandinavia.

ONE of the most respected of the men who founded this nation once said, "We must all hang together, or assuredly we shall all hang separately." That truth has struck responsible and thoughtful leaders of management and labor in recent years with tremendous force. It is not a new idea. Every successful leader of industry and of the labor movement has been aware of it. The typical unit of work and living is not the individual but the group. The products of effort are social products. "No man liveth unto himself."

Good teamwork in industry and unions in the past has depended in the main on the presence in the organization of some leader of men who from experience or sound intuition sensed the basic laws of human relations and understood how to interest men in the welfare of the group as a whole and the organization that bound them together. He knew how to shape and manipulate the bonds that welded a crowd of varied individuals into a working team.

If the cases of successful development of teamwork are to multiply rapidly enough to avoid a breakdown or explosion within our free-enterprise system, the principles of effective human relations will have to be learned by a lot of executives in industry and labor unions who are not "natural-born leaders." And even a lot of natural-born leaders will have to learn how to use principles developed from experience in small, intimate, face-to-face industrial operations in large, complex, and impersonal organizations. For the main reason so many managers and union leaders are concerned about teamwork today is that their organizations have grown beyond the point where they can exercise their leadership through personal contacts with every member of the team. The force of their personal character and know-how has to be channeled through the medium of other persons and the organizational structures of their companies and unions. By the time it reaches the average worker, that character and know-how appear quite different than it was at the point of origin. Even the natural-born leader faces this problem; and the plain fact is that there are not enough natural-born leaders whose personal power is sufficient for the task to go around.

If the rest of us are to be successful in building good teams, we have to learn the principles of teamwork. We have to learn to organize men. We have to learn the nature of the organizational devices that provide the structure within which men work together as a group. We have to learn what makes these devices effective in promoting the purpose of the enterprise and efficient in satisfying the desires of all those who participate.

One may be fortunate enough to work in close contact with a natural-born successful leader who can give personal tutoring on the subject. If not, he may find some profit in considering some of the conclusions derived from a study of both successful and unsuccessful experiences in industry. In either case, it may prove helpful to have a systematic way of analyzing the problems. For the problems change, and the rule-of-experience principles developed by leaders in the past may not be as adequate in dealing with the problems in human relations we now face as they were in dealing with the problems from which they gained their experience.

Now, assuming we are not satisfied simply to talk about teamwork, but want to get down to fundamentals, how shall we go about it?

I suggest we start at the beginning with the most basic question, What makes a team out of a crowd of individuals? Here they are in a plant or in a union. They've come from all sorts of social, political, religious, and economic backgrounds. They've been brought up in different ways. Some are polite, considerate, and respectful; others are swashbuckling, bigoted pirates, born a hundred and thirty years too late. Some are skilled, competent chaps who know what to do and how to do it; others are fumbling fellows with ten thumbs on their hands and solid bone for a brain. Some of them are natural cooperators; others make the sparks fly whenever they come within sight of another person. The job is to take this bunch of varied individuals and weld them into a team. What are the basic organizational devices one can use? I'm going to call these organizational devices the *bonds of organization.*

The bonds are the same whether the team is a public utility, a manufacturing enterprise, a family, a union, a research organization, a navy, an army, a nation, or a football squad. What are they? They are (1) job specifications and requirements, (2) the communication system, (3) the status system, (4) the system of rewards and punishments, (5) the organizational charter. I shall illustrate each one by reference to a football team; but their relevance to any organization is clear.

NUMBER one is *job specifications.* Every player has a position, the duties of which are well defined. If he is an end or a center or a quarterback, he knows what is expected of him as an individual and as a member of the team. He has a definite job to do. Likewise in a company or union there are job specifications for everyone who participates. Sometimes these specifications are spelled out in black and white. Sometimes they are just understood. But unless members of the team have clear definitions of their jobs, and the other members have the same understanding, the first requisite for good teamwork is absent. How well do workers or union members understand what is expected of them? How well do they understand what is expected of the chief engineer or the president or the union officers? How accurate is the conception of the president or general manager of the job requirements of the pipe fitter or the steam boiler engineer? The first requirement for good teamwork is that each shall understand clearly and adequately his own job specifications and shall have a sufficient understanding of the essentials of the jobs of other people with whom he must work so that he can sense what effect those requirements have on their behavior. Managers frequently say, with good reason, that union leaders don't know what a manager is up against. Do managers know what a union leader is up against as a manager of a union, what his job requirements make it necessary for him to do and say? Do union leaders know what being a manager requires of the person who fills that job?

The second bond of organization that binds a group of individuals into a team is the *communication system.* On a football team there are certain signals that have to be given so that each member of the team knows what he is to do and what the others are doing. There is a method of communication, perhaps a huddle. Someone is responsible for barking the signals. Someone calls time out for a conference. Skull sessions are called to make sure that everyone shares the same understanding of the plays and has possession of the information that makes them willing and able to work together. Every organization has its communication system. Policies, procedures, administrative orders, have to be transmitted down the line. Reactions, performance reports, and many other details have to be transmitted up the line. Do the communications from both directions get through clearly and adequately to the people for whom they are intended?

The communication system has to handle every kind of information that people in the organization want to get across to one another. Much attention has been paid by management to the forms and procedures for transmitting *orders* down the line. Less attention has been paid to procedures for transmitting equally important information concerning the objectives, achievements, and problems of the company down the line, and for getting into the hands and minds of management the problems of the people they supervise. Unions have also frequently neglected the line of communication *from* the rank and file. The communication system has to carry information in both directions.

How frequently do managers or union leaders sit down and ask themselves these questions: What information must I have and what information must those under me have in order to work together effectively? What instruments have I devised for making that information available? Is the system adequate, and does it place in the hands of every person clear and concise information that he needs to do his job and to maintain satisfactory relations with the rest of the team?

The third bond of organization is the *status system*. Every team has a captain whose word is supposed to be law, whose approval is required at critical times. There are coaches and the head coach. Sometimes there are players on the team who have no official status but to whom others look for direction. They "rate," not formally, but informally. In every organization men are placed in a status system, and each place carries its authority over, and obligations to, others, its deference from, or to, others. Every company and union has an organizational chart that is supposed to represent this hierarchy of authority and deference. Our studies have indicated that this organizational chart frequently bears little resemblance to the actual hierarchy of authority and deference in the company or union. Good teamwork is a process governed by the status system as it is, not as it appears on the chart in the president's office. If the formal status system is inconsistent with the informal status system, teamwork is bound to suffer.

The fourth bond of organization that holds a group together is the *system of rewards and punishments*—rewards for satisfactory work, punishments for unsatisfactory work. On a football squad rewards may be formal, like a letter, or bars for additional service; or they may be as informal as a cheer for a good play or a slap on the back and a resounding "Atta-boy, Mac." Punishment may also be formal, like being "benched"; or informal, like hard looks, or another player saying, "What are you trying to do, play the whole game yourself?"

In any organization, what is the actual system of rewards and punishments as seen through the eyes of participants at each status level? What do they think are the conditions upon which their achievements and benefits within the organization depend? Are executives assuming that the system is limited to increases and decreases in pay or position? Do they think people look to the organization as one that provides them simply with a pay check and a title, or can the organization provide or withhold other benefits men want in order to live a more complete life?

The system of rewards and punishments is one of the most important group-making devices for which managers and leaders are responsible; but in order to be effective, this system must be realistic from the point of view of the people who are rewarded or punished. My first lesson in the fact that what may appear as a reward for one person may be a punishment for another was when I learned that many workers would consider advancement to a foremanship an unwelcome promotion, and that the receipt of abnormally high earnings from piecework might declass a man instead of giving him superior status with his fellows.

How much do managers, responsible for shaping the nature of rewards and punishments in a firm, actually know about what their subordinates and employees consider to be rewarding and punishing? Have they unconsciously assumed that what has proved rewarding to them must be rewarding to everyone? For instance, consider the pension system. Executives have frequently been with the company a long time and expect to stay with it until they retire. Is the pension system realistically rewarding to those younger employees who may work for half a dozen companies before they are through? Or consider increases in individual pay and promotion. Success as a manager has followed the pattern of winning out over other individuals. Increases in salary and position are symbols to them of that kind of success. But suppose one's whole experience had taught him that success depended on group solidarity, and sticking together, and sharing bad as well as good fortune. Would a promotion in pay or position that sets one apart from his fellows be a reward?

How broad is the conception of union leaders of the rewards for being a union member? Have they been satisfied to assume that the traditional rewards are the only ones members would like to receive?

I am not offering any dogmatic conclusions here. I am simply suggesting that the system of rewards and punishments leaders work out for their teams will be effective to the degree that it gives people what *they* want if they do well and withholds from them what *they* want if they do poorly, and that a leader has to find out what *they* want by looking at the situation through their eyes, not his own.

The final bond of organization that welds a group of people together is what we may label as the *organizational charter*. It is an intangible but real thing. It is the picture that members of the team have of the team as a whole. That picture is composed of their conception of its purposes, achievements, traditions, and the symbols of these. It is the picture men have of the company when they say, "I work for National Tube." It is the picture men have of their family when they say I am a MacDonald, or a Cabot, or a Koussevitsky, or a Knudson, or a Callahan. It is the picture that men have of their nation when they say, "I am an American." It is the conception they have of their union when they say, "I am a member of the United Steel Workers of America." One of the greatest differences we have found between management and men, or union leaders and members, is their picture of this organizational charter of the firm for which they work, or the union of which they are a part. If the pictures in their minds are too inconsistent, effective teamwork is impossible. What attempts have managers and union leaders made to learn what their employees or members think of the organization as a whole, and whether things can be done to make their picture more consistent with that of the leaders, and to make both the pictures more consistent with the truth? In so doing, they would be concerning themselves with one of the most important bonds of organization that make a team out of a miscellaneous collection of individuals.

For organizers and leaders of a team here are five organizational devices, five bonds of organization that weld the people who work for or with them into a team: job specifications, the communication system, the status system, the system of rewards and punishments, the organizational charter. Their first job is to know the facts about these devices as they are actually operating in their organization.

I have learned as the director of a research team of forty people that I cannot take anything for granted. I cannot assume that these devices in our organization are what I planned them to be or that they appear the same to the people who are working with me as they appear to me. I am busy doing my job. I have my specific duties to perform and my specific obligations to meet; and they keep me at work long after the members of the team have left the office and gone off to attend to their personal affairs. But I know I shall never succeed in organizing an efficient team unless I make these organizational devices satisfying from their point of view. If I were a poet or an artist, or a concert pianist, or someone else who worked independently, I would not have to worry about such things. But I am not. I am the organizer of a team. My success is dependent on the work and loyalty of the members of a team. So the first question I have to ask is what the organizational devices look like through the eyes of the members of the team.

But that is only the first question. The second one is equally important. It is this: Are these devices or bonds satisfying to them? Are they helping them to get from their work what they want to get from it? What are their standards of successful living against which I can check the efficiency of those devices? Again, I say, it is their standards, not mine, that are important, simply because it is their response and not mine that is involved in getting the job done. If the organizational devices help them to make progress toward their goals, they will put their shoulders to the wheel and give their best. If not, they will hold back, lose their interest, and in so doing, act like a crowd of individuals instead of working together to accomplish the team's objectives.

I AM no authority on the motivations of men; but at the Labor and Management Center we are trying to learn what these are. Whether our investigations have been focused on the behavior of unemployed men, workers who were being organized, relations between union leaders and management, union leaders and union members, or management and employees, we have tried to find out what really made these people tick. What were *their* goals, and how did the environment created for them by the community, the managers, or the union leaders help them or hinder them in their progress toward these goals? Without claiming that the list is complete, or that it has been adequately tested, it may prove of value to list them. As they stand, they are our honest attempts to summarize the responses men make to such questions as "How do you define the difference between a fellow who has got ahead and one who is a failure?" "What do you want your children to get out of life that you have failed to get?" "Is there anyone you would like to change places with? Why?" and similar questions. What are the goals, the standards of successful living, that stand out in the answers to questions such as these? I am convinced that these are major standards against which members of the team measure the adequacy of the organizational devices, the bonds of organization we have been discussing.

Men want to experience *progress* toward, and *security* with respect to, the following goals. And they want to feel that the amount of their progress and security are *just* in the light of their conception of their own worth.

1. *The respect of their fellows.* This is the desire to play a role that the people they associate with consider important and respectable.
2. *Creature sufficiency.* This is the desire to have the amount and quality of food, clothes, shelter, health, etc., and the means to provide them, enjoyed by the most favored of their customary associates.
3. *Increasing control over their own affairs.* This is the desire to have their own decisions and actions effective in shaping the course of their own lives and to reduce the control exercised by others.
4. *Understanding.* This is the desire to have a clear picture of the facts and forces that are at work in their world, the relation between what happens and what caused it to happen—in other words, "to know the score."
5. *Capacity performance.* This is the desire to have the chance to use the full range of one's abilities. It is not the desire to burn one's self out on a particular task. The question as it applies to industrial relations is whether a man's job gives him the opportunity to do the sort of things he thinks he is capable of doing, or whether it calls out only an insignificant portion of his capabilities.
6. *Integrity.* This goal is the hardest to define, but its importance is evident. We use it in the sense of wholeness. A man wants to feel whole within himself; that is, he wants to feel that his actions and principles are consistent. (Call that aspect of integrity, self-respect.) But he also wants to feel whole with the world of people and things about him. He wants to be geared in, to sense that he is a significant part of it all. (Call that aspect of integrity, relationship.)

The second requirement of good teamwork is that the bonds of organization that men experience in the group shall be efficient instruments for helping the members of the team satisfy these desires they have.

I would like to suggest that when a manager or a union leader is found who has been successful in getting good teamwork results, the first two secrets of his success will prove to be these: He was skilled in developing workable organizational devices of the sort we have been talking about; i.e., he knew how to construct effective bonds of organization. Moreover, he had a good understanding of what these goals we have been discussing meant to his people and saw to it that the bonds of organization were efficient in giving them a sense of security and progress and justice toward those goals.

I HAVE simply outlined the framework of devices by which teamwork is carried on, and the standards of successful living by reference to which the members of the team judge those devices as satisfactory or not. The principles of the relationship between the two would fill many volumes.

I can suggest only a very few of these principles. I shall state them negatively, that is, as inadequacies in these bonds of organization that have turned up in some of our studies. I suspect that many, if not all, of them will be familiar to most readers. At least I hope so, because experience of practical organizers in the past, present, and future is the first testing ground for such conclusions. The question then is: Where we have found evidence of poor teamwork, what was wrong with these organizational devices as measured against the standards of successful living of the members of the team?

Consider, first, inadequacies in the *communication system.*

1. The communication system did not carry down the line all the kinds of information needed and desired. Particularly scanty was information about the purposes, traditions, and problems of the company or union itself, the general economic and social factors that affect the welfare of the company and union, and with which top management or leadership was wrestling. Frequently lacking was clear information about major policies of the company in its relation to the public, to the union, and even to its own employees, or about the major policies of the union in its relation to the public, the company, or its members.
2. The communication system did not carry messages both ways. No careful provision was made to get reactions from, and an understanding of, the problems of men down the line into the minds of management and leadership up the line. I am not speaking of grievances alone. Often even more important than grievances are the day-to-day reactions of men to the company, their jobs, their management, their union officers, their suggestions for improving conditions and processes of work, or the conduct of union affairs, and just the plain facts about what life and work look like from where they stand.
3. The communication line was not short and direct enough—in both directions—so that at the end of the line the same message came out that was started. As a result, short cuts and by-passes led to misunderstandings and situations which men interpreted as undermining their authority.
4. The authority of those who were qualified to originate or to pass on information was not defined clearly and precisely enough so that men could have secure confidence either in the people from whom it came or the information they received.
5. The communications were not designed to motivate emotional acceptance of the information as well as to assure its intellectual comprehension.

Notice in these examples that the communication system was not adequate to give men a sense of security with respect to realizing their goals of understanding, social respect, control, and integrity.

Now turn to the *status system.* What inadequacies in that bond of organization frequently retarded good teamwork?

1. Sometimes men felt they were responsible to, and must look for guidance and formal approval from, more than one boss or official. The lines of authority were confused.
2. The system was so autocratic that only those at the very top could get any real sense of importance from the positions they occupied.
3. Qualifications for filling the higher positions in the status system were so arbitrary or unrelated to effective performance that men's sense of justice was violated.

Notice in these examples that the status system was not adequate to give men a sense of security and justice with respect to realizing their goals of control over their own affairs, social respect, understanding, and integrity.

Now consider the possible inadequacies in the *organizational charter.* In fifteen years of interviewing workers and management in England, Scandinavia, and the United States, I have never found one who did not want to work for a firm or be a member of a union he could be proud of. In every community there exists in the minds of the workers a pretty clear understanding of the firms and unions that "rate" and those that do not. They are arranged in a more or less well-defined hierarchy from "bum" to "swell." Those that rate high in the scale of course have an easier job in developing and maintaining an organizational charter in the minds of the members of the team, which helps greatly to promote good teamwork. But every firm and union must make the best of what it has and try to create a common picture in the minds of its members of its purposes, achievements, and traditions. The men will develop their own picture anyway. It is the job of leaders to see that every possible feature of that charter that will make men proud of their connection is made vivid and clear. One of the greatest shortcomings of the leadership of companies and unions we have noticed, where poor teamwork was evident, was their failure to pay any attention at all to this requirement for effective group activity. A second shortcoming was their bragging about features or characteristics of the organization that had no basis in fact. The result of these shortcomings of management and leadership is a failure to satisfy the desire of team members for respect and a sense of relationship, or integrity.

Managers and union leaders have given more attention to the bond of organization I have labeled *job specifications* than to any other. That bond has struck them as essential and important. Some of the major inadequacies that do appear, however, are these:

1. They focus entirely on instructions as to how to do a particular job and neglect the definition of how that job is related to other jobs and how the people doing them are related to each other functionally.
2. They leave too little freedom for the initiative and inventiveness of the person assigned to the job.
3. The job specifications are not matched with the actual and potential capacities of the job holders.
4. They are changed so frequently and arbitrarily that people do not know where they stand, and are constantly having to adapt themselves to change. As soon as they get into the swing and rhythm of an operation, it is upset by new instructions.

Such inadequacies keep men from realizing their desire for security, control over their own affairs, and satisfying relationships or integrity.

Finally, we turn to the inadequacies in the *system of rewards and punishments*. The listing of specific shortcomings that have appeared in our studies would fill a book. Let me, therefore, set down only six conclusions about the general nature of these inadequacies.

1. The system that defines what kind of performance results in what reward or punishment is not clearly understood by all concerned. What managers and union leaders think the system is frequently bears very little resemblance to the ideas workers have of it.
2. The rewards and punishments are dispensed arbitrarily and sporadically rather than systematically. In other words, they are not dependable. They seem to be administered by whim rather than by plan.
3. Discrepancies in performance requirements and responsibilities exist among those receiving the same pay.
4. Rewards and punishments are geared solely to individual performance, emphasizing the significance of competition with, rather than cooperation among, men. Man is set against man rather than geared into a team. Rewards are for individual achievement rather than for team accomplishments.
5. The emphasis in the entire system is upon economic rewards and deprivations, ignoring the desire of men for social respect, freedom, capacity performance, self-respect, and a significant relation to people and the world about them.
6. The most important shortcoming I have already mentioned. The rewards and punishments are not realistically set up to jibe with what the people affected consider to be rewarding and punishing.

Wherever these shortcomings are found in the bonds of organization, one or more of the basic goals for which men are consciously or unconsciously striving is moved beyond the horizon. Men get lost in the thickets and underbrush. They lose the sense of satisfying contact and team play with other members of the organization toward a common objective. If they do not adopt a "What-the-hell" attitude, they at least get used to taking the next obvious step without enthusiasm or hope. Obsessed by personal frustration, they go doggedly along their own way unconcerned about the welfare of the whole team. Why should they exhibit a team loyalty if the managers and leaders of the team fail to bind them together with the bonds of organization that provide them with opportunities for, rather than obstacles to, the realization of their standards of successful living?

Does it look like a mountainous task to develop skill in constructing these bonds of organization, and an understanding of the standards of successful living by which members of the team judge them adequate or inadequate? Of course it is. The organizer of a team is faced with the most complicated set of facts in the world, the nature of human beings and the ways in which they interact with other human beings. The question, Why do men act as they do? is the hardest question in the world to answer. But the answer we must have, and have it right, if we are to organize effective teams.

Managers have faced engineering problems nearly as difficult. If they were to give this problem as much systematic attention and study as they give to the quality of materials, the layout of the plant, the performance of equipment, or the development of a rate schedule, it might not prove insurmountable after all. But it does take systematic attention and study. It cannot be left to guesswork or chance any more than engineering problems can be left to guesswork and chance.

Engineering problems do not have to be left to chance, because systematic ways are available for analyzing these problems. Scientists—physicists, and chemists—provide basic principles and laws as to the behavior of materials and equipment. Schools are established in which men can be trained on the basis of that science. Nothing less than that is required for successful human engineering. The assembling and organizing and directing of a team of human beings are no less difficult than assembling and operating a turbine.

At the moment we could coast for awhile, if necessary, with respect to our knowledge and skill in the field of technology. No major breakdown impends, no irreparable disaster threatens us. But no honest person can survey the conflicts and confusion that characterize our efforts in developing satisfying and productive human relations in industry and avoid the conclusion that a breakdown or explosion is not only possible, but probable, unless we discover and apply better principles of teamwork than we have used to date.

The problem is not alone one of making company and union teams more effective; it is one of making our free-enterprise economy and our democratic society strong in relation to other economies and societies whose principles of teamwork differ from our own. It was never more true than today that unless we all learn to hang together we shall all hang separately. It is one thing to recognize this truth, but it is another to learn how to hang together, and to get from individuals the willing and enthusiastic activity and loyalty that makes it possible to hang together. That is the task to which the organizers and leaders of teams in America are called. It is the task that those of us in the universities are trying to make easier by the development of a science of human relations that practical men can use. Together we carry on our shoulders the responsibility for the success or failure of this task. Our failure will carry the gravest consequences for the welfare of our people, their institutions, and their way of living. Our success will uncover fresh sources of power and renewed strength for the kind of free enterprise and democratic teamwork to which we are accustomed and to which we have committed our destiny as a nation.

EIGHTY-EIGHT YEARS OF SYNTHETIC RUBBER

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FOURSCORE and eight years ago our chemical forefathers brought forth on this globe a new material conceived in the laboratory and dedicated to the proposition that it was the first synthetic rubber.

Most people are inclined to think of synthetic rubber as the child of this decade. It comes as quite a surprise, even to chemists, to learn that the first crude attempts to solve the rubber puzzle were made eighty-eight years ago. Eighty-eight years takes us back to 1860, a year before the War of the Rebellion started. 1860 was the year Charles Goodyear died. It was the year the vulcanized rubber industry came of age—twenty-one years old.

All of us ought to feel a little humble to know that the foundations of the present multimillion-dollar synthetic rubber industry were laid that long ago. It is a human trait to feel that everything worth knowing has been discovered or developed within the scope of one's own lifetime. Consequently, it is well to remember that nearly ninety years ago there were men who were doing sound thinking on the same problems that interest rubber chemists today, and doing it without benefit of any background of pioneer research and without the equipment and facilities that we accept as commonplace.

The first great name in the story of synthetic rubber is that of the Englishman Greville Williams. Others before Williams had subjected rubber to destructive distillation. As early as 1833, Barnard had obtained a volatile oil, which he patented as a rubber solvent under the name "caoutchoucine." In 1834 both Justus Liebig and John Dalton obtained volatile distillates from rubber, but evidently considered them of little importance. There were others—Dumas, Gregory, Himly, and A. Bouchardat, to name a few.

In 1860 Williams made the first systematic attempt to isolate and examine the products present in rubber. He repeatedly purified the evil-smelling distillate he first obtained until he had a pure, low-boiling fraction that he named "isoprene." Historically, this was of great importance because isoprene or one of its relatives has been the basis of every synthetic rubber ever made, the Thiokols excepted. Williams did not stop at tearing rubber down. From his isoprene he built up—something.

Writing in *Philosophical Transactions* in 1860, he said:

> If isoprene be left in a partly filled bottle for some months it gradually loses its fluidity and at last becomes quite viscid; at the same time it is found to have acquired powerful bleaching properties. It is, in fact, ozonized.
>
> On submitting ozonized isoprene to distillation, if the temperature be kept as low as is compatible with the volatilization of the unaltered portion of the oil, a colorless limpid fluid distills over, having the composition and boiling point of pure isoprene. As the operation proceeds, the fluid thickens, causing the temperature to rise somewhat suddenly. The ozone at this point instantly begins to act with energy, a cloudy vapor arises accompanied by an intensely sharp odour, and the contents of the retort instantly solidify to a pure white, spongy elastic mass, having, when successfully prepared, but slight tendency to adhere to the fingers. When pure it is opaque; but if allowed to become exposed to the air, especially when warm, it becomes transparent, first on the edges and subsequently throughout the whole mass. When burnt it exhales a peculiar odour hitherto considered to be characteristic of caoutchouc itself. . . .
>
> I am anxious to call attention to the fact that the atomic constitution of caoutchouc appears to bear some simple relation to the hydrocarbons resulting from its decomposition by heat. The composition of caoutchouc coincides with that of

> isoprene, as found by analysis, to a degree which is remarkable when we consider that caoutchouc in addition to being non-crystalline is scarcely capable of purification by chemical means.

It should be emphasized that Williams himself did not claim that he had produced synthetic rubber. No less an authority than William Henry Perkin, however, considered that Williams had probably produced some sort of polymer of isoprene.

A Frenchman, Gustave Bouchardat, was next to point out the close connection between isoprene and rubber. In 1875 he carried out quantitative experiments on the decomposition of caoutchouc, obtaining something over half a pound of isoprene from eleven pounds of rubber. He heated this material in sealed tubes and noted that it became viscous and acquired elasticity. In 1879 he heated one part of isoprene with twelve parts of concentrated hydrochloric acid for twenty hours in a sealed tube. After distilling to remove the isoprene hydrochlorides, he obtained a product which he said had "the elasticity and other properties of rubber itself. It is insoluble in alcohol, it swells up in ether, also in carbon bisulphide in which it dissolves after the fashion of natural rubber." Bouchardat performed other chemical tests on it and decided that "all these properties seem to identify this polymer of isoprene with the source of isoprene, namely rubber."

In England, William Tilden confirmed Bouchardat's experiment in 1882 and worked out the chemical structure of isoprene. In a speech that year before the British Association for the Advancement of Science, he said:

> The other peculiarity of isoprene is its conversion into true india-rubber when brought into contact with certain chemical reagents such as strong aqueous hydrochloric acid as noticed by Bouchardat or nitrosyl chloride as observed by myself. It is this character which gives it a somewhat practical interest, for, if it were possible to obtain this hydrocarbon from some other and more accessible source the synthetical production of india-rubber could be accomplished.

Tilden himself was the first to find that other source. In 1884 he made isoprene by passing turpentine vapor through a red-hot tube. He was professor of chemistry at Mason College, Birmingham, at the time. He made a considerable quantity of isoprene, some of which he used; the rest he bottled up, set aside on his laboratory shelf, and apparently forgot about for eight years.

Meanwhile, in 1887, Otto Wallach reported in the German *Annalen:* "When isoprene is placed in sealed tubes and is exposed to the action of light for a long time, a product is formed, which, when treated with alcohol forms a tough mass, resembling rubber; this material became more or less hard on exposure to air."

Unaware of Wallach's results, Tilden on May 18, 1892, read a paper before the Birmingham Philosophical Society, in which he said:

> Specimens of isoprene were made from several terpenes in the course of my work on those compounds and some of them I have preserved. I was surprised a few weeks ago at finding the contents of the bottles containing isoprene from turpentine entirely changed in appearance. In place of a limpid, colourless liquid, the bottle contained a dense syrup in which was floating several large masses of a solid of a yellowish colour. Upon examination this turned out to be india-rubber. The change of isoprene by spontaneous polymerization has not, to my knowledge been observed before. . . . The artificial india-rubber like natural rubber appears to consist of two substances, one of which is more soluble in benzene or carbon bisulphide than the other. . . . The artificial rubber unites with sulphur in the same way as ordinary rubber forming a tough, elastic compound.

That Tilden really had some kind of synthetic rubber has never been questioned except by a few die-hard Germans bent on getting full credit for their country. The spontaneous change was confirmed by the great rubber chemist, C. O. Weber, in 1904 and by Samuel Pickles in 1910.

Isoprene monopolized all the attention in these early efforts at polymerization. Not until 1900 did the Russian Kondakov report that methyl isoprene could be converted into an elastic mass by heating with potash. The following year Thiele showed that piperylene also polymerized to a rubbery product when left in the dark for a long time. On first thought, it seems strange that the polymerization of butadiene was not reported for almost another decade. Butadiene had been discovered by Caventou in 1863. Probably because it is a gas at ordinary temperatures, it was not found to undergo polymerization until 1910. In that year, Lebedev reported that on heating it yielded a rubberlike material.

The Tilden of 1882, who was optimistic about artificial rubber, lost his optimism with the passing years and shortly before 1900 he wrote to the *Kew Bulletin*:

> I have tried everything I can think of as likely to promote the change but without success. The polymerization proceeds very slowly, occupying according to my experience several years and all attempts to hurry it result in the production not of rubber but of colophene, a thick, sticky oil quite useless for the purposes to which rubber is applied.

A little later and a little more discouraged, he wrote in the same publication

> It is a question whether the process could be made commercially productive even if a suitable reagent could be found to effect the transformation because the yield of isoprene from turpentine is very small. . . . If isoprene were obtainable at a low cost from other sources it might be found possible to utilize the hydrochloric acid process, though I doubt it.

In 1907 Tilden said that synthetic rubber was commercially impossible at any price and advised those who had invested in plantations to go on putting their money in them because the future of the industry hung on the cultivation of the rubber tree.

Tilden's work marked the end of the first, or academic, phase of the search for synthetic rubber. Even while he was losing faith, the second phase was getting under way—chemists everywhere were devoting more and more time to the problem. It is not difficult to understand why this was so. In 1900 the average price of rubber in this country was 50.6 cents. Demand for rubber was increasing. Production of wild rubber was dropping off, owing mainly to overexploitation of trees and of native populations. Production from plantations was negligible; in fact, the idea had not even been proved to be a sound one. In 1906 rubber sold for $1.50 a pound; in 1907 and 1908, for $1.40; in 1909, for $2.20; and in 1910, the price hit $3.06. Obviously, the chemist who could have discovered how to make rubber in the laboratory at that time, before the plantations were well established, could have become one of the world's wealthiest men.

The quest for synthetic rubber developed into a bitter international race between Germany and England. Turpentine was abandoned early as a satisfactory raw material for the synthesis. It was not cheap, the supply was not dependable, and it was subject to market manipulation the same as rubber. Consequently, processes were developed for the production of isoprene and its relatives from carbide, coal tar, petroleum, and starch.

In Germany, the Bayer chemists, Fritz Hoffman and Carl Coutelle, in March 1909, produced isoprene from paracresol by a complicated process. This was done by catalytically hydrogenating to methyl cyclohexanol, oxidizing to β-methyl adipic acid, converting to the diamide and then to methyl tetramethylene diamine, and finally obtaining isoprene by exhaustive methylation.

Coutelle tried all sorts of likely and unlikely chemical and physical agents to hasten the thickening into rubber and, in August 1909, obtained a rubber by simply heating the isoprene in closed tubes for about eight days at a temperature below 200° C. The discovery received wide publicity and has been cited by German writers ever since as the first synthetic rubber.

In the following year, there occurred an example of the almost simultaneous discovery of a scientific fact by more than one observer. In England, Francis Matthews found that sodium caused isoprene to polymerize rapidly and applied for a patent on October 25. Three days later, Harries, in Germany, communicated the same discovery to Bayer. In resulting patent suits, Matthews' claims were upheld in both countries.

Fritz Hoffman started the manufacture of isoprene rubber on a small scale in Germany in 1910. The price of natural rubber, remember, was over $3.00 a pound. Isoprene at that time was difficult to make, and Hoffman turned to methyl isoprene, which was easier. Today the reverse is true. The manufacture of so-called methyl rubber began to assume some importance in Germany, but it was halted by the crash in natural rubber prices, which occurred as a result of plantation rubber finally starting to become a factor.

An interesting offshoot of the work on methyl rubber in Germany was the independent discovery of organic accelerators of vulcanization in that country. Methyl rubber aged badly. Hoffman and his associates, Delbrück and Meisenburg, found that addition of materials such as piperidine gave a better product and also hastened vulcanization. They tried their discovery in natural rubber and arrived at the same results achieved by Oenslager and Spence in the United States some years earlier.

ENOUGH for the moment about Germany; let's see what was going on in England during this period. The English were far from idle. In 1908 E. H. Strange, of the firm of Strange and Graham, started his head chemist, Francis Matthews, on the search for artificial rubber. Matthews first proposed to follow the acetone-to-isoprene route, but shortly devised a better method, using amyl alcohol from fusel oil. The alcohol was treated with hydrogen chloride, and the chloride obtained by fractionation was converted by further chlorination into a mixture of dichloropentanes. This was passed over soda lime at 470° C. and gave a 40 percent yield of isoprene.

William Perkin, son of the great dye chemist, was working on the same subject and was approached by Strange, who proposed an alliance. The group was soon joined by Sir William Ramsay, Chaim Weizmann, and half a dozen other foremost English scientists. A mass attack was started on the problem of obtaining rubber from various alcohols. Fernbach, of the Insitut Pasteur in Paris, was enlisted to find a better source of amyl alcohol than fusel oil, which was relatively scarce. Fernbach decided that the reason fusel oil was scarce was that all previous fermentation work had been aimed at producing as little of it as possible. He deliberately sought to make fusel oil the principal product and after about eighteen months' work discovered how to make huge quantities of it by the proper fermentation of potato starch. From the fusel oil, both amyl and butyl alcohols were obtained, the former being used for isoprene and the latter for butadiene. Rubber was made from both of these by Matthews' sodium process.

The group was so enthusiastic about its prospects that it organized the Synthetic Products Company in 1912 to manufacture acetone, alcohols, and synthetic rubber. The output of butyl alcohol and acetone kept the company going for some time, but no serious attempt was ever made to produce rubber because the price of crude was down to 48 cents in 1913.

The smoldering rivalry between German and English chemists burst into open flames in 1912 and was an omen of the greater conflict that was to start two years later.

Perkin began it with a lecture he delivered in London in 1912. It is not hard to understand his antipathy for German chemists. He was the son of the man who discovered the first coal-tar dye. England, in his father's time, had little interest in chemistry. The government did not support the senior Perkin's plan for developing the industry. Universities were above educating chemists. The development of organic dyes slipped out of the hands of the English and into the hands of the Germans, who appreciated what they had and built it into the immensely valuable dye industry. By 1912 English textile makers were paying millions of dollars annually for German dyes.

In his lecture Perkin made it clear that he thought little of Hoffman and Coutelle's 1909 discovery. He said:

> As regards this synthesis, it seems to me desirable in view of statements which have been made abroad to review the work of earlier investigators and particularly that of Tilden in England and Bouchardat in France in order that I may emphasize the fact, as I particularly wish to do, that much of the credit of the pioneer work in this subject belongs to this country and France. It is surprising that Professor Harries should doubt whether Tilden really had synthetic rubber in his hands in 1882 and 1884 and should advance the view that certain samples produced in 1909 by Bayer and Company were the first true samples of synthetic rubber. . . . The specimens of crude synthetic rubber which Tilden undoubtedly prepared were made many years before Bayer and Company produced rubber by their process.

Perkin went even further back and credited Williams and Bouchardat with the discovery. Of the former, he said:

> It is true that this substance . . . was not at his disposal in sufficient quantity for him to definitely identify it with rubber, which it undoubtedly contained. [And of the latter he remarked] Bouch-

> ardat in 1875 showed that isoprene is converted by heating in a sealed tube into a mixture which there can be no doubt contained a considerable portion of rubber.

Harries came back swinging in the *Annalen* about six months later. He wrote:

> It is only about two years since my last paper on synthetic rubber was published and this paper has produced a remarkable literary activity. . . . This result would have been very welcome had there not appeared less agreeable features, which probably never before appeared to such an extent in any other field of chemistry. I refer to the introduction of nationalistic motives into scientific research, and to the broadcast discussion of questions of priority in scientific matters in the daily press, or in certain journals which usually are not selected for such discussions.
>
> On July 15, 1912, W. H. Perkin, Junior, delivered a lecture before the London Section of the Society of Chemical Industry. . . . The purpose of this address is quite apparent, namely, the presentation of the discoveries in the field of rubber chemistry as purely English accomplishments. One can only shake his head as Perkin now seeks to interpret the old, meager statements of Williams relating to the auto-oxidation products of isoprene so that it will appear that Williams really had synthetic rubber in his hands. Perkin again returns to the experiments of Bouchardat and Tilden who claim to have polymerized isoprene to rubber by means of hydrochloric acid and he explains, contrary to my statements, that the observations of these investigators are undoubtedly correct. . . . If Mr. Perkin, Junior, is so much interested in the synthetic preparation of rubber by means of the action of hydrochloric acid on isoprene, I beg that he himself try at least once to rediscover the experimental conditions which were hidden by Bouchardat and Tilden. If then Perkin can so describe the reaction that it may be successfully repeated every time, I will admit without reservation the priority of Bouchardat and Tilden.

The German-English rivalry cropped out at the eighth International Congress of Applied Chemistry, which was held in New York in the fall of 1912. At this meeting Perkin announced the method of making synthetic rubber raw materials by fermentation processes and as a matter of interest displayed some samples of rubber made by Tilden years before. He was followed by the German, Carl Duisberg, who sneered at the English work, told of the discovery of synthetic rubber in Germany, and claimed all the credit for Hoffman. He said:

> As far back as the '80's the Englishman, Tilden, claimed to have prepared artificial rubber from isoprene. . . . Neither Tilden nor his assistants, though they worked strenuously for years succeeded in repeating the experiments. . . . Dr. Fritz Hoffman is to be regarded as the real discoverer of synthetic rubber.

To emphasize his statements, Duisberg displayed a set of tires that had been made from Hoffman's synthetic rubber in Germany.

Nothing had been done in the United States about synthetic rubber during the fifty years preceding 1910. It was in this year that Brazilian rubber touched $3.06 a pound, and this unquestionably was responsible for the start of work in this country. Work on the problem was begun at about the same time by Richard Earle and L. P. Kyriakides at the Hood Rubber Company, in Watertown, Massachusetts, and by David Spence, at the Diamond Rubber Company, Akron, Ohio. The Americans actually contributed little that was original, their work following mainly along lines that had already been plotted by the English and Germans.

Kyriakides was one of the first to realize the great importance of purity in his raw materials.

> It has been our experience [he wrote] that impure samples of isoprene never gave a good sample of rubber. It follows, therefore, that the hydrocarbon obtained by the older pyrogenic depolymerization methods of terpenes at ordinary pressures must be separated from the accompanying trimethyl ethylene before it is subjected to polymerization.

All subsequent work has served to confirm this observation. Kyriakides' isoprene was made by reacting ethyl magnesium chloride with chloroacetone and catalytically dehydrating the methyl-2-butyleneoxide-1, 2 which resulted. This pure isoprene was found to polymerize with sodium much more rapidly than had been reported by Harries. Butadiene was made from acetaldehyde by a process quite similar to that used in Germany, and was also polymerized with sodium.

The method chosen by Kyriakides and Earle in 1913 as being the most suitable for commercial development involved the preparation of dimethyl butadiene from pinacol derived from acetone, a synthesis first worked out in Germany. Plans were made for large-scale production of methyl rubber from this diolefin at an estimated cost of 80 cents a pound. Development work was abandoned when costs of wild rubber dropped sharply in this year and it also became evident that production on plantations was practical.

In summarizing his work, Kyriakides said:

> The preparation of hydrocarbons directly from the hydroxy compounds is the simplest of all known methods. . . . I believe that I have indicated the lines along which work will be done in the future for the commercial preparation of the hydrocarbons in the synthesis of "rubber." This problem will eventually be solved, although it will require a good many years of earnest and patient study. The starting point in these syntheses will most probably be alcohol, which can be produced fairly cheaply.

A prophetic statement!

Spence's 1910 work was also on isoprene, which he made initially from rubber itself in order to have raw material on which to test various polymerization catalysts. After selecting acetic anhydride and pyridine as the most promising catalysts, he made considerable rubber in 1911 from isoprene prepared from dipentene and turpentine. In the same year he made polymers of several substituted diolefins. After an intensive study of the relative merits of isoprene and dimethyl butadiene as starting materials, it was decided at the Diamond Company that the latter offered most promise. Both sodium and acetic anhydride were considered for catalysts, but no final choice was made by the time the work was dropped late in 1912 as a result of falling rubber prices.

The start of the first world war in 1914 caught the Germans without any rubber. They probably had never thought of its importance because rubber had not played much of a part in any other war in history. This time it was needed for gas masks, for dirigibles, for U-boat batteries, for telephone wires, and most of all for the tires on which trucks and autos and motorcycles moved. Because England controlled the sea, wild rubber could not get to Germany from South America or Africa. England owned the far-eastern plantations that by 1914 were out-producing the wild trees. People in the United States who wanted to buy rubber had to promise that it would not be transshipped to Germany. These promises were not always kept in the years before this country entered the war, but the offenders were barred from further dealings in plantation rubber. Twice in 1916 a submarine made the trip across the Atlantic, a feat never before accomplished, and each time took 500 tons of rubber back to Germany. Attempts were made to smuggle rubber out of this country in barrels of rosin, in bales of cotton waste, and even in jars of pickles. The small amounts that got through did not begin to supply the German need. Before the war was very old, all usable auto and bicycle tires in Germany were taken by the army. Even these were not enough, and trials were made of tires made of wood, of wood covered with leather or canvas, and of steel springs.

At first no one thought of using Fritz Hoffman's synthetic rubber from methyl isoprene because prewar plant trials had convinced German production men of its inferiority. The German need soon became so great that tests were made of some material left in Hoffman's 1910 plant, which had been partially dismantled after crude rubber prices fell. This material was found good enough to make hard-rubber cases for the batteries that drove the submarines, and it was decided to attempt its large-scale manufacture.

To make more methyl rubber, the Germans needed acetone, but the available acetone was needed for smokeless powder. More acetone could have been made from wood, but there was no wood to spare. It could have been made from grain or potatoes by fermentation processes, but these were badly needed for food. It could have been made from acetate of lime, but this had always been imported. Consequently, the Germans turned to acetylene from electric-furnace carbide. From this they made acetaldehyde by means of a mercurous sulfate catalyst; oxidized this to acetic acid, heated with lime to obtain calcium acetate; and dry-distilled this to acetone. The acetone was treated with aluminum and caustic to give an aluminum salt of pinacol, and this was distilled under pressure to give methyl isoprene.

An enormous number of experiments were run on ways of changing methyl isoprene into rubber. Three methods were finally chosen. By allowing the material to stand in tin drums for six to ten weeks at 30° C., H-rubber (H = *hart*) was produced. This was found very satisfactory for battery cases and hard-rubber electrical parts. By the spring of 1918 a ton a day was being produced for these purposes. By storing methyl isoprene in iron drums for three to six months at 70°C., W-rubber (W = *weich*) was made. This was used in soft-rubber goods but was far less satisfactory than the H-rubber was in hard rubber. Solid tires made of W-rubber were better than wood or steel, but that was about all that could be said for them. They were very inelastic and chipped badly in cold weather. In warmer weather, cars had to be jacked up when not running to keep the tires from developing flat spots. A third form, B-rubber, was made by allowing methyl isoprene to stand in contact with sodium wire in a carbon-dioxide atmosphere. This was suitable for insulated wire.

Near the end of the war, the Germans were making rubber at the rate of 150 tons a month. Altogether about 2,350 tons were made. When the war ended, two plants that would have boosted production to 8,000 tons a year were under construction.

THE end of the war also marked the end of the second phase of the synthetic rubber story. Interest in the subject disappeared completely until 1925, when restrictions on production sent the price of natural rubber to $1.25 a pound and a number of companies took up the question again. This was the start of what might be called the modern period in the history of synthetic rubber. In 1926 prices fell back to 35 cents, and everyone except Du Pont, I. G. Farben, and the Russians again lost interest. Du Pont's continued work led to the discovery of the chloroprene rubbers; I. G.'s work, to the bunas; the Russians concentrated on improving sodium-polymerized butadiene rubber.

Neoprene, which is the trade name given by Du Pont to its chloroprene rubbers, was discovered by a Catholic priest who was not looking for synthetic rubber at all, but was only interested in what he could do with acetylene gas. He was Julius A. Nieuwland, who came to this country from Belgium in 1880, when he was two years old. After graduating from Notre Dame, he entered Catholic University, in Washington, D. C., in 1903. There he became interested in chemistry and began the study of the reactions of acetylene that he pursued the rest of his life. In 1906 he happened to pass acetylene into a solution of copper chloride. The gas was absorbed as he expected, but at the same time he noticed a peculiar new odor he could not explain. He suspected formation of some new gas, but he was unable to isolate it. For fourteen years he returned to this experiment from time to time. In 1920 he hit upon the key. Instead of copper chloride, he was using the ammonium salt and the proper catalyst and succeeded in trapping not only a new gas but also a yellowish oil. After another year's work the new arrivals were identified. Acetylene itself consists of two atoms of hydrogen and two of carbon. The new gas had twice as many—two acetylene molecules had joined hands. In the case of the yellow liquid, three acetylene molecules had joined together. The gas was monovinyl acetylene; the liquid, divinyl acetylene.

Divinyl acetylene is extremely reactive, even more so than acetylene. Left to itself, it thickens into a jelly and then into a hard resin, which explodes with little encouragement. In spite of this, Nieuwland kept experimenting with the material and in 1923 found that when he treated it with sulfur chloride he obtained a soft, elastic solid. In 1925 he mentioned this fact at a chemical meeting in Rochester, and it immediately aroused the interest of Du Pont chemists who were present. They had been trying for several years to make synthetic rubber from acetylene without success. Arrangements for patent royalties were made, and the Du Pont chemists began working with Nieuwland. Their work with the explosive liquid was disappointing, because no two batches ever turned out alike. Finally, they began experimenting with the gas, monovinylacetylene, and Nieuwland suggested that it be reacted with hydrogen chloride. This was done, and a thin clear liquid, chloroprene, resulted. Chloroprene differs from isoprene chemically only in the fact that it contains one chlorine atom in each molecule. Practically, the difference between the two materials is very great, chloroprene polymerizing about seven hundred times as fast as isoprene.

Actually, polychloroprene (Neoprene) should not be called synthetic rubber; it has chlorine in its molecules, and rubber has none. Identical or not, it was the first practical material ever made that would do all that rubber would do just as well or even better. Neoprene was put on the market in 1931 and sold for $1.05 a pound at a time when rubber cost five cents. But Neoprene resisted oil and grease and was far better than rubber for gasoline hose, engine mounts, printing rolls, and similar items, and its use continued to mount until a million pounds a month were being used just prior to the second world war when rubber was still freely available. During the war, production and consumption of Neoprene reached a total of 46,000 tons a year, but it has since fallen somewhat from this peak.

Brief mention might be made at this point of the other early American discovery in rubberlike materials. As in the case of Neoprene, this product was not at all like rubber chemically but did some jobs better. Again, as with Neoprene, its inventor was not looking for synthetic rubber. Thiokol, as this synthetic is called, was discovered by J. C. Patrick in 1928 when he was trying to develop new antifreeze mixtures for radiators. One day he mixed ethylene dichloride and sodium polysulfide and obtained a sticky, gummy mess that could not possibly be used in a radiator. Disgusted, Patrick threw the mess away. If he had had a one-track mind, he would have gone on looking for antifreeze and never have given his unsuccessful experiment another thought. Instead, he began to wonder if the gummy material might not be good for some other use. This proved to be the case. Thiokol is much more resistant than rubber or any of the other synthetics or aromatic solvents. It is not strong, however, and does not make very satisfactory articles by itself. When strength can be supplied by some other material, as by cotton fabric in hose, it is quite satisfactory, and there is a steady demand for about 1,000 tons a year.

During these years when American chemists were developing the first practical synthetic rubberlike materials, the Germans and Russians were also busy. Something else besides the high price of rubber in 1925 was spurring them on. Both were more concerned about national self-sufficiency than they were about the economics of synthetic rubber production and for that reason continued their work when other countries lost interest.

As early as 1915, the Russian chemist Ostromislenski had made butadiene by the catalytic action of aluminum oxide on a mixture of alcohol and acetaldehyde. Incidentally, this was the forerunner of the process used so successfully by the Carbide and Carbon Chemical Corporation in our own wartime butadiene program. In 1925 Lebedev modified Ostromislenski's method and developed it into a commercial process, the only direct method of converting ethanol into butadiene with a satisfactory yield. A mixture of catalysts that split off water and hydrogen at the same time was used. The butadiene so obtained was polymerized with sodium to the so-called SK rubber. Sodium polymerization presents certain difficulties, which caused the Germans to abandon this method. The principal trouble arises from local overheating, which is difficult to avoid in a mass-polymerization method. Unlike the Germans, the Russians stayed with the sodium process and solved the problems involved. This was accomplished mainly by changing from a batch process employing sodium rods to a continous process in which butadiene in the vapor phase is polymerized by a sodium dispersion. Fairly large-scale production of SK rubber is reported to have begun in 1932, and by 1939 capacity was reported at 90,000 tons annually by our own Department of Commerce.

About 1925 the Germans had begun to concentrate on butadiene rather than on isoprene (methyl butadiene) or methyl isoprene (dimethyl butadiene) because it is structurally the simplest molecule that will polymerize readily. The butadiene was made from electric-furnace acetylene.

Using sodium, they first developed a series of synthetic rubbers called the numbered bunas (Buna 85, Buna 115, etc.). The first two letters of the name stood for butadiene, the second two for natrium (sodium), and the numbers indicated the molecular weights in thousands. The numbered bunas were never very satisfactory except for hard rubber. Consequently, the Germans decided to abandon sodium polymerization and concentrate their efforts on emulsion polymerization, since this appeared more practical.

The idea of emulsion polymerization was not new, the first patent for this method having been taken out by Bayer in 1912 for the polymerization of isoprene in an aqueous solution of egg albumen, starch, or gelatine. A similar patent in 1913 covered similar treatment of butadiene. Quite possibly, the first conception of this process resulted from analogy with the latex of the rubber tree.

Polymerization times that at first were measured in weeks were gradually reduced to days and then to hours by development of improved catalysts, but quality of the polybutadiene still left a great deal to be desired.

In their efforts to improve their butadiene rubber, the Germans had wondered if they might be able to overcome its leathery character by modifying it with some other material; in other words, by making a copolymer instead of a single polymer. In the late twenties, at the same time that they were trying to improve their straight polybutadiene rubber and were actually getting into pilot-plant production on it, they were trying all sorts of materials to make softer and more flexible bunas. The two best materials that were found were styrene (vinyl benzene) and acrylonitrile (vinyl cyanide). The inventors of the styrene copolymer, which came to be known as Buna S, were Eduard Tschunkur and Walter Bock, of the Leverkusen works of I. G. Farben. The acrylonitrile copolymer, which was called Buna N, was invented by Tschunkur and Erich Konrad. Buna S was a general-purpose type of synthetic rubber, and Buna N was for oil-resistant uses.

Development of the two butadiene copolymers led Dr. Schacht, of the Hitler cabinet, to announce to the world at large in 1934 that the synthetic rubber problem had been solved and that production would start in a short time. A year later production amounted to 25 tons a month. The timetable called for production of 200 tons a month in 1936 and 1,000 tons a month by 1938. This schedule was not met, but production did reach about 5,000 tons a year in 1938, and is reported to have reached 60,000–70,000 tons annually in 1941. German rubber-goods manufacturers were compelled to use every pound produced whether they wanted to or not in order to reduce costly imports, which upset Germany's foreign trade balance.

First word about the straight butadiene polymers, the numbered bunas, reached this country in the late twenties after both Neoprene and Thiokol had been discovered here. In 1930 Standard Oil of New Jersey and I. G. Farben formed the Joint American Study Company for the commercial testing and licensing of new processes developed by either party for making chemical products from petroleum raw materials. One of the buna processes was the first thing taken up by the new company. In 1933 an agreement was made with General Tire Company for testing the polybutadiene rubber, and substantial samples were sent to the American factory. The report on this material was quite unfavorable.

The copolymer of butadiene with acrylonitrile (Buna N) was introduced in this country in 1937. An explosion at the Du Pont Neoprene plant left rubber-goods manufacturers here without a domestic source of oil-resistant rubber, which by this time had become one of their regular raw materials, albeit in small volume. A small shipment of I. G.'s Perbunan (Buna N) was made, and it worked so well that a small but steady demand was created.

In addition to the Buna N, samples of Buna S began to reach this country in 1937, and American rubber men acquired some familiarity with it. During the middle thirties, some of the leading American rubber chemists, namely Dinsmore, Sebrell, and Semon, visited the I. G. plants in Germany to observe what was being done in the emulsion polymerization of butadiene copolymers. By the end of the decade, two of the four largest American rubber companies were producing butadiene copolymers on a pilot-plant scale, and in 1940 the other two obtained licenses from Standard Oil to make Buna S and Buna N. Standard, as an equal partner in JASCO

(Joint American Study Company) had the rights to the buna rubbers outside Germany.

In 1940 our government began to be concerned about rubber supplies and showed some interest in making synthetic rubber. Chemists of the four largest companies recommended Buna S as the rubber that should be made. This was an obvious choice since Buna S was the only general-purpose synthetic with which the American rubber-goods manufacturing industry had any background of experience. At first, plans were drawn for four 2,500-ton units. During the summer of 1941, the size of these plants was increased to 10,000 tons apiece. When we were attacked in December 1941, they were jumped to 30,000 tons each. As the war progressed and more and more of the natural rubber lands were lost, additional plants were contracted for until a total designed capacity of 705,000 tons was reached.

For several reasons, a detailed account will not be given here of this country's wartime Buna S program. The story has already been told adequately several times (e.g., SM, January, 1944). Further, it would be out of place here because, actually, no fundamental scientific discovery was made. Without question, the construction of our wartime synthetic rubber industry was a tremendous chemical engineering feat, perhaps the greatest the world has ever seen. The fact remains, however, that we duplicated an existing product, we did not discover a new one. The nationalistic-minded claim that GR-S should not be called Buna S because it is far superior. This is quite true, but basically the two materials are the same—approximately three parts of butadiene to one of styrene copolymerized in emulsion. The modifiers, additives, and improved techniques discovered in this country do not alter the fact that GR-S and Buna S are fundamentally identical and that Buna S was entirely a German discovery.

Not so with butyl rubber. Like Neoprene and Thiokol, butyl is purely an American discovery, although it is an outgrowth of an earlier German invention. About 1932 I. G. chemists learned how to polymerize a refinery gas, isobutylene, into a white, rubberlike solid by using a boron fluoride catalyst at low temperature. Polyisobutylene, or "Oppanol," as it was called by the Germans, is a saturated material; that is, the carbon atoms have no free bonds, and, consequently, it is a completely unreactive material. Unlike rubber, it will not vulcanize or combine with sulfur. As a result, the German discovery was of no more practical value than was natural rubber before Charles Goodyear learned how to vulcanize it in 1839. Polyisobutylene becomes sticky when warmed. Articles made of it will not hold their form, but gradually flow into a shapeless mass.

This flexible, rubberlike solid attracted the interest of two young Standard Oil chemists, William J. Sparks and Robert M. Thomas, in 1937. They reasoned that if some other material could be made to copolymerize along with the isobutylene, the product might have a few carbon atoms with reactive double bonds. This proved to be very clear thinking. Sparks and Thomas found that, when they added 2 or 3 percent of butadiene, isoprene, or methyl isoprene to the isobutylene and polymerized the two together, they had a useful synthetic rubber. There was just enough unsaturation in a molecule of butyl rubber to permit it to vulcanize. It then lost all the faults of the original German Oppanol and would hold its shape as well as natural rubber. On the other hand, it retained all the inertness of the original polymer and was but little attacked by oxygen or strong chemicals.

Butyl rubber is made by a unique method of polymerization. Isobutylene and isoprene are first cooled to 150° below zero. At this point they are brought into contact with the aluminum chloride catalyst, and the butyl rubber forms instantly. The particles of rubber are quenched in hot water to stop the activity of the catalyst, and the product is then dried and massed.

Butyl was not introduced to the rubber-goods manufacturing industry until 1940. Consequently, rubber chemists knew little about it when they had to decide what kind of synthetic the government was going to make; they picked Buna S because they were familiar with it and knew that the Germans were using it. First plans did not call for the government to make any butyl at all. Standard Oil started to build a small plant of its own. After the war started the government took over this plant and increased its capacity to 7,200 tons a year. When it developed that Buna S was unsatisfactory for inner tubes and that butyl was superior even to natural rubber, the facilities for producing butyl in this country were increased to 68,000 tons. The reason for butyl's marked superiority over natural rubber in this one application is that it holds air ten times longer.

Eighty-eight years have only brought synthetic rubber to the threshold of maturity. Although hundreds of thousands of tons of it have been used in the past five years, most of it was used because of dire necessity. General-purpose synthetic rubber has not yet really had to compete on merit. Today it faces the hottest kind of competition, as natural rubber is permitted to enter this country freely. This competition will undoubtedly stimulate the development of better and cheaper synthetic rubbers. Eventually, the natural-rubber plantations may go the way of the indigo fields of India and the madder farms of France, and *Hevea brasiliensis* may join those other ex-aristocrats of the vegetable kingdom, the camphor and cinchona trees, in the limbo to which they have been relegated by the organic chemist.

PARABLE OF THE TWO YOUNG PROSPECTORS AND THEIR RICH UNCLE

TWO Young Prospectors, their eyes shining with a Vision of Discovery, went to their rich Uncle and said: "Uncle, we wish to search for precious ores in the Unknown Mountains. Will you grubstake us?"

Now the rich Uncle was worldly-wise and cautious. He replied: "Perhaps. But first you must tell me what precious ores you propose to find, in what condition and quantity, the length of time that it will take you to accomplish this, and the amount of money that it will cost. Here is a neatly printed Form with appropriate blanks for this information."

"But Uncle," exclaimed the Prospectors, "how can we truthfully tell you this? No one has explored the Unknown Mountains. We may search for years and find nothing. If we could foretell what we would find, it would not be prospecting."

"Young men," replied the Uncle, "I have Responsibilities to my Family; I cannot risk their money on what may be a fool's errand. Let me grubstake you to work in the Old Diggings. You may not discover precious ores, but you can be sure of finding enough of the baser metal to guarantee the safety of my money."

The First Young Prospector took the neatly printed Form, filled it out, and set off for the Old Diggings. The years passed, each year bringing its small but regular return of the baser metal. At last the Uncle called in the First Prospector. "You have been so faithful in the Old Diggings," he said, "that now I will grubstake you to prospect in the Unknown Mountains." But the First Prospector was no longer young; no longer were his eyes shining with the Vision of Discovery. He now supervised a crew of young diggers. "Why should I leave my comfortable home, my assured income from the baser metal?" he thought. He did not regret that he no longer had the Vision. He had forgotten it.

The Second Young Prospector rejected his Uncle's proposal to work in the Old Diggings. He became a Teacher. Of his limited salary he put aside what little he could spare from his frugal living, until the sum became a little grubstake. With this, in vacation time and on a small scale, he prospected the Unknown Mountains. He did not find the great mother lode of precious ore; prospectors from other lands did that; but he did discover its outcropping, and other men, following the trail that he had blazed, gradually transformed the Unknown Mountains into the New Diggings.

The years passed, and the Second Prospector was poor and no longer young, but his eyes still shone with the Vision of Discovery, and now and then a spark of that Vision kindled a flame in the eyes of one of the young prospectors who sat at the Teacher's feet, and now and then one of these young prospectors, grubstaking himself as best he could, set out to prospect in other Unknown Mountains.

K. Starr Chester

PRINCIPLES OF REALLY SOUND THINKING

MAX BLACK

Dr. Black, a native of Baku, Russia, was educated in European universities and received his Ph.D. from the University of London. He came to this country for the first time in 1938. In 1940 he was made professor of philosophy at the University of Illinois and since 1946 has held the same post at Cornell. Dr. Black is editor of the Journal of Symbolic Logic *and author of the very successful* Critical Thinking *(Prentice-Hall, 1946).*

THERE is thinking and thinking; we shall be concerned with the second kind. Textbooks are still full of impracticable principles of logic for which men of affairs have unqualified contempt. It is time to be more realistic. But for the dead hand of Aristotle and the lingering influence of medieval scholasticism, logicians would not have the impudence to refer to practical thinking as "invalid" or "poor." Such emotive language should be avoided. The principle of identity should have taught academic logicians that Really Sound Thinking can be nothing if not Really Sound.

We are interested in the way the man in the street should think. Formal logic should be left to mathematicians and scientists who get paid for trafficking in abstractions. Symbolic logic should be left to those lonely thinkers who understand the symbols.

There are two major principles of really sound thinking:

A. Think only as a last resource.
B. Trust your feelings.

THINK ONLY AS A LAST RESOURCE

The really sound thinker knows thinking to be an uncomfortable, disturbing, and antisocial occupation. Consider the attitude of Rodin's statue "The Thinker." This is not the favorite posture of a successful executive or a regular guy.

Modern life fortunately provides a number of defenses against the early onslaught of thinking. The radio is always close at hand —use it. The company of others, preferably of the opposite sex, is to be strongly recommended. If the irritation is too severe, one may retreat to bed until restored to a healthier frame of mind.

It has to be confessed, however, that complete protection against thinking still remains to be achieved:

> But men at whiles are sober
> And think by fits and starts
> And if they think, they fasten
> Their hands upon their hearts.
> —A. E. HOUSMAN

Conscientious adherence to the next principle will go far to palliate the discomforts of unavoidable and involuntary thinking.

TRUST YOUR FEELINGS

The logic texts have created the fiction of Logical Man, coldly calculating the probabilities of alternative hypotheses, willfully blind to human sentiment and passion. Do you want to be this kind of philosophical monster, interminably vacillating between conflicting conclusions? Of course not. In any matter of serious concern, you will *feel* strongly that a certain conclusion *must* be right. This is the clue to success in really sound thinking. Let yourself go—think in technicolor.

Suppose you are worried about the possibility of war with Russia. You will notice in yourself a tendency to think of Stalin as a bloodthirsty ruffian, dripping with the gore of murdered innocents. Dwell upon the notion —let your blood pressure rise. In a short time you will *feel* strongly enough to be able to stop *thinking* altogether. In really sound thinking, it is the conclusion that counts, not the premises. Trusting your feelings will quickly provide you with satisfying, heartwarming conclusions.

THE two principles of really sound thinking can be illustrated by the following maxims, widely accepted by successful practitioners.

1. *If you must stick to the point, be sure it's blunt.* The natural human reaction to contact with a sharp point is violent motion in reverse. Such animal wisdom is deeply significant. It's the dead butterfly that stays on the point. Cultivate judicious irrelevance.

Example: Does John Smith deserve a raise in salary?
Blunted point: Doesn't everybody deserve a raise?
Really sound reasoning: Of course they do! Who is John Smith to be favored at the expense of everybody else?

2. *What's in it for me?* Remember that a really sound thinker is practical. And what can be more practical than concern for one's own interests? The chief advantage of this maxim is the strong light it throws upon the truth of many a debatable proposition.

Example: Should educational facilities be improved in the South?
Really sound reasoning: What's in it for me? Nothing—*I* don't live in the South.
Conclusion: NO. (Notice the directness and incisiveness of the method.)

3. *It all depends on who says it.* Men are easier to classify than arguments—attend to the man, not the argument. (For classifying the speaker, see principle B above.)

Example: Should Congress be reorganized?
Really sound reasoning: Who says so? X? Oh—he ran for Congress three times unsuccessfully.
Conclusion: You can't trust *him.*

4. *A million people can't be wrong.* It would clearly be undemocratic, not to say snobbish, to think otherwise. We can't all be Gallups, but we have a ready fund of popular wisdom to hand in the form of proverbs. Make frequent use of such axioms as "Human nature never changes," and "An ounce of experience is worth a peck of talk," and, especially, "It will all be the same in a thousand years." The last is particularly consoling.

Example: Can we prevent another war?
R.s.r.: I've *seen* men fighting. You'll never change human nature. After all, it will all be the same in a thousand years.

This method can be usefully supplemented by the use of identical propositions, such as "East is East, West is West," "Business is Business," "A man's a man for a' that." These are best introduced by the words "after all." Even a logician can hardly dispute the truth of such tautologies.

5. *The exception proves the rule.* Corollary: The more exceptions, the better the rule. This popular maxim hardly needs recommendation. It has the great advantage of allowing us to make simple generalizations in an intolerably complex world. (See also Maxim 8 below.)

Example: You say women are no good at physics. What about Madame Curie?
R.s.r.: The exception proves the rule! (Absolutely conclusive, as r.s.r. should be.)

6. *It's all right in theory but it won't work in practice.* We might almost say: *Because* it's right in theory, it won't work in practice. This maxim is very useful in puncturing the pretentions of experts.

Example: Should we support the United Nations?
R.s.r.: (You know what!)

7. *Consistency is the hobgoblin of little minds.* None of the great thinkers from Socrates to Korzybski have been consistent. Who are you to improve upon their practice? The sciences are notoriously full of unresolved contradictions. If scientists don't care, why should you?

Example: You say that we ought to work for universal free trade, but insist on raising American tariffs.
R.s.r.: I contradict myself? Very well, I contradict myself.

8. *Truth is always pure and simple.* Notice the purity and simplicity of this maxim. Oscar Wilde denied its truth, and see what happened to him (compare Maxim 3). Anything too complicated for translation into Basic English is unworthy the attention of a really sound thinker. The truth must be incapable of shocking the Johnston (formerly Hays) Office. In any case, truth is too precious to be lightly squandered. It is better to hold it a closely guarded hostage far back in reserve.*

Example: Is there anything in psychoanalysis?
R.s.r.: Of course not. Why, I can't even understand it. And it isn't fit to print.

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* With acknowledgments to Peter Fleming.

9. *Take care of the sound and the sense will take care of itself.* This is perhaps the most important of all the maxims of really sound thinking. "There is a great advantage in names" (Mark van Doren). Be sure you get the greatest benefit out of the names you use.

If you trust your feelings as you should (principle B above), you should have little trouble in finding the right name. Thus, the *Management Review* lately recommended the use of "Income Account" instead of "Profit and Loss Account," "Earnings" instead of "Profits," "Reinvested in the Business" instead of "Added to Surplus." You get the idea?

Example: On being questioned about the implications of a legislative program no really sound thinker would say, "I don't know." This is better: "It is totally unreasonable to expect a blueprint which answers every question which can arise day after tomorrow in this distraught earth, when no man knows for twenty minutes at a time what is going to happen" (Senator Vandenberg, quoted in the *New York Times,* April 18, 1947).

10. *Never argue with a man who is wrong.* Corollary for married ladies: Never argue with a husband. For the purposes of this maxim, a man who is wrong is easily identified as one who (a) is an unsound thinker, (b) refuses to see that you are right, or (c) has an unwholesome look (see Maxim 3 above).

The careful reader will have noticed that the reasoning used in the above exposition of the principles of really sound thinking provides numerous further illustrations of the principles discussed.

ARCTIC TERN

Bird of snow,
white, white, whiter than anything
(flake on feathered flake,
wing spread to wing)
over what leagueless ocean
have you come,
past what green isles,
to rest at last
(unwearied, graceful still)
upon a floating skyscraper of ice—
or to a lonely sandbank
where the sun
leans shadowless?
Now, the long flight over
(flight of wind,
flight of drifting snow)
find once again your nest,
your mate . . .
and in the hangar of the Arctic night
rehearse your Odyssey of flight.

MAE WINKLER GOODMAN

MAN'S CONTROL OVER CIVILIZATION: AN ANTHROPOCENTRIC ILLUSION

LESLIE A. WHITE

After taking his Ph.D. at the University of Chicago in 1927, Professor White taught at the University of Buffalo and served at the same time as a curator of the Buffalo Museum of Science. He remained in Buffalo until 1930, when he joined the staff of the University of Michigan, where he is now professor of anthropology. His specialty is the evolution of culture.

. . . numerous survivals of the anthropocentric bias still remain and here [in sociology], as elsewhere, they bar the way to science. It displeases man to renounce the unlimited power over the social order he has so long attributed to himself; and on the other hand, it seems to him that, if collective forces really exist, he is necessarily obliged to submit to them without being able to modify them. This makes him inclined to deny their existence. In vain have repeated experiences taught him that this omnipotence, the illusion of which he complacently entertains, has always been a cause of weakness in him; that his power over things really began only when he recognized that they have a nature of their own, and resigned himself to learning this nature from them. Rejected by all other sciences, this deplorable prejudice stubbornly maintains itself in sociology. Nothing is more urgent than to liberate our science from it, and this is the principal purpose of our efforts.—EMILE DURKHEIM.

. . . it appears like a grandiose dream to think of controlling according to the will of man the course of social evolution.—WM. F. OGBURN.

THE belief that man controls his civilization is widespread and deeply rooted. Customs and institutions, tools and machines, science, art, and philosophy are but man's creations and are therefore here only to do his bidding. It lies within man's power, therefore, to chart his course as he pleases, to mold civilization to his desires and needs. At least so he fondly believes.

Thus we find a distinguished British scientist, the late Sir James Jeans, assuring us that

> We no longer believe that human destiny is a plaything for spirits, good and evil, or for the machinations of the Devil. There is nothing to prevent our making the earth a paradise again—except ourselves. The scientific age has dawned, and we recognize that man himself is the master of his fate, the captain of his soul. He controls the course of his ship, and so, of course, is free to navigate it into fair waters or foul, or even to run it on the rocks.

Mr. Stanley Field, president of the Chicago Natural History Museum, appeals to anthropologists in espousing free will:

> But if we listen to the anthropologists, who can scientifically demonstrate that it is not color of skin, or type of hair or features, or difference of religion, that creates problems between peoples, but factors for which man is responsible and which he can control or change if he will, then we shall at least come within sight of that better world which we now realize we must achieve if we are not finally to perish as victims of our own perversity.

Professor Lewis G. Westgate (SM, August 1943, 165) tells us that man can "take the problem of his future in hand and solve it. . . . The mind that can weigh the infinitely distant stars . . . track down the minute carriers of disease . . . dig the Panama Canal . . . can solve its social problems when and if it decides to do so."

It would seem that the salvation of an earlier era has become the social reconstruction of today: we can achieve it if we will; if we fail, it is because of our "perversity."

When, however, we look for examples of man's control over culture, we begin first to wonder, then to doubt. We shall not begin our inquiry by asking if two world wars in one generation are evidence of planning or perversity, or whether Germany and Japan were crushed and Soviet Russia made dominant in Eurasia in accordance with a far-sighted plan or as a result of blindness and folly. We shall start with something much more modest. During the past century we have witnessed attempts to control tiny and relatively insignificant segments of our culture, such as spelling, the calendar, and the system of weights and measures, to name but a few. There have been repeated and heroic attempts to simplify spelling and make it more rational, to devise a more rational calendar, and to adopt an ordered system of weights and measures instead of the cumbersome, illogical agglomeration of folk measurements we now use. But what successes can we point to? Reform in spelling has been negligible. We have succeeded to a considerable extent but not wholly in eliminating the *u* from such words as *honor*. But to do away with silent letters, such as the *b* in *lamb*, is too big a mountain for us to move. And such spellings and pronunciations as *rough, cough, dough,* and *through* are much too strong to yield to our puny efforts. It usually takes a great political and social upheaval to effect a significant change in spelling or a calendrical system as the French and Bolshevik revolutions have made clear. And as for the metric system, it has found a place among the little band of esoterics in science, but yards, ounces, rods, pints, and furlongs still serve—awkwardly and inefficiently—the layman.

We begin to wonder. If we are not able to perform such tiny and insignificant feats as eliminating the *b* from *lamb,* or modifying our calendar, how can we hope to construct a new social order on a world-wide scale?

Let us look about us further. Men and women are forever contending with fashions. Man perennially rebels against his attire. It is often uncomfortable, injurious to the health at times, and, some men think, the ordinary costume is unaesthetic, the formal attire ridiculous. But what can he do? He must wear his coat and tie no matter how hot the weather. He is not permitted to wear pink or blue shoes. And as for "evening clothes" —he must submit to them or stay home. Man's vaunted control over civilization is not particularly conspicuous in this sector.

But if man is helpless, woman is an abject slave, in the grip of fashion. She must submit to any change, no matter how fantastic or ugly. To be sure, she may not realize that the new designs are fantastic and ugly at the time; "the latest style" can becloud a woman's judgment. But one has only to browse through an album of old snapshots to realize that beauty, grace, and charm do not dominate the course of fashion.

And as for women's skirts! First they are short; then they are long. A distinguished anthropologist, Professor A. L. Kroeber, of the University of California, has made a very interesting and revealing study of the dimensions of women's dresses over a considerable period of time. He found that "the basic dimensions of modern European feminine dress alternate with fair regularity between maxima and minima which in most cases average about fifty years apart so that the full-wave length of their periodicity is around a century." The rhythms are regular and uniform. Women have nothing to say about it. Even the designers and creators must conform to the curve of change. We find no control by man—or woman—here, only an inexorable and impersonal trend. When a maximum point on the curve is reached, the trend is reversed and skirts lengthen or shorten as the case may be. Women are helpless; they can do nothing but follow the trend. When the curve ascends they must shorten their dresses; when it descends, they must lengthen them. It may seem remarkable that a great class of citizens who cannot even control the dimensions of their own skirts will nevertheless organize themselves into clubs to administer the affairs of the world. We shall return to this point later.

Few men would undertake to repair an automobile or a radio without some understanding of its mechanism. We tend more and more nowadays to leave medicine and surgery to those who know how. Knowledge and skill are required even to make good pies or home brew. But in matters of society and culture everyone feels qualified to analyze, diagnose, and prescribe. It is one of the premises of democracy that not only do the people rule, but they have the requisite knowledge and understanding to do it effectively. In matters political, one man's view is as good as another's.

When, however, we examine the knowledge and understanding with which the affairs of the nation are administered, we begin again to wonder. We find the most august authorities espousing different and even contradictory views on such subjects as inflation, the function of labor leaders, the divorce rate, the popularity of crooners, and so on. This is a picture of the anarchy of ignorance.

When we turn from matters of national proportions, such as the cause of inflation, to lesser problems, we are not always reassured. Does capital punishment diminish the number of murders? Does the use of alcohol affect the divorce rate? Why do people keep dogs? They are noisy, dirty, unhealthful, useless, and expensive. To say that they are kept because people like them is merely to phrase the problem in a different form. Why don't they "like" raccoons? They are cute, cleanly in their habits, and very amiable.

The fact is we don't really know very much about the civilization we live in. Let us take one of the simplest and most elementary questions imaginable: Why does our society prohibit polygamy? Other societies permit plural mates, and Western Europe once did also. But now we feel very strongly about it. We will put a man in a prison for years if he takes unto himself more than one wife at one time. His wives may be perfectly satisfied with the arrangement and he may have injured no one. Yet we put him in gaol. Why?* Why not have one more wife and one less schoolmarm? There are, to be sure, ready answers to these questions: polygamy is "wrong," "immoral," "undemocratic," etc. But practices are not prohibited because they are "wrong"; they are wrong because they have been prohibited. It is not wrong to buy and sell whisky now; it was while the Eighteenth Amendment was on the books. And as for democracy, we permit a man to have two yachts if he can afford them, why not two wives?

I know of no really adequate answer to this question in such literature of social science as I am acquainted with. As a matter of fact, the question is very seldom raised. I have looked for it in a great number of treatises on sociology and anthropology written during the past quarter century without finding it. Some social scientists of the latter half of the nineteenth century tried to explain the prohibition of polygamy, but we cannot accept their conclusions.

The fact is we are ignorant. We do not know the solution to such an elementary problem as singular or plural mates. And in our day, we have not reached the point of asking such questions, to say nothing of answering them. As Archibald MacLeish has said, "We know all the answers, but we have not yet asked the questions." Over half a century ago the great French savant, Emile Durkheim, commented upon the immaturity of social science as follows:

> In the present state of the science we really do not even know what are the principal social institutions, such as the state, or the family; what is the right of property or contract. . . . We are almost completely ignorant of the factors on which they depend . . . ; we are scarcely beginning to shed even a glimmer of light on some of these points. Yet one has only to glance through the works on sociology to see how rare is the appreciation of this ignorance and these difficulties.

Despite the progress that has been made since *The Rules of Sociological Method* was written, this statement has a certain relevance today. If the science of society and civilization is still so immature as to be unable to solve such a tiny and elementary problem as the prohibition of polygamy, where are the knowledge and understanding requisite to planning a new social system, to constructing a new world order? One would not expect a savage craftsman, whose best tools are made of chipped flint, to design and build a locomotive.

LET us have a look at this civilization man thinks he controls. The first thing we notice is its antiquity. There is no part of it, whether it be technology, institutions, science, or philosophy, that does not have its roots in the remote past. The lens of the new 200-inch telescope, for example, is made of glass. Glass emerged from the manufacture of faïence in ancient Egypt, which in turn originated apparently as a by-product of burning bricks and pottery, which followed the use of sun-dried brick and, earlier, mud daubs of Neolithic, or even Paleolithic, huts. The United Nations can be traced back to primitive tribal councils and beyond. Modern mathematics goes back to counting on one's fingers, and so on. Culture is as old as man himself. It had its beginnings a million-odd years ago when man first started to use articulate speech, and it has continued and developed to the present day. Culture is a continuous, cumulative, and progressive affair.

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* We recall a recent instance in which a man was sent to the penitentiary for marrying some twelve women without ever bothering with the ritual of divorce. Had he been less honorable or chivalrous and lived with each woman without the formality of marriage, his "crime" would have been much less. This man served society well in a municipal railway system. His numerous wives pressed no complaint. Why did society feel it necessary to incarcerate him?

Everyone—every individual, every generation, every group—has, since the very earliest period of human history, been born into a culture, a civilization, of some sort. It might be simple, crude, and meager, or it might be highly developed. But all cultures, whatever their respective degrees of development, have technologies (tools, machines), social systems (customs, institutions), beliefs (lore, philosophy, science), and forms of art. This means that when a baby is born into a cultural milieu, he will be influenced by it. As a matter of fact, his culture will determine how he will think, feel, and act. It will determine what language he will speak, what clothes if any he will wear, what gods he will believe in, and how he will marry, select and prepare his foods, treat the sick, and dispose of the dead. What else *could* one do but react to the culture that surrounds him from birth to death? No people makes its own culture; it inherits it ready-made from its ancestors or borrows it from its neighbors.

It is easy enough for man to believe that he has made his culture, each generation contributing its share, and that it is he who controls and directs its course through the ages. Does he not chip the arrowheads and stone axes, build carts and dynamos, coin money and spend it, elect presidents and depose kings, compose symphonies and carve statues, worship gods and wage war? But one cannot always rely upon the obvious. It was once obvious that the earth remained stationary while the sun moved; anyone could see that for himself. We are now approaching a point in modern thought where we are beginning to suspect that it is not man who controls culture but the other way around. The feat of Copernicus in dispelling the geocentric illusion more than four hundred years ago is being duplicated in our day by the culturologist who is dissipating the illusion that man controls his culture.

Although it is man who chips arrowheads, composes symphonies, etc., we cannot explain culture merely by saying that "man produced it." There is not a single question that we would want to ask about culture that can be answered by saying, "Man did thus and so." We want to know why culture developed as it did; why it assumed a great variety of forms while preserving at the same time a certain uniformity; why the rate of cultural change has accelerated. We want to know why some cultures have money and slaves and others do not; why some have trial by jury, others ordeal by magic; why some have kings, others chiefs or presidents; why some use milk, others loathe it; why some permit, others prohibit, polygamy. To explain all these things by saying, "Man wanted them that way," is of course absurd. A device that explains everything explains nothing.

Before we go very far, we discover that we must disregard man entirely in our efforts to explain cultural growth and cultural differences—in short, culture or civilization as a whole. Man may be regarded as a constant so far as cultural change is concerned. Man is one species, and, despite differences of skin, eye, and hair color, shape of head, lips, and nose, stature, etc., which after all are superficial, he is highly uniform in such fundamental features as brain, bone, muscle, glands, and sense organs. And he has undergone no appreciable evolutionary change during the past 50,000 years at least. We may, therefore, regard man as a constant, both with regard to the races extant today and with regard to his ancestors during the past tens of thousands of years. Man has a certain structure and certain functions; he has certain desires and needs. These are related to culture, of course, but only in a *general*, not a specific, way. We may say that culture as a whole serves the needs of man as a species. But this does not and cannot help us at all when we try to account for the variations of specific cultures. You cannot explain variables by appeal to a constant. You cannot explain the vast range of cultural variation by invoking man, a biological constant. In England in A.D. 1500 there was one type of culture; in Japan, another. Neither culture can be explained in terms of the physical type associated with it. Culture underwent change in England between A.D. 1500 and 1900, as it did in Japan. But these changes cannot be explained by pointing to the inhabitants in each case; they did not change. Plainly, we cannot explain cultures in terms of Man.

Nor can cultural differences be explained in terms of environment. Quite apart from the difficulty of accounting for differences in musical styles, forms of writing, codes of etiquette, rules of marriage, mortuary rites, etc., in terms of environment, we soon discover that even economic, industrial, and social systems cannot be so explained. The environment of central Europe so far as climate, flora, fauna, topography, and mineral resources are concerned has remained constant for centuries. The culture of the region, however, has varied enormously. Here again we see the fallacy of explaining the variable by appeal to a constant.

If, then, we cannot explain cultures in terms of race or physical type, or in terms of psychological processes, and if appeal to environment is equally futile, how *are* they to be accounted for and made intelligible to us?

There seems to be only one answer left, and that is fairly plain—after one becomes used to it, at least. Culture must be explained in terms of culture. As we have already noted, culture is a continuum. Each trait or organization of traits, each stage of development, grows out of an earlier cultural situation. The steam engine can be traced back to the origins of metallurgy and fire. International cartels have grown out of all the processes of exchange and distribution since the Old Stone Age and before. Our science, philosophy, religion, and art have developed out of earlier forms. Culture is a vast stream of tools, utensils, customs, and beliefs that are constantly interacting with one another, creating new combinations and syntheses. New elements are added constantly to the stream; obsolete traits drop out. The culture of today is but the cross section of this stream at the present moment, the resultant of the age-old process of interaction, selection, rejection, and accumulation that has preceded us. And the culture of tomorrow will be but the culture of today plus one more day's growth. The numerical coefficient of today's culture may be said to be 365,000,000 (i.e., a million years of days); that of tomorrow, 365,000,000 + 1. The culture of the present was determined by the past, and the culture of the future will be but a continuation of the trend of the present. Thus, in a very real sense, *culture makes itself*. At least, if one wishes to explain culture scientifically, he must proceed *as if* culture made itself, *as if* man had nothing to do with the determination of its course or content. Man must *be* there, of course, to make the existence of the culture process possible. But the nature and behavior of the process itself is self-determined. It rests upon its own principles; it is governed by its own laws.

Thus, culture makes man what he is and at the same time makes itself. An Eskimo, Bantu, Tibetan, Swede, or American is what he is, he thinks, feels, and acts as he does because his culture influences—"stimulates"—him in such a way as to evoke these responses. The Eskimo or American has had no part in producing the culture into which he was thrust at birth. It was already there; he could not escape it; he could do nothing but react to it, and that on its own terms. The English language, the Christian religion, our political institutions, our mills, mines, factories, railroads, telephones, armies, navies, race tracks, dance halls, and all the other thousands of things that comprise our civilization are here in existence today. They have weight, mass, and momentum. They cannot be made to disappear by waving a wand, nor can their structure and behavior be altered by an act of will. We must come to terms with them as we find them today. And they will be tomorrow what their trend of development in the past dictates. We can only trot along with them, hoping to keep up.

MAN has long cherished the illusion of omnipotence. It is flattering and comforting to his ego. In days gone by, man has believed that he could control the weather; countless primitive peoples have had rituals for making rain, stilling high winds, or averting storms. Many have had ceremonies by means of which the course of the sun in the heavens could be "controlled." With the advance of science, however, man's faith in his omnipotence has diminished. But he still believes that he can control his civilization.

The philosophy of science—of cause-and-effect relationships, of determinism—has been firmly established in the study of physical phenomena. It is well entrenched in the biological field also. Psychology may have demonstrated the operation of the principle of cause and effect, of determinism, in mental processes, and may have dispelled the notion of free will for the *individual*. But social science is still so immature as to permit one to find refuge in a collective free will. As Professor A. L. Kroeber has recently observed:

> I suspect that the resistance [to the thesis of cultural determinism] goes back to the common and deeply implanted assumption that our wills are free. As this assumption has had to yield ground elsewhere, it has taken refuge in the collective, social, and historical sphere. Since the chemists, physiologists, and psychologists have unlimbered their artillery, the personal freedom of the will is thankless terrain to maintain. Culture they have not yet attacked; so that becomes a refuge. Whatever the degree to which we have ceased to assert being free agents as individuals, in the social realm we can still claim to shape our destinies. The theologian is piping pretty small, but the social reformer very loud. We are renouncing the kingdom of heaven, but going to establish a near-millennium on earth. Our personal wills may be determined, but by collectivizing them we can still have social freedom.

Primitive man could believe that he could control the weather only because he was ignorant; he knew virtually nothing of meteorology. And today it is only our profound and comprehensive ignorance of the nature of culture that makes it possible for us to believe that we direct and control it. As man's knowledge and understanding grew in meteorology, his illusion of power and control was dissipated. And as our understanding of culture increases, our illusion of control will languish and disappear. As Durkheim once observed, "As far as social facts are concerned, we still have the mentality of primitives."

Needless to say, this is not the view taken by many today who look to science for our salvation. Far from expecting belief in our ability to control to diminish with the advance of social science, many people expect just the reverse. It has become the fashion these days to declare that if only our social sciences were as advanced as the physical sciences, then we could control our culture as we now control the physical forces of nature. The following quotation from a letter published in *Science* (**103**, 25–26) is a conservative statement of this point of view:

> For if, by employing the scientific method, men can come to understand and control the atom, there is reasonable likelihood that they can in the same way learn to understand and control human group behavior. . . . It is quite within reasonable probability that social science can provide these techniques [i.e., for "keeping the peace"] if it is given anything like the amount of support afforded to physical science in developing the atomic bomb.

The premise underlying this view is unsound. It assumes that wars are caused, or at least made possible, by ignorance and the lack of social control that goes with ignorance. It assumes that, given understanding through generous grants of funds to social scientists, wars could be prevented—the "peace could be kept." The lack of understanding and realism displayed here is pathetic. The instinct of self-preservation of a society that subsidized atom-bomb inventors rather than social scientists holding views such as these is a sure one. Wars are not caused by ignorance, nor can "the peace be kept" by the findings of social scientists. Wars are struggles between social organisms —called *nations*—for survival; struggles for the possession and use of the resources of the earth; for fertile fields; coal, oil, and iron deposits; for uranium mines; for seaports and waterways; for markets and trade routes; for military bases. No amount of understanding will alter or remove the basis of this struggle, any more than an understanding of the ocean's tides will diminish or terminate their flow.

But the fallacy of assuming that we can increase and perfect our control over civilization through social science is even more egregious than we have indicated. To call upon science, the essence of which is acceptance of the principles of cause and effect and determinism, to support a philosophy of free will is fairly close to the height of absurdity. Verily, Science has become the modern magic! The belief that man could work his will upon nature and man alike *if only he had the right formulas* once flourished in primitive society as magic. It is still with us today, but we now call it science.

No amount of development of the social sciences would increase or perfect man's control over civilization by one iota. In the man-culture system, man is the dependent, culture the independent, variable. What man thinks, feels, and does is determined by his culture. And culture behaves in accordance with its own laws. A mature development of social science would only make this fact clear.

The philosophy of free will and omnipotence is rampant in the field of education. "Educators," high-school principals, commencement orators, and others never seem to tire of telling the world that its salvation lies in education. Naturally, one does not expect meat packers to preach vegetarianism, and it is reasonable for educators to advocate schooling and to urge the support of schools and colleges. But one could wish that they would place their propaganda on a somewhat higher intellectual plane; after all, one expects more of educators than of vendors of soap and cigarettes. The fact is, however, that the appeals and claims of educators are seldom more realistic than soap and cigarette ads. Education will no more cure the ills of society than cigarette X will soothe the nerves, or soap Y beautify the hands, of the user. As a matter of fact, the premise upon which the educator's claim rests is, if anything, less sound than the premise of the advertisements, for these are at least relevant, whether sound or unsound, whereas the educator's premise is hardly even relevant. The soap or cigarette will certainly have *some* effect upon the user. But education does not, strictly speaking, have an effect upon culture at all; *it is a part of it.* Education, whether formalized in the classroom or informal in the course of everyday living, *is one of the processes of culture:* it is the means of transmitting culture from one individual, one group, or one generation, to another. Being a process *within* the system that is culture, it is therefore fallacious to think of it as acting upon culture from the outside. It is not people who control their culture through education; it is rather the other way around: *education is what culture is doing to people,* namely, equipping them with ideas, beliefs, ideals, codes of conduct, tools, utensils, machines, etc.—in short, determining how they shall think, feel, and behave.

The position taken here will of course be vigorously denied and opposed. People do not give up their illusions easily. As the distinguished anthropologist, A. L. Kroeber, has put it:

> Our minds instinctively resist the first shock of the recognition of a thing [cultural determinism] so intimately woven into us and yet so far above and so uncontrollable by our wills. We feel driven to deny its reality, to deny even the validity of dealing with it as an entity; just as men at large have long and bitterly resented admitting the existence of purely automatic forces and system in the realm that underlies and carries and makes possible the existence of our personalities: the realm of nature.

A common reaction—verbal reflex—to the theory of cultural determinism is to brand it "fatalistic" or "defeatist." The choice of these words is significant. Why is it that, when one employs the principle of cause and effect in the realm of physical and chemical phenomena, no one cries "fatalism," but the instant one applies it to human cultural phenomena this accusation leaps forth? Why is it that an admission of our inability to control the weather brings forth no charge of "defeatism," whereas this reproach is promptly leveled against anyone who recognizes man's inability to control the course of civilization?

The reason is fairly plain. "Fatalism" implies free will; "defeatism," omnipotence. When atoms, cells, or tissues behave in accordance with their nature and properties, no one calls it fatalistic because no one expects freedom of choice and action of them. But when one asserts that cultural phenomena have a nature of their own and consequently must behave in terms of their nature, the response is not an acceptance of the principle of cause and effect but a charge of "fatalism."

> To many educated minds [the great English anthropologist, E. B. Tylor, wrote many years ago], there seems something presumptuous and repulsive in the view that the history of mankind is part and parcel of the history of nature, that our thoughts, wills, and actions accord with laws as definite as those which govern the motion of waves, the combi-

> nation of acids and bases, and the growth of plants. . . . If law is anywhere it is everywhere.

We have combined "a scientific realism, based on mechanism," says Alfred North Whitehead, with "an unwavering belief in the world of men and of the higher animals as being composed of *self-determining organisms*" (emphasis ours). He feels that this "radical inconsistency" is responsible for "much that is half-hearted and wavering in our civilization. It . . . enfeebles . . . [thought] by reason of the inconsistency lurking in the background."

Implicit in the charge of "fatalism" and "defeatism" is the further notion of refutation. To brand a view "fatalistic" is, to many minds, to call it false as well. "Cultural determinism is fatalistic and therefore false," is about the way the reasoning would go if it were made explicit. "How can determinism exist?" is the question that is implied but unspoken. "Determinism is unthinkable." And so it is to one possessed by a philosophy of free will.† We find this point of view rather well expressed by Lawrence K. Frank in a recent article, "What is Social Order?" (*Amer. J. Soc.*, 49, 475):

> Perhaps the major obstacle we face today, therefore, is this essentially defeatist tradition expressed in the various conceptions of social order described earlier, as above and beyond all human control. . . . In this situation, therefore, we can and we must find the courage to view social order as that which must be achieved by man himself.

Of course man can "find the courage" to view social order as something "that must be achieved by himself." It does not take courage to do this, however; what is required is ignorance and hope. "Must find the courage," "must be achieved by man himself," is hardly the language of science. It is, rather, exhortation and rhetoric—of a type with which we have long been familiar: "if we will but purpose in our hearts. . . ."

No doubt the first to question man's control over the weather, the first to claim that the winds will blow, the rain and snow fall, the seasons come and go, in accordance with their own nature rather than in obedience to man's wish and will expressed in spell and ritual, were accused of "fatalism" and "defeatism," if, indeed, they were not dealt with more harshly. But, in time, we have come to accept our impotence in this regard and to become reconciled to it. If it be argued that man cannot control the weather because that is a part of the external world, whereas culture, being man-made, is subject to his control, it must be pointed out that the exact opposite is the case. It is precisely in the realm of the external world that man's control is possible. He can harness the energies of rivers, fuels, and atoms because he, as one of the forces of nature, lies *outside* their respective systems and can therefore act upon them. But man, as an animal organism, as a species, lies *within* the man-culture system, and there he is the dependent, not the independent, variable; his behavior is merely the function of his culture, not its determinant. Both theoretically and practically, therefore, it is possible for man to exert more control over the weather than over culture, for he can exert *some* control over the former even now (dispel fog on airfields) and he may increase this control in the future.‡ But he exerts no control whatever over his culture, and theoretically there is no possibility of his ever doing so.

THE usual reactions to this manifesto of cultural determinism are as unwarranted as are the assumptions of free will, from which, of course, these responses flow. After expostulating on the theme of "fatalism" and "defeatism," the conventional protest goes on to demand: "What is the use then of our efforts? Why should we try to do anything to improve our lot if we have no control over our culture? Why not just sit back and let the evolutionary process take care of everything? Of what use could a science of culture possibly be to us if control lies beyond our grasp? What good is an understanding of culture if there is nothing we can do about it?"

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† Note that we have said "possessed by," rather than "believes in." Philosophies possess, hold, animate, guide, and direct the articulate, protoplasmic mechanisms that are men. Whether a man—an average man, typical of his group—"believes in" Christ or Buddha, Genesis or Geology, Determinism or Free Will, is not a matter of his own choosing. His philosophy is merely the response of his neuro-sensory-muscular-glandular system to the streams of cultural stimuli impinging upon him from the outside. What is called "philosophizing" is merely the interaction of these cultural elements within his organism. His "choice" of philosophic beliefs is merely a neurological expression of the superior strength of some of these extrasomatic cultural forces.

‡ Since the above was written the press has carried reports of experiments in "rain making," in which dry ice is dropped into clouds from an airplane.

These questions are naive and betray a lack of understanding of what the cultural determinist—the culturologist—is trying to say. The determinist does not assert that man is irrelevant to the culture process. He knows full well that the contrary is the case; that man is an absolute prerequisite to it; that without man there could be no culture. He realizes very clearly the essential role that man plays in the system that is man-and-culture. What the culturologist contends is that in this system the human organism is not the determinant; that the behavior of the culture process cannot be explained in terms of this organism but only in terms of the culture itself; that the growth and changes among the Indo-European languages, for example, cannot be accounted for in terms of man's nerves, muscles, senses, or organs of speech; or in terms of his hopes, needs, fears, or imagination. Language must be explained in terms of language.

But to turn to some of the specific questions with which dissatisfaction with the philosophy of determinism is expressed: In the first place, we cannot "just sit back" and let the evolutionary process take care of all our problems. While we live we are confronted by our culture and we must come to terms with it. Even just sitting back, incubating a case of dementia praecox, is "doing something about it." So is committing suicide; as a matter of fact, suicide rates for various societies provide excellent indexes of cultural determinism. In some societies, the rate is high; in others, suicide is virtually nonexistent. This is not because suicide determinants are more abundant in the chromosomes of some populations than of others. It is due to the fact that the cultural determinants vary: hara-kiri is something that a culture does to an organism that, of its own nature, tends to persevere in that form of motion we call "life." It is obvious that we cannot avoid reacting to our culture.

To assume that the process of cultural evolution will take care of everything without effort on our part is of course absurd, and constitutes no part of the determinist's philosophy. Of course we must exert ourselves while we live, we cannot do otherwise. But the question is not "Who does the work, ourselves or cultural evolution?" It is obvious that the energy is expended by or through human beings. The question is, *What determines the nature, the form and content of this expression of energy in the culture process, the human organism or the extrasomatic culture?* The answer is of course fairly obvious—after a small amount of reflection. Let us consider two groups of human organisms, A and B. Group A raises taro, catches fish, carves wood, makes no pottery, speaks a Polynesian language, has chiefs but no currency, is nonliterate, drinks kava, is greatly concerned with genealogy, and so on. Group B mines coal and iron, speaks Welsh, imports its food from the outside, uses money, is literate, drinks ale, etc. Now the question is, Why does each group behave as it does? Is it that one group of organisms possesses traits or characteristics—genes, instincts, or psychological tendencies—that cause them to drink kava rather than ale? This is, of course, ridiculous; the one group of organisms is fundamentally like the other biologically. It is obvious that each group of organisms behaves as it does because each is reacting to a particular set of cultural stimuli. It is obvious also that a consideration of the human organism is totally irrelevant to the question. Why is one group stimulated by one set of stimuli rather than by another? This is a cultural historical question, not a biological or psychological one. So, one is not so silly as to say, "Why should we mine coal or catch fish? Let our culture do it." The question is not Who mines the coal? but What is the determinant of this behavior? And, the culturologist points out the obvious: the culture is the determinant.[§]

[§] A distinguished sociologist has trouble telling cart from horse in the man-culture equation: Professor Robert S. Lynd comes up with the verdict that culture does not "enamel its fingernails, or vote . . . but people do" (*Knowledge for What?* P. 27).

The reaction of many sincere, altruistic and conscientious people, upon being told that it is not they who control their culture and direct its course, is "Why then should we try to do good, to better our lot and that of mankind?" We have answered this question in part already. In the first place one cannot avoid trying to do something. As long as one accepts life and is willing to continue with it, he must exert himself. "Trying" is merely the name we give to the effort exerted in the process of living. To strive for this or that, therefore, is inseparable from our lives. But *what* one strives for and *how* his effort is expressed is determined by his culture. For example, the goal of one people may be eternal life in heaven, for which their terrestrial existence is but a preparation. The goal of another might be the good life "here below." One group may deny the reality of sickness; another may admit its existence and try to combat it. One group may use charms and incantations; another, clinics and laboratories. Whatever the goal and whatever the means employed to reach it are matters determined by the culture of the group.

But, it should be pointed out with emphasis, this is not a philosophy of defeatism or hopelessness by any means. Least of all does it declare that one's efforts do not count. The fact that one's efforts to stamp out tuberculosis are culturally determined in no way minimizes the effort or the result. A life saved is a life saved. A letter written to a congressman has an *effect,* too, no matter what kind or how much. A resolution on world affairs passed by a woman's club has a real function in society, although it may be a very different one from that imagined by the good ladies. The question we raise is not one of the value of effort or whether effort has consequences. Human effort is just as real as anything in the realm of the geologist. And effort is followed by consequences, just as effect follows cause in physics or geology. Living human beings cannot help but exert themselves, and everything they do counts for something in one way or another. Far from wishing to deny or ignore this, we wish to emphasize it. But this is not the question raised by the culturologist, the cultural determinist. What he claims is not that it is futile to try because what one does counts for nought, but that what one does, how he does it, and the end and purpose for which it is done is culturally determined, is determined by the culture of the group rather than by the free will of the individual or of the group. More than that, what a person or group desires is determined or at least defined by the culture, not by them. What constitutes the "good life" for any people is always culturally defined.

From the cultural determinist's point of view, human beings are merely the instruments through which cultures express themselves. A physician, saving lives each day, is an instrument through which certain cultural forces express themselves; if they were not there or if they were different, the organism in question would not be practicing medicine, or he would practice it in a different way. The gangster, evangelist, revolutionist, reformer, policeman, impoverished beggar, wealthy parasite, teacher, soldier, and shaman are likewise instruments of cultural action and expression; each is a type of primate organism grasped and wielded by a certain set of culture traits. It is only the inveterate habit of thinking anthropocentrically that makes this point of view seem strange or ridiculous.

But, granting that what we do counts even though it is culturally determined, of what use is it to develop a science of culture if we cannot control civilization or direct its course? We have a science of pathology in order to combat disease, sciences of physics and chemistry to control the external world. But if we do not control our culture and cannot ever hope to control it, of what use would a science of culture be? We might begin our reply to this question by asking, Of what value is it to know the temperature of a star a million light-years away? Questions such as these betray a limited understanding of science. Science is not primarily a matter of control in the sense of harnessing rivers with hydroelectric plants or constructing uranium piles. Science is a means of adjustment; control is but one aspect of adjustment. Man finds himself in a universe to which he must adjust if he is to continue to live in it.

Mythology and science are means of adjustment; they are interpretations of the world in terms of which man behaves. There is, of course, a vast difference in terms of adjustment between a philosophy that interprets stars as a flock of snowbirds lost in the sky and one that measures their masses, distances, dimensions, and temperatures. This difference is a very practical one, too, in terms of the contribution that each philosophy makes to the security of life.

Our ancestors once thought they could control the weather, as contemporary savages still do. They finally outgrew this illusion, even going so far as to outgrow calling the new view "fatalistic" and "defeatist." But we do not think a knowledge and an understanding of weather and climate useless. On the contrary, we are devoting more time and money to meteorology now than ever before. Here again we see the situation in terms of adjustment rather than *control.* We may not be able to control the weather, but adjust to it we must. And knowledge and understanding make for more effective and satisfying adjustments. It would be advantageous if we *could* control the weather. But if we cannot, then weather prediction is the next best thing. And for prediction we must have knowledge and understanding.

So it is with culture. We cannot control its course, but we can learn to predict it. As a matter of fact, we make predictions all the time, and many of them are quite accurate: wheat production, traffic fatalities, freight carloadings, births, exhaustion of oil reserves, and many other matters are already within the reach of limited but nevertheless valuable prediction. If our ability to predict were greatly increased by the development and maturation of a science of culture, the possibilities of a rational, effective, and humane adjustment between man and culture and between one cultural segment and another would be increased accordingly. If, for example, a science of culture could demonstrate that the trend of social evolution is toward larger political groupings, then the chances of making the futile attempt to restore or maintain the independence of small nations would be lessened. If the trend of cultural evolution is away from private property and free enterprise, why strive to perpetuate them? If it could be shown that international wars will continue as long as independent, sovereign nations exist, then certain delusions now popular would find less nourishment and support. The fact is that culture has been evolving as an unconscious, blind, bloody, brutal, tropismatic process so far. It has not yet reached the point where intelligence, self-consciousness, and understanding are very conspicuous. Our ignorance is still deep-rooted and widespread. We do not understand even some of the most elementary things—the prohibition of polygamy, for example. In short, we are so ignorant that we can still believe that it is we who make our culture and control its course.

This ignorance is not surprising, however. It has not been very long since we gave up burning witches, cudgeling hysterics to drive out demons, and other savage practices. Even in technology, which tends to outstrip the social and ideological sectors, we have surpassed the savage at two points—fire making and the use of the bow and arrow—only within the past century or two. Chemical matches are but a little more than a century old, and within the past hundred years the bow and arrow was used in bison hunting on the American plains in preference to the best firearms available at the time. It was only yesterday, culturologically speaking, that a small portion of mankind began to emerge from a condition of savagery. For most of his career thus far man has subsisted wholly upon wild foods; less than 2 percent of human history, as a matter of fact, has elapsed since the origin of agriculture. Other significant indexes: some 0.7 percent of culture history since the beginning of metallurgy, 0.35 percent since the first alphabet, 0.033 percent since Galileo, 0.009 percent since the publication of Darwin's *The Origin of Species,* and only 0.002 percent since William Jennings Bryan and the Scopes trial. A mature, urbane, and rational civilization is not to be achieved in a mere million years from the anthropoid level.

It should be made clear that, if an adequate understanding should come about as a consequence of a science of culture, it would not have been "us" who achieved it but our culture. In the interaction of elements in the culture process, those traits less effective in providing adequate adjustment in terms of understanding and control are gradually relinquished and replaced by more effective traits. Thus, bronze axes replace stone axes, ikons and spells give way to laboratories and clinics, and, finally, a science of human culture begins to challenge the primitive philosophy of omnipotence and free will. The new science will of course have to prove its superiority over the older view, just as astronomy, chemistry, and medicine have in other sectors of experience. The success of science—the philosophy of materialism, of cause and effect, of determinism—in the physical and biological sectors of experience encourages us greatly in the belief that this point of view and these techniques of interpretation will prove effective in the social sphere also.

Our role in this process is a modest one. Neither as groups nor as individuals do we have a choice of roles or of fates. Swedes are born into their culture just as Zulus, Tibetans, and Yankees are born into theirs. And each individual is thrust by birth into some particular place in the "magnetic field" of his culture, there to be molded by the particular organization of cultural influences that play upon him. Thus, he may have the belief that typhoid exists only in the mind, or is caused by witches or bacilli, thrust upon him—or "into his mind." He may be endowed with a belief in personal immortality, the efficacy of prayer, or the periodic law of Mendelyeev. He may be inspired to preach the only true faith to the heathen in distant lands, or to wear out his life in a genetics laboratory, or to believe that "only saps work." To be sure, the response of the human organism to cultural stimulation will vary with the quality of the organism. Some will be silk purses; others, sow's ears. The order in which an organism undergoes experiences is important too; the influence of events *a, b, c,* will not be the same as *a, c, b.* An experience will have one effect at fifteen; quite another at fifty. There is room, therefore, for almost infinite variety of permutation and combination in the experience of individual organisms.

Man discovers his place in the cosmos slowly and accepts it with extreme reluctance. Time was when his solid earth was planted in the center, the sun and stars spread upon the vault of heaven, and men and gods together acted out the drama of life and death. It was all so compact, so familiar, so secure. Then it was that man, like God, could cry, "Let there be light," and there was light. Like God, too, man was "omnipotent," if, however, to a lesser degree. With his magic formulas, his spells, prayers, charms, and rituals, mighty man could control the weather, the seasons, and even enlist the gods in the service of man. Now it is different. Man finds himself but one of innumerable animal species crawling about on an insignificant planetary speck, fighting, feeding, breeding, dying. Once the child of God, he now finds himself an ex-ape. But he has acquired a new faculty, one unknown among all other species: articulate speech. As a consequence of this, a new way of life has been developed: culture. But this culture, this mass of extrasomatic tools, institutions, and philosophies, has a life and laws of its own. Man is just beginning to understand this.

Man is wholly at the mercy of external forces, astronomic and geologic. As a matter of fact, it is rather disconcerting to think of how narrow is the margin within which man lives. Change the temperature, velocity, rainfall, or atmosphere of the earth but a little and life would cease. It is a curious and, from a cosmic viewpoint, momentary, concatenation of circumstances that has made life possible. Man did long rebel against his dependence upon these outside forces; to be wholly at their mercy was unendurable. As a matter of fact, man has employed his precious and unique gift of speech more to deny the facts of his existence than to improve upon them. But a certain portion of the human race has come at last to accept our dependence upon nature and to try to make the most of it.

And so it is with culture. Belief in our omnipotence has, as Durkheim says, always been a source of weakness to us. But we are now discovering the true nature of culture and we can in time reconcile ourselves to this extrasomatic order as we have to the astronomic, geologic, and meteorologic orders. To give up magic and mythology, which promised much but yielded nothing—nothing but the soothing comfort of illusion—was a painful experience. But to receive and accept a science and a technology that promises less but achieves a great deal is to reach a goal most men are loathe to lose. We may believe that knowledge and understanding of culture will make for a more satisfactory life, just as these traits have been of value in physics and biology. To be sure, understanding culture will not, as we have argued here, alter its course or change the "fate" that it has in store for us, any more than understanding the weather or the tides will change them. But as long as man remains an inquiring primate he will crave understanding. And a growing Science of Culture will provide him with it.

WELL OF DESOLATION

There is a deep abyss of darkest night
Where man is lost and utterly forsaken,
When groping for a truth beyond his sight,
Or forging blindly toward a hill untaken.
A vast and cruel loneliness of spirit,
For which no hope or comfort can be found;
A well of bitter desolation—near it
The echoes of derisive laughter sound.

Copernicus and Galileo knew it,
Before they swung the earth around the sun;
Beethoven brought his mighty anguish to it,
And Shakespeare paced its black oblivion.
No thinnest ray of light can enter there,
Yet all things great are born of its despair.

KATHARINE O'BRIEN

CULTURAL DISCONTINUITY AND THE SHADOW OF THE PAST

JULES HENRY

Professor Henry has recently left the Bureau of Labor Statistics, U. S. Department of Labor, to join the Department of Sociology and Anthropology at Washington University. Shortly after the end of World War II he went to Japan as chief of the Analysis Section, Morale Division, U. S. Strategic Bombing Survey. Dr. Henry helps to edit the American Journal of Orthopsychiatry.

> *. . . in many respects there is no question of riding roughshod over national habits of life which have a deep symbolic meaning. The question is irrelevant because old ways are gone.*[1]

THE persistence of some old, even of some ancient, institutions and attitudes is a fact of scientific experience. This persistence is often used to explain and even to justify the present; and it casts a shadow dark and long over the world of politics and international relations. The thesis of this paper is that in our culture there is an exaggerated tendency to explain present social behavior in terms of past institutions. It may be true in some cases that attitudes developed in connection with specific institutions may continue when the institutions have vanished, but I hope to show that the notion of persistence is frequently overworked and often lacks support in the realities of the social process. Hence, it becomes a block to sociological insight and should be supplanted by a useful hypothesis.

From the standpoint of cultural continuity, there are two possible ways of viewing social forms: One, the present method of the social sciences, is to view each form as evolving out of a preceding form or complex of forms and always retaining its connection with the past. This may be called the hypothesis of continuous historical development and transformation. Another way of viewing social forms is to consider them as capable of drastic, and sometimes even swift, transformation, of such character that the tie with the past is largely lost. This may be called the hypothesis of discontinuous social change. While at first blush the idea of discontinuity may seem antihistorical, I think that it can be shown that as a practical heuristic device, at least, the hypothesis has some value. It is useful, I believe, especially where changes of great magnitude occur. Even in a period of rapid and far-reaching change, of course, the new forms are imposed upon, or developed out of, the old. But in such situations the strength of the new forms may become so great as to generate a new system of social institutions and attitudes. Then, as change succeeds change in accordance with the internal laws of the new social form, the old ones give way before it until an entirely new social situation, having little or even no relevant connection with the past, has supplanted the old. The scientist will then do well to seek the explanation of contemporary events first in the nature of the new form; and only when he has canvassed the immediate sociological terrain will he make an excursion into the past. For the continuity has been broken. However the new forms may be traceable to the past, the change has overlaid, or absorbed and digested, the old culture. Such a change occurred in Europe, Great Britain, and the United States with the industrial revolution, and in Japan with the introduction of industrial capitalism. Within the present generation we may yet witness another one with the development of atomic energy.

The process of seeking the explanation of contemporary forms first in contemporary circumstances seems to me to be just good scientific method. On the other hand, there is a powerful psychological undertow in our culture toward the ocean of the past. It was, for example, a problem on the United States Strategic Bombing Survey in interpreting Japanese behavior. For there was a constant, and in my opinion, an exaggerated tendency among us of the social science personnel to see in the behavior of the contemporary Japanese "survivals" or "remnants" of the "feudal pattern."

Spurious continuity. Although it seems that as old institutions disappear the attitudes associated with them tend to vanish gradually also, situations may arise in which there is an apparent congruence between a contemporary idea, rooted in a new situation, and an ancient one, based upon an ancient situation. In such a case there may be a tendency to refer the contemporary idea to the old situation, even though the contemporary idea may be based upon an entirely new set of circumstances. Thus, Lapiere says: ". . . in America . . . the old feudal class system has persisted only in the most fragmentary form. Of those fragments the most important is the feudal idea of the divine right of the aristocracy to rule . . . which . . . now appears as the biological interpretation of class differences."[2] Here Lapiere, while acknowledging that most of the old feudal-system-related patterns of behavior and attitude have fallen away, still thinks that in some way the "idea of the divine right of the aristocracy to rule" has persisted in our thinking. However, even were we to admit, merely for argument's sake, that Americans interpret class differences in biological terms, it is still possible to assume that this contemporary notion is derived not from feudal times but from modern theories of biological inheritance, and that what has occurred is not the wedding of biological notions with feudal "survivals," but the marriage of a poorly understood biological theory with our modern class structure. If we wished, then, to pursue this matter further, the problem would be to discover how this union of the modern American class system and the biological theories came about.

In this case, the American class system and an alleged belief in the "biological inheritance of class differences" appear to coincide with "feudal" attitudes. But there is a certain cultural support for this attitude in the new situation; and this support is precisely in our class system. However, the class system is contemporary and bears no relation to the ancient one.

In this connection the following from one of America's unsung early historians is pertinent:

> In America there is but little pride taken in genealogy, and it is a rare occurrence to meet a family who can trace their ancestral lineage farther back than two or three generations. Business is the great absorbing interest of all classes of society, and keeps them intent upon the present and the future; what is past cannot materially affect their business, and the indulgence of family reminiscences would only occupy their mind to the exclusion of other thoughts more available perhaps in a financial view. This is the substance of their reasoning, and hence the ignorance displayed by most families in ancestral knowledge.[3]

Odum and Moore's remarks on this score are also significant: ". . . in the Middle States frontier men and women were expected to stand on their own achievements rather than those of their ancestors."[4] And a final touch is given to this picture of the development of social differentiation in early America by Ernest Groves:

> It would be misinterpretation to insist that the frontiersmen maintained social equality. It is rather that they refused to accept artificial or vicarious distinctions . . . the basis for determining character traits rested upon personal behavior. The methods for giving or denying prestige were distinctly frontier measurements. The Westerners were willing to follow leadership, but only as it was supported by qualities they approved and admired.[5]

Thus, these data, and many others, suggest that whatever biological theories of class developed in America, they had no connection with Old World feudalism.

This peculiar circumstance in which a contemporary system resembles an old one, yet has a different base, was perceived by Warner in the structure of the present caste system in the American South. He says:

> But a new social system in Old City and its country side began to evolve out of the destruction of the old. . . . The new system continued to place all Negroes in inferior positions and all whites in superior positions. It gave certain of the family groups among the whites superior positions to all other whites, and certain family groups the most inferior and least desirable places within the white society.
>
> As the years have gone by, this new social system has become less like the old master-and-slave system out of which it was created.[6]

We may now say that when contemporary social structures, relationships, or systems bear a resemblance to old ones they may foster ideas similar to old ones. The tendency in such cases will then be to mentally leap over the contemporary situation and hark back to the old one. This is a cultural trait of ours. It is as surely a folk idea as the insistence in our culture that children be properly trained in decent toilet habits. Thus, when the desire to hark back to the past overcomes disciplined scientific analysis we have spurious continuity—an intellectual construct—as distinguished from true continuity, which is a social reality.

JAPANESE LABOR: A TEST CASE

The transformations that have taken place in the relationships between labor and management in Japan since the introduction of industrial capitalism provide an excellent and pertinent context in which to examine the problem of discontinuity for the following reasons:

1. The structure and processes of industrial capitalism as a social form can be defined with reasonable clarity.
2. We can observe the same structures and processes operating in cultures other than the Japanese, and so can be reasonably certain that the structure as a whole has certain constant characteristics, even though in specific circumstances labor-management relations may be and are affected by the local "cultural pattern."
3. Rapid changes are taking place in Japan at the present time in labor-management relations.
4. In contemporary social science thinking there is an exaggerated tendency, often based on very little evidence, to refer present Japanese behavior to feudal times.
5. We have excellent documentation for the period in which Japan was transformed from an agrarian to an industrial economy.

Industrial capitalism everywhere is characterized by the following:

1. Urbanization.
2. Increase in number of factories.
3. Increase in industrial employment.
4. Increase in mechanization.

These conditions were as characteristic of Japan as they were of other countries into which the system of industrial capitalism was introduced. I will attempt to show that the system has had far-reaching consequences on Japanese social organization; that the consequences were to create a true discontinuity in Japanese social organization, and that consequently labor-management relations in Japan in the prewar and the postwar periods are to be interpreted largely in terms of the industrial-capitalist system of labor-management relations rather than in terms of the old feudal patterns.

In urging this interpretation I am, of course, not unmindful that most of the argument has been on the opposite side. Thus, in speaking of the relationship of the "feudal clan" to Japanese industry, the United States State Department says: "A similar paternalistic attitude dominated the relations between the larger employer and his factory hands. A subsistence income in bad times as well as good was virtually assured to labor by employers."[7]

Possibly the best way to begin the discussion of the changes that have occurred in labor-management relations in the past fifty-odd years in Japan is to give some data illustrating what occurred in the culture as a whole (Table 1).

It will be seen from this table that Japan displays characteristic features of industrial-capitalist cultures: marked increase in spatial mobility and urbanization, decline of the rural population, increase of the number of industrial wage earners, increase in mechanization. By 1940, 20 percent of Japan's population lived in her six great cities.[10]

Although it is true that many Japanese workers were employed in small establishments, the number employed in large ones even in the 1930s is impressive. Teijiro Uyeda shows that in 1935 the overwhelming bulk of the workers in the cotton-spinning industry were employed in factories having more than 100 operatives. In 1932, 72.7 percent of the workers in the rubber-boot and -shoe industry were employed in establishments having a hundred or more workers. In the electric lamp industry in 1935 more than half the workers were in factories having more than 100 employees.[11] In important strategic industries like mining, railroads, automobiles, aircraft, and shipbuilding large pay rolls were, of course, common.

Japan thus quickly adopted, or became subject to, important overt features and internal processes of industrial capitalism. How close was this union of "old Japan" with the economic system of the West is indicated by the fact that as early as 1897 Japan was struck by periodic financial and industrial crises similar to those of the United States and at almost exactly the same time! Thus Japan's crises occurred in the following years: 1897, 1900, 1908, 1914, 1920, 1923, 1927 and 1929–30.[12] This series of crises paces our own practically step by step.[13]

TABLE 1

Some Economic and Demographic Changes in Japan 1898–1940*

Percentage of population living in places of 10,000 or less in 1898	82
Percentage of population living in places of 10,000 or less in 1940	50[8]
Number of towns of 10,000 or more inhabitants in 1898	153
Number of towns of 10,000 or more inhabitants in 1925	609
Increase in numbers of factories per year, 1868–98	100–300
Increase in number of factories in 1907	1,000
Increase in number of factories in 1917	2,000
Number of factory workers in 1909	800,000
Number of factory workers in 1931	2,032,353
Number of industrial workers in 1931	4,729,436
Number of industrial workers in 1940	10,094,000[9]
Percentage of factories using electric power in 1909	28.3
Percentage of factories using electric power in 1929	83.2

* Statistics from *Industrial Labour in Japan*. (Studies and Reports, Ser. A, Industrial Relations, No. 37). Geneva: Int. Labour Off. 1933.

It seems reasonable, now, to expect that this vast and sudden urbanization, this flocking of millions into the factories and sweatshops of Japan, could not but unhinge old relationships that were based on an economy overwhelmingly agrarian, largely farm-tenant, and oriented toward primary relationships. This hypothesis appears even more reasonable when it is borne in mind that the process of industrialization was accompanied not only by the adoption of Western mechanical techniques, but also by an influx of Western ideas.[14]

What, then, were the consequences in terms of human relationships, of this change of great magnitude? In accordance with the stated objectives of this section I shall attempt to illustrate what happened through an analysis of labor-management relations.

The social organization of industrial capitalism is characterized everywhere by the following:

1. There are two groups of persons: the employers and the employed.
2. The employers own the means of production, the employees are paid money to operate them.
3. The employer tries constantly to maximize the return on his investment and minimize costs.
4. The employees strive constantly to maintain what they consider a reasonable balance between their money earnings and the cost of living.
5. A system of laws comes into existence the purpose of which is to regulate the relations between management and labor.
6. These laws are enforced by police power.

The operation of these factors has, in capitalist cultures, whether through internal development or through diffusion, been accompanied by the following:

1. Conflict between labor, management, and government.
2. Development of labor unions.

Japan did not enter the capitalist structure of labor-management relations at a single bound. Rather, the new structure was erected on the battered foundations of the feudal order. The first large-scale factory in Japan, a cotton-spinning works with 5,000 spindles, was set up by the feudal lord of Satsuma in 1862. Other early factories were established by the feudal chief of Mayebashi and by Nabeshima, the old feudal lord of Saya, in 1870.[15] In the early days of industrialization, labor-management relations appear to have been colored by *kasoku seido* ("family system"), in accordance with which the employer, out of his *ninjo* ("humane feelings"), was presumed to look after the well-being of his employees. The rapid industrial expansion after the Sino-Japanese war, however, resulted in the development of a new class in Japan: the industrial wage earner. Meanwhile, the problems of competitive enterprise, on the one hand, and the rising cost of living in the cities, on the other, placed heavy burdens on worker earnings. At the same time, the recurrent industrial and financial crises resulted in widespread poverty and unemployment. These were situations of such magnitude that the old family system could no longer cope with them. In this connection the words of the International Labour Office are portentous:

> Capitalistic economy which has brought into existence a large class of wage earners . . . has led to the transformation of the old friendly relationship between the *oya-kata* and his *dei-shi* (at once pupil and worker) into an impersonal relationship between the employer and employed. . . .[16]

With the influx of new ideas from the West the stage was set for a change in the structure of human relations; the stage was set for industrial disputes. Between 1919 and 1930 the number of disputes involving strikes, lockouts, or slowdowns doubled. One of the most important strikes occurred precisely in a firm in which the family-system type of operation was most strongly exemplified: the Kanegafuchi Spinning Company, which employed 35,000 workers in thirty-six mills. This concern provided its workers with dormitories, educational and recreational facilities, health services, illness and invalidity allowances, and death benefits. Nevertheless, when the company attempted to cut the cost-of-living bonus, a strike ensued.

As in all young labor movements, the first strikes were poorly organized and not very successful. Nevertheless, the railway strike of 1898 was carried through under strict discipline and paralyzed the right of way of the Nippon Railway Company. After a period of police suppression of the labor movement beginning in 1900, a new series of strikes broke out during the first world war. In 1919 the slowdown was used in twenty-three strikes. The period 1920–30 was characterized by growing discipline in organizing strikes. Of the depression period of 1920, the International Labour Office states: ". . . any strike that broke out was serious, for it was part of organized warfare, with carefully planned tactics and a 'technique' of its own." Concluding its survey of ten years of Japanese strikes, the same source says: "This survey of the larger strikes of the last 10 years suggests the increasing tension of industrial relations in Japan."

Japanese labor organization operated constantly in the shadow of police power and restrictive legislation, culminating in its ruthless suppression in 1938. However, the report of the United States Strategic Bombing Survey points out that the Japanese were never able to eliminate entirely strikes or walkouts. They did, however, succeed in reducing the number from 628 cases involving 127,730 participants in 1937 to 216 cases with 6,627 participants in 1944.[17]

HAVING established the growing resemblance of the Japanese labor-management relations pattern to the labor-management relations pattern in other capitalist countries, we may now proceed to examine some recent interpretations of the relationship of "feudalism" in Japan to labor-management relations in that country. We shall examine the interpretations of those who, through failure to recognize the discontinuity that has developed in human relations in Japan, continue to evaluate the situation in terms of ancient institutions. I should like to emphasize in this connection that my disagreements with the interpretations I shall cite are not of a dogmatic character. In a changing situation it is difficult to assert with absolute assurance that "this is of the old" or that "this is of the new." What I should like to suggest, however, is the real importance of the alternate hypothesis for the understanding of contemporary Japan.

Example 1. "The wage structure [of Japan] reflects the feudal philosophy of wages, aiming not so much at compensation for the productive effort as at assuring the subsistence—on a very low level—of the employee and his family, and at rewarding him for his loyalty to the employer."[18]

Alternate hypothesis: The aim of employers in a capitalist economy is to reduce costs to a minimum. Low wages in capitalist countries have been one of the principal problems of the capitalist economy. Hence, the situation in Japan reflects the general-capitalistic rather than the specific-Japanese.

On the other hand, it can be no news to the technicians of the United States Bureau of Labor Statistics that estimates of worker income anywhere should include income other than wages. In this connection, the United States Strategic Bombing Survey states that in addition to wages, Japanese workers received extra money for efficiency, overtime, perfect attendance, and family obligations.[19] This form of supplementary payment is common the world over. In a certain sense, the United States is unique in the degree to which supplementary allowances are not an important part of worker income.

The article cited contains a number of unproved assumptions of this kind: Thus, in explaining the "absence of any provision for special procedures" in certain circumstances in Japanese labor agreements, the article says: "This omission might be explained by a traditional Japanese inclination to avoid formal decisions about right and wrong. . . ." The explanation of low wages paid to women is "the subordinate position which women traditionally have in Japan." Again, "The basic human rights of workers have never, because of Japan's feudal and paternalistic background, been properly recognized or protected." But absence from labor-management agreements of "special procedures" is characteristic of young labor movements; low wages have been woman's lot in many places in the world, and the "basic human rights" of workers were scarcely recognized even in the United States before the days of the N.R.A. On the other hand, they were recognized ten years earlier in Chile, which is still much closer to feudalism than we are!

Example 2. Occasional "voluntary offers" by Japanese workers to take pay cuts in times of depression are "no doubt a survival of the feudal Spirit in Japanese industry. . . ."[20]

Alternate hypothesis: In times of depression it is necessary for the employer to reduce costs. Hence, the acceptance by labor of wage cuts is a reflection of the general-economic rather than of the specific-Japanese.

The following quotation from the *New York Times* of August 10, 1947, is of particular interest. The article bears a Rutland, Vermont, dateline and under the head WORKERS TAKE CUT TO SAVE RAILROAD makes the following statement:

> The employees of the line have taken cuts before. In 1941 they accepted a 5 per cent deduction for two years as a "voluntary contribution of the employees for the purpose of improving the financial condition of the Rutland (Vt.) Railroad."

The statement in the International Labour Office report is particularly interesting because of the words "voluntary" and "offer." Conversation with officials of the American labor movement and of the United States Department of Labor suggests that although wage cuts were accepted by American workers during depression periods, the closest we get in this country to an "offer" by American labor to cut wages is the open negotiation of agreements. Closer examination of the Japanese situation, however, shows that even the occasional "offers" were not quite as deeply rooted in the soil of old Japan as they may at first blush appear to be. In the last sentence of a footnote referring to this question is the following: "In another case the workers at a certain knitting works in Nagoya made a proposal to accept a 10 per cent reduction, but with a view to forestalling the company's announcement of a cut of 20 per cent." Thus, what the situation appears to boil down to in Japan is the occasional special phrasing of the unpleasant situation.

Example 3. "Industry generally employs two or three times too many persons because of the ancient philosophy that the boss is responsible for the welfare of his workers."[21]

Alternate hypothesis: Postwar paralysis in enemy countries, uncertainty, unrest, material shortages, inflation, and physical weakness of labor from hunger make it inexpedient for management to aggravate the crisis by wholesale discharge of workers. At this point it is important to observe two facts: The practice of maintaining pay rolls far in excess of requirements is characteristic of postwar Europe, especially Germany;[22] the plan for laying off workers intensified unrest in Japan, according to the article cited, and is about to precipitate the "end" of the "truce." The article in the *Times* states that "the plan for dropping useless workers are [*sic!*] making the work of agitators far easier." Even without the parallel instances from European experience, it would be possible to argue from the Japanese situation alone that the appearance of unrest in the face of the proposed layoffs is in itself proof that the maintenance of excessive pay rolls was intended to prevent unrest.

It is important to bear in mind that government control over the labor force, which grew as the Asiatic venture moved toward ultimate catastrophe, hastened the depersonalization of employer-employee relationships, and the undermining of the old patterns.

WHO seeks for "old"—for "feudal"—Japan in the Nippon of today seeks a ghost among ashes. The old order, reflected so sensitively in the work of Lafcadio Hearn, was disappearing even before our iron birds made rubble of Japan's great cities. For Japan, having adopted the machinery of industrial capitalism, was being changed by it. What was taking place in Japan was not so much the transformation of capitalism by the Japanese "cultural pattern," as the anthropologists love to put it, but rather the transformation of Japan by a new cultural entity introduced from across the sea. And this, it seems to me, is the situation today: it is not so much that old, local "culture patterns" are determining what changes shall take place in the world today, but rather that irresistible forces are pushing the world toward a goal that shall be very much the same for many of us regardless of our traditions. Wherever we go, wherever we shall arrive, will be a place of no return. For the old ways will be gone, and the changes that occur will bring about a new alignment of human relationships. This alignment will be for us a discontinuity—a break in our "cultural tradition"—that will force us to adopt new ways of thinking about ourselves and the world if we wish to survive in all sanity.

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SCIENCE ON THE MARCH

CENTRIFUGAL FIELDS

THE generalization of Newton that every uncharged macroscopic piece of matter attracts every other uncharged macroscopic piece of matter in the universe is perhaps one of the most widely applicable laws of physics. Newton further showed that the amount of this attraction F between any two homogeneous spherical masses M_1 and M_2 a distance R apart is given by the simple relation

$$F = G \frac{M_1 M_2}{R^2}$$

where G is a universal constant. It will be observed from this relation of Newton that the gravitational force of the earth on any body near its surface is proportional to the mass of the body and to the mass of the earth. Since the force on a body is equal to the mass times the acceleration, every freely falling body near a given point on the earth's surfaces falls toward the earth with the same acceleration. This acceleration is called the acceleration of gravity g, and, although it varies slightly over the earth's surface, g is about 981 cm./sec^2. From various observations, astronomers have determined the gravitational acceleration on the surfaces of a number of astronomical bodies. For example, on the moon the gravitational acceleration is about one-sixth g, and that on the sun is almost 28 g. The most intense gravitational field so far found by the astronomers is 80,000 g on the surface of the star van Maanen. In other words, a mass weighing 100 pounds on the earth would weigh 4,000 tons on van Maanen if the respective weighings were carried out on spring balances. So far, man has not been able to change appreciably the gravitational acceleration of the earth or any of the astronomical bodies. He has, however, found ways of exerting forces and accelerations that are equivalent to gravitational accelerations and forces in the way matter is affected. For example, such forces are experienced whenever a body is stopped or started or when it has its direction of motion changed. The forces resulting from the latter are, in practice, more akin to gravitational forces than any of the others, since they can be maintained constant for comparatively long periods of time.

If a mass M is whirled around an axis with an angular speed of N revolutions per second, the centrifugal force $F = 4\pi^2 N^2 Mr$ where r is the distance of M from the axis of rotation. If one takes a value of $r = 1$ meter, then the value of N required to give an acceleration equal to that of gravity g on the earth is only 0.5 rev./sec. Consequently, we see that it is possible to produce centrifugal forces up to values many times that of gravity; also, that they may be maintained over comparatively large regions or volumes. Perhaps the most valuable use so far found for centrifugal forces is the separation and purification of various materials. It is common knowledge that, if particles of various sizes and densities are allowed to settle out of a fluid, the larger, heavier particles settle out first, followed in turn by the smaller or less dense ones. Just over a century ago, Sir George Stokes showed that the velocity of fall or sedimentation of a spherical particle with a density d_p and radius a is given by the relation

$$v = \frac{2}{9n} (d_p - d_l)\, a^2 F$$

where d_l is the density of the liquid, n its coefficient of viscosity, and F the gravitational or centrifugal force. Consequently, the larger the value of the force F, the higher the settling speed. Upon examining the above equation, it might seem that from a practical standpoint all separation due to sedimentation could be carried out in the earth's field rather than in a centrifugal field, as the only advantage of the centrifugal field would be in the shorter time of settling, and this could be compensated for by using larger settling tanks. However, this turns out not to be the case. It will be observed from the equation that the speed of settling decreases very rapidly as the size of the particle is decreased, and becomes extremely small when the particle approaches molecular dimensions. It is well known that all particles experience an ever-present Brownian motion, the amplitude of which increases as the size of the particle decreases and becomes quite large when the particle approaches molecular dimensions. This Brownian motion, or diffusion, always transports particles from regions of higher concentration to regions of lower concentration and hence opposes or stops sedimentation unless the centrifugal or gravitational force is large enough to overcome it. Consequently, in a great many important applications, the larger centrifugal forces, instead of gravity, must be used to produce any appreciable sedimentation, and as a result centrifuges or apparatus for producing centrifugal fields are common tools both in industry and in the laboratory.

As shown above, when the particle size reaches molecular dimensions, the centrifugal field must be made very large to produce sedimentation. However, with modern technique, sufficiently large centrifugal fields can be obtained to produce molecular sedimentation and, in fact, to separate the isotopes of the elements. Fortunately, most of the biologically important substances, such as viruses and hormones, are not deactivated by centrifugal fields large enough to concentrate them, so that the centrifuge method is a most effective way of purifying them. Since the velocity of sedimentation in a centrifugal field depends upon the size and mass of the particles being sedimented, the centrifuge may be used for determining both the mass and size of the particles.

If the particles are molecules, their mass and size also can be determined, and it turns out that the ultracentrifuge method, first developed by Svedberg and his colleagues about twenty-five years ago, is perhaps the most reliable way so far devised for measuring the molecular weights of proteins and many other types of high molecular weight compounds. Also, if the particles are not of uniform size, the distribution of their masses and sizes can be obtained by centrifuging. It is interesting to note that the general centrifuge method has been used for obtaining molecular weights over the entire known molecular weight range, i.e., from hydrogen, the lightest known molecule, to certain biological compounds with molecular weights in excess of twenty million molecular weight units.

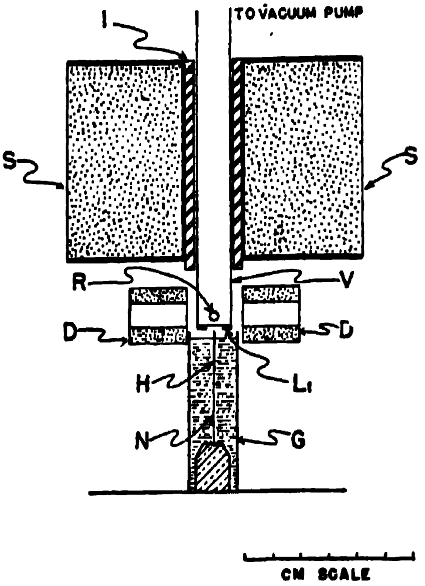

FIG. 1. ULTRACENTRIFUGE

SCHEMATIC DIAGRAM OF APPARATUS FOR PRODUCING ULTRAHIGH CENTRIFUGAL FIELDS. THE FUNCTION OF THE PARTS LETTERED IS GIVEN IN THE TEXT BELOW.

In most of the experiments, where molecular sedimentation is obtained, the centrifugal field is in the range from 10^4 to 10^6 gravity which, except near the surface of a few distant stars, is much larger than any of the gravitational fields found so far in nature. Also, H. W. Beams, the Harveys, and others have found this same range of centrifugal fields most effective in their studies of the displacement of the components in biological cells. However, it has been found that the above maximum centrifugal fields of a million times gravity are far too small for effective use in a number of different experiments, especially in physics. Consequently, a search for new ways of producing higher centrifugal fields has been undertaken at the University of Virginia, with the result that much higher centrifugal fields are available. For example, centrifugal fields of the order of half a billion gravity are now possible for laboratory use.

The technique of producing these high fields, although comparatively simple, will not be outlined in detail here, as it has been adequately described elsewhere. It might be of interest, however, to indicate briefly the general principles of the method. Figure 1 shows a schematic diagram of the apparatus. The rotor *R* is made of steel or other ferromagnetic material and is supported by the axial magnetic field of the solenoid *S*. It is well known that such a suspended rotor is stable as far as its lateral motion is concerned, but is not stable vertically if the current through the solenoid is steady. In order to give the rotor vertical stability, a small pickup coil *L* placed either below or above the rotor is made to regulate the electrical current through the solenoid *S*; i.e., if the rotor *R* moves slightly upward, the current through *S* is decreased, whereas if it moves downward, the current through *S* is increased. Electrical damping also is introduced into the circuit so that, in practice, no vertical or horizontal oscillation of the rotor can be observed when it is viewed with a 30-power magnifier focused on scratches on the rotor surface. A small iron wire *H*, mounted in a glass tube filled with a liquid, assists in damping any horizontal motion of the rotor. The rotor is surrounded by a glass vacuum chamber *V*, which can be highly evacuated to reduce the gaseous friction. The rotor is spun by a rotating magnetic field produced by an alternating current through four coils *D* placed symmetrically around the rotor. The rotating magnetic field is used both to accelerate and decelerate the rotor. The speed of the rotor is measured by a photoelectric pickup arrangement.

It will be observed that the magnetic field that supports the rotor is symmetrical with respect to the axis of rotation. Consequently, there are no appreciable eddy currents induced in the rotor that can resist its rotation, so that the only forces which substantially oppose its rotation arise from gaseous friction on the rotor surface. Since the air pressure in *V* can be made very small, the frictional drag can be made correspondingly small. For example, with a 1.59-mm. spherical rotor spinning 100,000 r.p.s., the driving current in the coils *D* was turned off and the rotor allowed to coast for a few hours while the deceleration was measured. It was found that the loss in speed was only about 0.1 percent per hour when the gaseous pressure surrounding the rotor was about 2×10^{-6} mm. of Hg. This value is in accord with that calculated from the kinetic theory of gases and in fact suggests that such spinning rotor arrangements can be used as absolute pressure manometers for measuring low gaseous pressure.

Test experiments were carried out with a series of small hardened-steel spherical rotors to determine the rotational speed necessary to make them explode. Table 1 shows some of the results obtained. It will be observed that the maximum peripheral speeds obtained were roughly constant (10^5 cm./sec.) for all the rotors. This should be expected from theory if the steel were perfectly elastic. On the other hand, the maximum centrifugal acceleration obtained was much larger with decreasing size of rotor and became 428 million times gravity for the 0.521-mm. rotor.

TABLE 1

DIAMETER ROTOR	ROTOR SPEED	PERIPHERAL SPEED	CENTRIFUGAL ACCELERATION × GRAVITY	MAXIMUM CALCULATED STRESS
mm.	r.p.s.	cm./sec.		lb./in.^{2}
3.97	77,000	9.60×10^4	4.71×10^7	410,000
2.38	123,500	9.25×10^4	7.20×10^7	385,000
1.59	211,000	1.05×10^5	1.43×10^8	498,000
0.795	386,000	9.65×10^4	2.40×10^8	420,000
0.521	633,000	1.04×10^5	4.28×10^8	488,000

The very low frictional torque on the spinning rotor obtained in practice with the technique outlined makes it possible to obtain extremely constant rotor speed. In fact, it is found that the rotors may be driven in synchronism with a piezoelectric crystal. This type of drive maintains the speed as constant as the frequency of the crystal-controlled driving circuit and should be better than one part in ten million. When mirror faces were ground on the magnetically suspended rotor, no appreciable friction was introduced, so that a rotating mirror of extremely constant speed was obtained. This development makes possible many experiments other than those requiring ultrahigh centrifugal fields. For example, the precision with which the speed of light can be determined may be increased by a factor of roughly 100 if the light path could be measured with the same precision as the rotational speed of the mirror. Incidentally, experiments are already in progress at Virginia for increasing the precision of measurement of the light path in comparable degree with that attained in measuring the speed of the rotating mirror by measuring the light path in terms of the wave length of a given spectral line. The method makes use of an interferometer somewhat similar to that used by Michelson many years ago for measuring the length of the standard meter in terms of the wave length of light except that a photoelectron-multiplier cell and scaling circuit are used, instead of the human eye, for counting the fringes. The low frictional torque on the rotor also makes possible many other experiments such as the measurement of the pressure of light, etc.

From both theory and experiment (Table 1) it is evident that, as the diameter of the rotor is decreased, the maximum centrifugal force produced before rotor explosion increases. Also, with the technique of spinning the rotors discussed above, smaller rotors obviously may be spun than is indicated in Table 1. As a result, much higher centrifugal forces can be obtained. However, the successful study of the general effect of these high centrifugal fields on the properties of matters depends upon the development of methods of observing such changes in the fields. Consequently, considerable effort in the future must be directed to the study of these problems.

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University of Virginia

TENTATIVE ITINERARY*

FIRST ANNUAL INTERNATIONAL PHOTOGRAPHY-IN-SCIENCE SALON

The Buhl Planetarium and Institute of Popular Science Pittsburgh, Pennsylvania	January 10–24
Cranbrook Institute of Science Bloomfield Hills, Michigan	February 2–28
Cleveland Health Museum Cleveland, Ohio	March 8–29
Science Illustrated New York City	April 6–16
Brown University Providence, Rhode Island	April 22–May 6
Gamma Sigma Epsilon University of Florida Gainesville	May 13–June 1
The John Crerar Library Chicago, Illinois	June 8–June 21
Auburn Camera Club Bureau of Animal Industry, U.S.D.A. Auburn, Alabama	June 28–July 12
American Osteopathic Association Annual Convention Boston, Massachusetts	July 19–23

* Definite dates will be published later. Reservations for showing the 1948 Salon pictures may be made now.

BOOK REVIEWS

THE GRENFELL COUNTRY

Outlines of the Geography, Life and Customs of Newfoundland–Labrador. V. Tanner. 2 vols. 909 pp. Illus. $13.50. Cambridge Univ. Press, Cambridge, Eng. Macmillan. New York. 1947.

DR. TANNER, with a broad grasp of geology and geography, has prepared a most interesting report on his field studies in the southeastern part of Labrador adjoining Newfoundland, but separated from that island by the Strait of Belle Isle. Associated with Dr. Tanner during his expedition to Labrador in 1937 were Dr. E. H. Kranck, a geologist and geographer colleague at the University of Helsingfors, and Dr. I. Hustich, a botanist from Helsingfors. These three men were experienced fieldworkers and were particularly well acquainted with the geology and botany of the Fennoscandia. They were inspired to carry out as far as they could, comparative studies in the physical environment of two areas in about the same latitude but on opposite sides of the North Atlantic Ocean.

In geologic structure there are many similarities in the two areas. Both regions are composed primarily of ancient rocks that are part of the Pre-Cambrian Complex. Both have been severely glaciated. They abound in lakes, ponds, and marshes and have, because of glaciation, disarranged drainage systems. Both areas have extensive forests, but in Labrador the forests do not come down to the east coast, where most of the inhabitants live, nor do they extend to the northern part of the peninsula of Labrador. In climate, population-carrying capacity, in natural resources and cultural development, the two areas are distinctly dissimilar.

In the second expedition, made in 1939, there were six American students who, because of their field studies in geology and geography, served as assistants. The work of this expedition was suspended owing to World War II, and Dr. Tanner, although he has written a very full and excellent report, emphasizes that his work is incomplete and that we must view these volumes as a report of progress.

After treating in detail the discovery and mapping of this land, which he believes is the first land seen in the new world by Europeans, the author presents in Part I a somewhat detailed treatment of the geomorphology of this little-known part of the world.

Over the surface of a Pre-Cambrian Complex, which is part of the Canadian Shield, there is an old-age stream erosion surface, a peneplain, which was uplifted late in Tertiary time. This old erosion surface is represented today by many gently rolling or nearly flat uplands. The valleys, developed since the peneplain was uplifted, had reached a stage of late youth when the great ice sheets of the Quaternary period advanced over this country.

The recurring invasions of ice carried away the old soils and smoothed many of the bare rock surfaces. The irregular deposition of glacial debris blocked the drainage and caused the development of the innumerable lakes, ponds, and marshes that give character to the landscape.

The Torngak mountains are in the far northern part of the peninsula and are due to uplift and block-faulting of the peneplain. At the southern margin of Labrador, bordering the St. Lawrence River, block faulting and uplift are recorded. The coast shows evidence of depression and drowning. There are numerous estuaries, and many of them have the form of small fjords. It is a coastline dotted with many islands, which can be accounted for by the sinking of the land relative to sea level.

Dr. Tanner believes that the submarine valleys off this coast were made by streams when the land was high. He does not enter into a detailed analysis of the various theories that have been advanced for the submarine canyons in the continental shelves of the world. He concurs with many other observers that the marine terraces and beaches along the Labrador coast are due to climatic changes brought on by the melting of the great ice sheets and the consequent removal of weight upon that part of the continent. The upward movement of the land was, in Dr. Tanner's judgment, slow and broken by brief halts. The periods of cessation in the upward movement gave the waves an opportunity to cut marine terraces and to clear away the fine debris on the land surface along the beaches. Lines of boulders that could not be easily moved by the waves are left at the inner edges of many of these beaches and are interpreted by many observers as shore-line features.

Definite indications have been observed of at least four separate ice advances. Distinct sheets of moraine have been found separated by stratified sands and peaty beds in the Hudson Bay area. The members of the expedition made many excursions far into the heavily forested interior of Labrador, and detailed accounts of their findings are recorded.

From his studies in northwest Europe and in North America, Dr. Tanner now estimates that the evaporation of sea water to form the great continental ice sheets of the Pleistocene period may have lowered the ocean about 240 feet. The return of the water to the sea as the ice melted should have caused a corresponding rise in sea level. Others have estimated that these changes in the relative position of the sea and the land may have amounted to as much as 300 feet. Dr. Tanner finds a close analogy between the raised beaches of Fennoscandia and those of the Labrador coast. He came to the conclusion that uplift is still taking place in both areas and, therefore, that readjustments following the ice age are not yet completed.

In Part II, entitled The Sea and the Labrador Current, there is a very full and careful presentation of all the known data on the configuration of the bottom of the sea immediately east of Labrador and the temperatures of the waters in that area.

In Part III, dealing with meteorology and climate, the author has analyzed with great care the continental and oceanic influences, giving due consideration to the more or less permanent low-pressure area over the Atlantic Ocean south of Iceland and the high-pressure area in the far north of that ocean. The many changes in weather from day to day he accounts for by the cyclonic storms coming chiefly from the southwest.

In his skillful and well-informed comparative studies of climates he makes the following exceedingly clear statement:

> On the one side we have the great heat regulating centre of the North Atlantic, the Gulf Stream and its continuation, the warm North Atlantic Current on the European coast, and on the other, off the American coast, we have the cooling apparatus, the icy Baffin Land Current and its continuation, the Labrador Current. If the functions of these two systems were suspended, the isotherms would be transported a thousand kilometers southward on the Fennoscandian coast and as much northward on the Labradorean.

Part IV deals with plant life. The northern portion of the Labrador peninsula is a part of the arctic tundra zone. That is a treeless landscape with grasses, mosses, and shrubs. The northern limit of trees crosses Labrador in latitude 55° north, which means that the whole Torngak mountain area and near-by coast is in a tundra belt. South of that belt there is an area of small coniferous trees without noteworthy economic value. The zone of small trees and bushes extends along the entire eastern coast of Labrador to the Strait of Belle Isle. The southern portion of Newfoundland Labrador, except near the coast, is clothed with a high forest of coniferous trees of distinct economic value, but it includes many swampy lands and barren rock surfaces.

Part V is devoted to the animal life of Labrador. These expeditions did not have professional specialists in zoology, and no claims are made for any startling discoveries in this field. Some of the descriptive statements in the report are most impressive, however. The forest observers in Dr. Tanner's party and many others who have visited this region have been much impressed by the absence of animal life. In much of Labrador, one writer reports he "was surprised by the awful silence of the forests." Another states:

> Long and anxiously I looked around in every direction to see if I could distinguish any signs of animal life, but without success. No sound was audible except the wind. . . . Not a bird, or butterfly, or beetle appeared to inhabit this desolate wilderness; the silence of the lifeless surroundings depresses the spirit and saddens the heart.

In spite of these reports, the Hudson Bay Company, which deals primarily in furs, has had stations established in Labrador for at least 200 years.

Part VI, dealing with Human Life, occupies the entire space of Volume II. After a very thorough and scientific analysis of the physical and biologic setting in this bleak environment, this geographer and his associates have appropriately turned to the study of man. No geographer should attempt to interpret the ways of living in any part of this world without first making such an analysis of the environment. Geography is a study of great human dramas, and the topography, climate, animal, and vegetable life present are parts of the stage settings where the human dramas are enacted. The actors are interesting and important in themselves, but their adjustment to the environment provides the substance for the geographer's field of study. His interpretations and predictions should become important and significant if he has followed faithfully a scientific method of work. No science has yet been developed and proved to be of great human value that has not the power of predictability.

Evidently Dr. Tanner has a keen interest in human geography. With numerous maps and photographic illustrations he has brought before us the location of the villages, the distribution of the areas occupied chiefly by the Eskimos, the white people, and the Indians. The permanent habitations in this land are always at the seashore. No dwellings have been set up out of sight of the high-tide lines. More than half the total population of 4,716 (based upon the census of 1935) are found south of latitude 54°, which means that they are concentrated along the southeast coastal area bordering and immediately north of the Strait of Belle Isle. That is the region where 90 percent of the white people live, and it is the region where the Sir Wilfred Grenfell missions were established.

We must recognize that the Newfoundland Labrador land is a primeval wilderness with the exception of a few widely scattered camping grounds in the interior and the few inhabited spots on the coast. For seven or eight months of each year a belt of coastal ice encloses this deserted land, completely separating it from the rest of the world.

The three most important ethnic groups in the population are the Eskimos, Indians, and the white people. The Eskimos occupy their natural home on the bleak coastal tundra and the neighboring strips of woodlands. Some of them live on the islands or have placed their little homes on the shores of fjords.

The Indians make their homes in the forest lands of the south, where the labyrinths of lakes and the network of rivers provide them with a habitat to which they are adapted. The distribution of the Indian population and the division of these people into various bands are presented in a map, from which it is clear that they, for the most part, avoid the tundra area. The trails of migration of these Indian bands, as well as the trapping ground of the white people, are clearly portrayed on another map.

After summarizing the work of many others who have visited Labrador and described the white people, Dr. Tanner presents his observations and describes in a very clear way the conditions of living along that bleak northern coast. There are many personal references in his reports to those who have given their lives as missionaries and social workers. He is exceedingly enthusiastic regarding Dr. Grenfell's work and also that of the late Dr. Harry L. Paddon, who served as surgeon, justice of the peace, and a "Great Gentleman of Labrador" during some thirty years as a local leader in The International Grenfell Association.

At the close of the book a little space is devoted to the prospecting for mineral deposits in Labrador. There are deposits of magnetite, layers of hematite, and some siderite. The Labrador Mining and Exploring Company is still carrying on field studies; the ore, known to exist, is claimed to be sufficient in amount to provide, at some time in the future, "a great local iron production."

At the close of Volume II there is a very complete bibliography of Labrador.

In a most modest and humble way the author and leader of these two exploring expeditions claims that he has made but a beginning in a work of scientific exploration. He wants his report to serve others who may carry further the studies he has begun. He is far too modest, for he has presented in many ways a model study in modern geographical science and embodied a vast amount of significant data. He has presented this in a most readable, and at places brilliant, literary style.

WALLACE W. ATWOOD

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LIFE MORE ABUNDANT

Science in Farming. The Yearbook of Agriculture, 1943–1947. Alfred Stefferud, Ed. xiv + 944 pp. Illus. $2.00. Gov. Print. Off. Washington. 1947.

THIS volume contains 135 articles written by the scientific staffs of the sixteen major bureaus and other units of the U. S. Department of Agriculture, presenting the most important results of research in farm problems investigated during the years 1943–47. Obviously, the sheer size of the book, with its wide variety of topics, precludes the possibility of listing even the titles of all the articles within the space of this review. A cursory glance at the table of contents gives one a fresh realization, if such were necessary, of the almost limitless range of scientific questions with which these experts of the U.S.D.A. constantly busy themselves.

Recognizing, as they do, that the continuation of human life and civilization hangs upon the production of adequate food and clothing, methods of increasing the output per animal or plant unit or per acre received perhaps the most devoted attention, with results of vital concern not only to farmers but the whole consuming public. The signs of ominously increasing pressure of population growth upon the limited area of productive land on the globe lend primary importance to all conceivable means of progress along those lines.

Naturally, therefore, the book is studded with details of practicable suggestions for increasing the volume and quality of beef, milk, pork, poultry, and eggs by improved methods of breeding, feeding, and housing, together with new discoveries in the care and sanitation of farm animals and control of disease among all food-producing animals, including goats and sheep. Some of these diseases are easily transmitted to man. Their control is hence as vital to the urbanite as to the farmer.

Likewise, new bits of advance are reported in methods of increasing the flavor, nutritive value, and vitamin content of a large number of food plants, of improving their appearance and market qualities. Even the everlasting struggle with the insect pests and fungus and bacterial diseases of garden and field crops is lightened by the discovery of more efficient ways of fighting these enemies.

Neither is the matter of newly introduced crops overlooked, nor the expanding use of farm crops for industrial purposes neglected. The basic need of better methods of soil conservation is set forth in sixteen articles, illuminating that field with many fresh suggestions. New plans of tillage and new farm machinery come into the discussion. The farm home, the preservation of food, care of clothing, and the future outlook for agriculture in the life of the nation are presented with helpful hints.

Finally, the interested reader will be glad to know that an elaborate index is provided whereby he may find his way among the wealth of details and topics.

E. V. WILCOX

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CONCHOLOGY

A Field Guide to the Shells of our Atlantic Coast. P. A. Morris. xvii + 190 pp. Illus. $3.50. Houghton Mifflin. Boston. 1947.

THIS very excellent little Field Guide should find a ready welcome among the shell collectors of the eastern coast. More than four hundred exceptionally clear photographs on a black background permit rapid and certain identification. Eight color plates are included which, in general, are very good.

The descriptions of the various species are short and concise, but not stereotyped. Mr. Morris includes in each description a sentence or two concerning the habits and distribution of the Mollusca. There is a total lack of synonymy which, for the layman, is perfect. At the back of the handbook there is a short glossary—always a help to the beginner, and a ready source of reference to the more advanced conchologist.

The discussions in the introduction, on the various classes, might have been a bit longer and more fully developed, for many users of this book will undoubtedly be amateurs who are not grounded in the fundamentals of conchology. A few minor errors on the distribution of some species have crept in, and the credit line of the Classification Chart should read "Michigan" instead of "Illinois."

Not the least in the book's favor is its size, for it fits nicely into one's pocket, as field guides should, for on-the-spot identifications. Its good illustrations and lucid descriptions fill a need long felt by all those interested in Atlantic Coastal shells.

CHARLES T. BERRY

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MICROCOSM

One Day at Teton Marsh. Sally Carrighar. vii + 239 pp. Illus. $3.50. Knopf. New York. 1947.

IT WAS the day of the autumnal equinox—and what a day! The author has dramatized the biology of the animal population in a skillful and delightfully literary manner. The marsh is a world in itself, and she sees it through the eyes of the otter, the mink, the scud, the snail, the mosquito, and a host of others, all leading bothered lives, each trying to eat somebody else and trying to escape being eaten.

Without making them human, Miss Carrighar has personalized her subjects. The mink proves himself a wanton killer, the beaver a substantial citizen, the otter a playboy when not worrying. One reads of the sensations of a frog about to hibernate, and of a trout who has just escaped an osprey's clutches.

Teton Marsh, near Jackson Hole, Wyoming, is the background for this study of animal life, and the marsh, as well as the pond, the brook, the river, the forest, and the very smell of approaching winter, are graphically described. It is the work of a keen and trained observer who can also write, and who understands the tragedies that occur in a community of wild things. The book is excellent natural history, presented from quite a different angle than most animal stories.

The black-and-white illustrations by George and Patritia Mattson are unusual and most attractive.

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HISTORICAL ETHNOLOGY

Chippewa Village: The Story of Katikitegon. W. Vernon Kinietz. 259 pp. Illus. $3.00. Cranbrook Inst. of Science, Bull. No. 25. Bloomfield Hills, Mich. 1947.

THE study of a Chippewa community is in the best tradition of historical ethnology and is quite the most ambitious of the Indian studies put out by Cranbrook. The author is already an established member of the small but distinctive segment of North American anthropologists who regard themselves as ethnohistorians, whose method, here exemplified, is beginning to show its effect on the writings of historians. But it is a sad commentary on academic life, nevertheless, that during the war such an able person could be induced to heed the greater returns of commerce. While still at Michigan, Kinietz combined two years of field work with the study of documents in such a way that his book on a living Indian community reflects how earlier observers—Cameron, Cass, Gilfillan, Grant, Schoolcraft, Tanner, and Trowbridge—helped formulate his questions, which he frequently answers by quoting these authors, and sometimes by the relations of living Indian informants, or else by observations in the first person. Oftentimes, the nice detail that appears at the documentary level is confirmed at the level of his own observation, or is remarked as absent, although Kinietz noted much that was overlooked by earlier students, or simply was not there. The chapter headings are the categories of conventional ethnography, and from them we signal out certain topics on which the treatment excels—tribal movements; economic changes (employment); social life (dances); life cycle (not birth, childhood, nor puberty, but drinking of adulthood); religion (especially contemporary spiritism); and medicine (the account of the *Midewiwin,* or "Medicine Lodge"). We have come to expect from Cranbrook Institute fine illustrations of their collections, as well as photographs from the field. The type is well designed, and the style should appeal to the general reader; for the scholar there is an adequate bibliography and index.

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GERIATRICS

The Years After Fifty. Wingate Johnson. xii + 153 pp. $2.00. Whittlesey House. New York. 1947.

Medical Aspects of Growing Old. A. T. Todd. 164 pp. $3.50. William Wood. Baltimore, Md. 1946.

AN INCREASING awareness both by the medical profession and the intelligent public of the urgency, magnitude, and significance of the health problems associated with senescence is to a great degree responsible for a small avalanche of new books purporting to explain senescence in nontechnical terms. Changes in population structure have made the years of later maturity more and more significant not only to the individual, but to society as a whole. The immense field of gerontology, or the science of aging, includes three major divisions: the biology of senescence, clinical medical geriatrics (concerned with the personal health of the aging and aged), and the sociologic and economic aspects of an aging population (SM, 62, 80, January 1946). These two most recent volumes are intended to help the intelligent man or woman anticipate the later years of life.

The Years After Fifty is delightful reading. The personal charm of Dr. Johnson permeates the text throughout; one listens to a cultured, mature physician discoursing frankly and sincerely on the personal problems of later maturity. His frequent citations from the classics are well chosen. The medical science is sound; the suggestions, worthy of more attention than most people will give to them. The commoner problems of chronic disease in later years are discussed clearly and briefly, perhaps too briefly. The only adverse comment justified is that the author, in a laudable and unusual effort to be brief, has committed the minor sin of omission of correlative and interpretative discussion. The book is essentially a manual of instruction and advice for individual senescents; it does not pretend to consider the biology of senescence or the broad sociologic problems. It is recommended reading.

Medical Aspects of Growing Old is of British origin; Dr. Todd is honorary physician to the Bristol Royal Infirmary. Unfortunately, he set himself a nearly impossible goal in trying to write for both a professional medical and a lay audience. The result is too simple to interest the trained professional and a bit dry for the layman. Unfortunately, also, some of the concepts presented are antiquated and long since discarded by the medical profession. The author appears unduly concerned over constipation: his therapeutic suggestions include ballet dancing for the senile! The volume is not scientifically sound, and thus cannot be recommended.

Edward J. Stieglitz, M.D.
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QUEST

The Naming of the Telescope. Edward Rosen. xvi + 110 pp. Illus. $2.50. Schuman. New York. 1947.

THIS fascinating small volume of seventy pages of text, fortified by twenty-five pages of documentation, presents Edward Rosen's analysis of the problem "Who originated the name *telescope?*" The book will be read at a single sitting, so alluring is made this quest for a word.

In following the evidence, the reader enters the colorful times when Galileo first used the "optic tube" to which he himself most often applied the term *perspicillum.* Chief witnesses are members of the famous Academy of the Lynxes, and include Frederick Cesi, founder and president of the Academy. Certain important clues resolve themselves into matters of interpretation. When Cesi writes "our telescope," is he using the "imperial we" or referring to his own person? Sidelights appear on timely controversial questions. Della Porta attacks Gilbert for his "mad idea that the Earth is in motion."

At a banquet for Galileo on April 14, 1611, Cesi repeats the name telescope many times, thus "unveiling" the new term. Who invented it, the Italian prominent in scientific circles, or the Greek, poet and theologian? The answer, on present evidence, is here.

Dr. Shapley provides an appreciative foreword to this admirable detective story in miniature.

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COSMIC CYCLES

Sunspots in Action. Harlan True Stetson. ix + 252 pp. Illus. $3.50. Ronald Press. New York. 1947.

In *Sunspots in Action,* Dr. Stetson tells in simple language about the things that have been learned and are being learned concerning the earthly effects of changes on the sun. Sunspots, which have been recorded accurately for more than a century, furnish the best index of solar activity. Their changes have been compared with almost every conceivable variable on earth, especially anything that varies in cycles.

It has been known for many decades that the earth's magnetic field is influenced by solar changes, but these effects, which did not touch our daily lives, seemed trivial and of only casual interest except to scientists. The same solar action that influences the earth's magnetic field also affects the ionosphere, varying the efficiency of radio transmission. With the increasing use of radio waves, this subject has become of personal importance to more and more of us. *Sunspots in Action* tells how and why radio reception is affected by sunspots and their related solar phenomena. Although the book deals primarily with the various effects of sunspots on radio reception, it also includes discussions concerning their possible effects on other earthly things that touch us closely—weather and economic cycles.

Popular interest in this subject is enhanced by the fact that the sun is now exceptionally active. The largest sunspots ever observed, and the greatest number of spots on the sun at one time, were recorded in 1947. This great solar activity, coupled with our increasing consciousness of the upper atmosphere through its relation to radio waves, makes Dr. Stetson's book a most timely contribution to this subject.

In keeping the language simple and concise, the author has not always been able to treat his subject with absolute scientific accuracy and completeness, but these failings are expected and excused in such a popular and simplified book. For readers who are more scientifically critical, the book contains a most valuable bibliography and a complete table of monthly relative sunspot numbers since 1749.

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LECTURES

Current Trends in Psychology. Wayne Dennis *et al.* vi + 225 pp. $3.50. Univ. of Pittsburgh. 1947.

In THIS little book are published eight lectures delivered at the University of Pittsburgh in March 1947. Seven specialties in the field of psychology are described, each by an expert engaged in the practice of the specialty he discusses. The eighth lecture, given by Dr. Dennis, is concerned with Psychology as a Profession and served as an introduction to the conference at which the lectures were presented.

The following specialties are discussed: Experimental Psychology (B. F. Skinner); Child Psychology (R. R. Sears); Clinical Psychology (E. L. Kelly); Psychotherapy (C. R. Rogers); Personnel Psychology (J. C. Flanagan); Human Engineering (C. T. Morgan); and the Sample Interview Survey (R. Likert). In most of the cases, the writers define their fields and give brief historical backgrounds for them, but the emphasis throughout is on current trends and anticipated future developments. The style is readable and the treatment full of insight and thought-provoking.

To the reviewer, reading the book was a stimulating and pleasurable experience. He does not agree with everything that is said in these discussions, but disagreements here are largely matters of opinion. As a description of how far psychology has advanced in the past decade and as an indication of the activity in and the breadth and scope of the field, this book can be highly recommended. To profit most from it, the reader should have some background of knowledge about psychology. But for anyone it will serve to give at least a partial conception of, and basis of definition for, a field that is widely misidentified with everything from crystal gazing to a concern solely for mental testing.

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University of Maryland

A NATURALIST'S WIFE IN COLOMBIA

East of the Andes and West of Nowhere. Nancy Bell Bates. 225 pp. Illus. $3.50. Scribner. New York. 1947.

HERE is a charming and interesting account of life with "The Boss" while he pursued his scientific studies for the Rockefeller Foundation in faraway places. "The Boss" in this case is the naturalist, Marston Bates, the husband of Nancy Bell Bates, the author of this book. Mrs. Bates, daughter of the renowned plant explorer David Fairchild and granddaughter of Alexander Graham Bell, fits naturally into the sort of life she has chosen, both through native inclination and because of her childhood experiences in company with her father on some of his expeditions.

Mrs. Bates sketches in an easy and interesting style and with a pleasant sense of humor her experiences with "The Boss," first in Albania and Egypt while he studied the malaria problem, and then in Colombia during his research in the field of yellow fever. Stopping on their way across the Andes at "the cold tropics: Bogotá" for the arrival of their first child, the Bateses descend the eastern slopes of the mountains, over roads made more precarious by unpredictable landslides, to reach their station at Villavicencio on the western margin of the Llanos. In these all-but-trackless grass plains, reaching eastward and touched here and there by the jungle into which they are finally absorbed, is the "Nowhere" of the book's title.

The lives of the Bateses blend harmoniously with the native surroundings, the customs and traditions that Mrs. Bates sympathetically portrays in her intimate manner. The "goings-on" in the lab are represented as a delightful mixture of human relationships and objective scientific procedures. Animal pets of the strangest sorts and the children's play pen are as much a part of the laboratory as of the Bates home, and many are the interesting incidents that arise from such circumstances.

As may be surmised, the Bateses have a truly catholic interest in the country about Villavicencio. Explorations into primitive communities and the native fiesta fascinate them as much as the strange butterflies and monkeys; the plants of the jungle interest them along with the mosquito larvae they collected from the rain puddles along the dim, dirt roads of the tropical woodland.

A number of splendid photographs of the Villavicencio region suffer somewhat from the obvious attempt to economize in their reproduction. One could also wish that these illustrations had been distributed throughout the book adjacent to the subject matter instead of being lumped together in three batches. A detailed map of Colombia, especially of the vicinity of Villavicencio, would have added considerable interest, although the book easily holds the attention of those readers who enjoy vicarious participation in such experiences as are described.

DONALD EDWARD MCHENRY
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COMMENTS AND CRITICISMS

ON COMMENTS AND CRITICISMS

My father, who in his time contributed occasionally to the old *Popular Science Monthly,* said somewhere in his writings that if human beings were fully rational animals, they would all draw the same conclusions from the same given evidence. I am increasingly impressed that contributors to "Comments and Criticisms" and to the main body of THE SCIENTIFIC MONTHLY are either examining different evidence or are drawing different conclusions from the same evidence.

It might not be irrelevant to add an anecdote about my father: It must have been at the time of the Scopes trial, when a reporter recognized him as he was waiting for a Mount Pleasant car. Just as the car came along the reporter hailed him and said, "Dr. Spillman, do you believe men are descended from monkeys?" "Not yet," answered my father, and swung aboard the car.

RAMSAY SPILLMAN, M.D.

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SCIENTISTS AND SOCIAL PROBLEMS

Are scientists who express opinions in relatively nonscientific fields—such as politics, sociology, religion, economics, or art—justified in ignoring the principles that they take for granted in scientific investigation? Or does the unique contribution that scientists can make to such problems lie in the scientific attitude—the objectivity, the willingness to consider all existing evidence and theory, the precise definition of meanings, and the scrupulous regard for logical consistency that we like to think are characteristic of the scientific approach? It should be superfluous to point out that scientists who merely use their professional eminence to gain a hearing for their unconsidered opinions and irrational prejudices do a service neither to science nor to society.

P. W. Bridgman had an article on "Scientists and Social Responsibility" in THE SCIENTIFIC MONTHLY for August 1947, and has set forth his views at greater length in a book called *The Intelligent Individual and Society.* His writings compare favorably with many similar articles being produced by scientists and engineers on such subjects: they are here singled out merely because Bridgman's standing as a scientist ensures their acceptance as representative of the best scientific thinking. One should certainly be justified in taking for granted that they will be well considered, logically consistent, and an advance over previous thinking on the subject. Is this the case?

"Scientists and Social Responsibility" begins by pointing out, irrefutably, that it is impractical to force a scientist to foresee the final results of his investigations, or to undertake the control of those results. It seems improbable, though, that any appreciable number of intelligent people ever assumed that this was possible, or had any such implications in mind when they spoke of social responsibility. To most of us, this implies simply an informed and active participation in the functioning of the society of which we are a part. It happens that the structure of our society is such that this does require the scientist to function to some extent as a politician, a sociologist, and an economist—but only to the extent that it requires this of any citizen who chooses to be influential in our society.

When Bridgman pictures the scientist as being bullied by society into taking up an intolerable burden, he appeals to our emotions rather than to our reason: the problem is not presented in that form. Power and responsibility go hand in hand, whether Bridgman likes it or not. The choice offered is simply whether the scientist, as he gains increasing control over the future of civilization, shall accept responsibility in proportion to his influence, or shall surrender both the responsibility and the power that goes with it to those who will be glad to use them for their own ends. The latter alternative will probably not be favorable to continued free inquiry and will certainly not improve the status of scientists as individuals.

So far as I can follow Bridgman's argument, he proposes to solve the problem by changing society to fit the scientist. This can hardly be considered a simple expedient: it certainly implies far more active involvement of the scientist in social dynamics than the solution he rejects. In any case, it seems to imply that the ideal society would be one in which no one had any responsibility except for his own specialty: certainly the argument that Bridgman advances for scientific irresponsibility could equally well be advanced by every other group in our society. His principle of "the general ideals of the specialization and division of labor, and the ideal of, as far as possible, each man to his best" leads logically to a state in which all politics, all economics, all religion, all philosophy, are controlled by self-appointed specialists in each field—since no group has any responsibility for any field but its own, and therefore no power in any field but its own. This is the concept that the human race has been fighting ever since the Middle Ages—it would wipe out the results of the American Revolution, the French Revolution, and every other struggle against the theories of divine right, hereditary aristocracy, and theocracy.

The argument is confused by the fact that Bridgman rejects any "metaphysical concept of society as some superthing, transcending the individuals who compose it. . . . Society is composed of you and me;

society does not have an individuality of its own, but is the aggregate of what concerns you and me." He then goes on to argue as though society had just such an individuality of its own: "Society can deal with the issues raised by scientific discoveries by other methods. . . . If it had not wanted to construct an atomic bomb, it need not have signed the check . . . which alone made it possible. . . . Let society deal with this situation by the means already in its hands. . . ." Who deals? Who signed the check? If society is composed of you and me, and you and I are irresponsible, who acts for society? Specialists like Hitler?

His concept of society as "composed of you and me" and nothing more seems more open to criticism, though, than any inconsistencies in his development of the argument. If anything can be said to be self-evident in the field of social theory, it would seem to be the fact that society is very much more than just "you and me"—that the whole is very much more than the mere sum of its parts, and that it differs significantly and qualitatively from such a sum.

When Bridgman talks of "you and me," it is obvious that he is not thinking of Bridgman the animal but of Bridgman the Professor, with all the education and background that he has received from society. But Harvard professorships and Nobel prizes and physical science were no part of Bridgman's germ plasm: they are social inventions, conceivable only in a large and complex and wealthy society. A society that really consisted of Bridgman and me, with none of the social traditions and values that he takes for granted, would be a repulsive thing. We should certainly not have a language or a number system. We should almost certainly not have fire or clothing or soap or weapons. We should spend damp, cold, miserable lives grubbing for enough roots and maggots to keep us alive, in a world without philosophy, religion, art, or science. If a female were in the neighborhood, we should probably spend our spare time throwing rocks at each other to impress her—or, more probably, Bridgman would be smart enough to think of throwing rocks, but I, less talented, should only bite him.

How many such creatures would it take to make a civilization?

Bridgman's refusal to see society as anything but a fortuitous juxtaposition of predators, most of whom have discreditable intentions toward Bridgman, probably accounts for his strange obsession with the idea that he is being exploited—that he must make a last-ditch stand against "the right of stupid people to exploit the bright ones." Just how such exploitation is possible, he does not adequately explain. Certainly, if he wishes to avoid contributing more than a minimum to society, he can always outwit us by taking a job as a janitor or day laborer—there is no way that a creative individual can be forced to create against his will. Strangely, though, what he seems to be opposing is any limitations on his activities as a scientist. If his scientific contributions are evidence of exploitation, this puts him in the strange position of defying his exploiters to deny him the privilege of being exploited.

Bright people have always been exploited by stupid ones, if exploitation is defined as contributing more than an average amount of creative work to a culture. It is improbable that even the greatest man has ever given society more than he received from it, however: the best of men can only make the world a very little better, but the worst society offers the individual the difference between being an ape and a human being. In any case, the greatest geniuses do not seem to have been embittered by the feeling that they were being exploited, nor felt their genius to be an unsurmountable barrier to leading a satisfying life. On the contrary, most of them seem to have found a very deep source of satisfaction in the very superabundance of their creativity. Is the individual exploited when society gives an individual the opportunity for the activity that is necessary to the complete expression of his personality? Does he profit when it refuses to accept his potential contribution?

The concept of life as a sort of perpetual feud between the individual and the human race seems to underlie much of the analysis in *The Intelligent Individual and Society* and to account for such conclusions as that on page 230: "This discussion must have made it particularly plain that the interests of society and the individual do not coincide by any means, although such has been obvious all along whenever we have scratched beneath the surface." Or that on page 281: ". . . I have got to recognize that in the conflict between the individual and society the only thing that makes sense for the individual is to get away with what fulfills his purpose. . . ." Bridgman is not very convincing, however, when it comes to pointing out just what antisocial "interests" the intelligent individual should promote, or how "getting away with" antisocial acts is the best way to ensure lasting gratification of the sort of tastes that an intelligent individual should develop. He seems baffled by the problem of the intelligent individual who finds that he gets more satisfaction out of living by burglary than out of any law-abiding career open to him. ("Would not the man be a fool if he did not take to burglary?" asks Bridgman.)

Strangely, it never seems to occur to Bridgman that his individual might, desirably, be educated as well as intelligent. If he were, he certainly would be aware of the fact that there may be serious consequences—for the individual as well as for society—in spending one's life in the consciousness that one is waging a sort of undeclared war on one's fellowmen. He might even have read Dostoevski—at least *Crime and Punishment*. He could very well be conscious of the fact that the great drawback to a life of crime is not possible retaliation by society, but the loss of almost every value that intelligent individuals have found worth living for. He would not be convinced that the juvenile thrills of playing cops and robbers were so lasting a source of satisfaction as to justify giving up, for their sake, all hope of honor or prestige, all sense of security or of achievement, all prospect of association with people whom he could respect or trust.

Bridgman's unawareness of alternatives to his proposals is partly explained by the Preface, in which he states that his reading of philosophical literature has been very limited. One can only wonder how he would react if one of his graduate students prefaced a thesis with the remark that it ignored all developments in the field of physics since Aristotle.

If Bridgman's nonscientific writings fall short of the scientific ideal, Du Noüy's are a headlong attack on that ideal. His recent book, *Human Destiny*, received a very sympathetic review in The Scientific Monthly for May 1947. It has had an enormous sale and almost certainly is now accepted by great numbers of Americans as an authoritative presentation of the latest scientific attitudes toward religion. As far as I am concerned, it is so bad as almost to defy serious criticism. It purports to be a scientific proof of the existence of God, but it is neither scientific nor a proof: the premises from which Du Noüy argues are questionable, his arguments illogical, and his conclusions largely meaningless.

As an example, Du Noüy sets out to prove that life could not have originated by chance. He takes as a model 1,000 black particles shaken with 1,000 white particles, and uses this model as the basis for considering the probability that a molecule with 2,000 constituent atoms and a degree of dissymmetry equal to 0.9 could originate by chance. He decides that the chance of such an occurrence is only one in 2.02×10^{321} and dismisses this as "impossible in the human sense." Now, molecules do not originate from atoms clumping together at random like damp cough drops: if this were the case, there could be no science of chemistry. Even if they did, it does not mean that one chance in 2.02×10^{321} is the same thing as impossibility: if it were, a poker game would become impossible after about fifty hands, since the probability that one will hold the precise sequence of five-card hands which one does hold is only one in $2{,}598{,}960^n$ where n is the number of hands. Finally, the argument would prove nothing of value even if it were valid: if God exists, the laws of probability are presumably as much a manifestation of his nature as is the existence of life. God is no greater and no less, no more understandable and no more inscrutable, if one assumes that he chose to originate life in one way or in another.

Similarly, Du Noüy's "proof" of the teleological basis of evolution amounts to little but the statement that there must be a purpose in nature intelligible to Du Noüy—because otherwise nature is incomprehensible to Du Noüy. This used to be called tautology.

Worst of all, Du Noüy has made a substantial contribution to misunderstanding by implying that he is defending God against a determined attack by "materialistic scientists" who are actively engaged in destroying "in the name of a science still in the cradle, the doctrines which up till now gave a meaning to individual life . . . namely, the religions." I know of no scientist who will not admit that there are relationships—and probably forces, particles, and unimagined sources of phenomena—of which scientists still know little or nothing. If Du Noüy wished to define God as that which was implied by this ignorance, he was at perfect liberty to do so. His argument seems to lead to a concept that is essentially the same thing—and one that is just as useless. But why must God be identified with ignorance, and understanding be regarded as atheism? Why does the believing mind find it intolerable to imagine that God should choose to work through what we call natural laws, instead of being utterly arbitrary and unpredictable? What denial of divinity is implied by the discovery that the fragment of time and space we have explored seems, to some extent, orderly and predictable?

If Du Noüy's reasoning is, as he claims, scientific, science might well be the menace he implies. Fortunately, this is not the case. Unfortunately, he has convinced many that it is.

Wm. Palmer Taylor

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THE MIMA MOUNDS

Victor B. Scheffer's article, "The Mystery of the Mima Mounds," which appeared in the October 1947 issue of The Scientific Monthly, raised some questions that went unanswered.

Since I had considerable experience with the pocket gopher in east Texas and in Louisiana, as well as in a number of other states, I shall attempt to answer some of the more important questions raised that went unanswered, and interpret differently one or two other factors the author attempted to explain.

It is customary for the pocket gopher to resort to mound building in areas where the soil is inadequately drained, where water is likely to stand on the surface of the ground for considerable periods of time under favorable conditions. The purpose of the mound in this case is to obtain adequate drainage for the nesting chamber. That also is why the rodent consistently pulls the dirt toward the nesting chamber, which is just beneath the surface at the highest elevation on the mound. This habit of continuously pulling the dirt toward the nesting chamber, incidentally, accounts for the mound itself, as generation after generation of the rodents continue to inhabit the same area.

Apparently, however, the motivating factor for mound building by the pocket gopher in the Puget Sound area is somewhat different. Here, according to Dr. Scheffer, the soil originally was composed of a thin stratum, or layer, of silt over a gravelly subsoil. The water storage capacity of this thin layer of soil was not sufficient to sustain vegetative growth during prolonged dry spells. Since the pocket gopher feeds on roots and other underground portions of growing plants, he was confronted with the choice of abandoning the area or of correcting the situation. He elected to do the latter. By increasing the depth of this layer of silt, at least in patches, by working it into a mound, always working the dirt toward the nesting chamber at the crown of the mound, the gopher could build up the water storage capacity enough to meet this requirement. By working the stones in the lower edge of the silt layer to the bottom and ejecting those near the surface from the soil mass, he could improve the water storage reservoir still more.

Contrary to Dr. Scheffer's expressed opinion that the pocket gopher dug deep into the gravel for safe nesting chambers, it is my opinion it was for the purpose of increasing the depth of the soil mass to ensure sufficient water storage capacity in the soil for plant growth. As previously stated, the nesting chambers appear immediately beneath the surface on the highest portion of the mound.

The rodent depends on the maze of tunnels leading to and from the nesting chambers for escape when in danger of being attacked by enemies. Several of these tunnels lead out in as many directions from the nesting chamber.

Insofar as my experience goes, the pocket gopher does not resort to excessive mound building in areas where the soil is deep and well drained.

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OMISSION

Dr. L. J. Meduna and Dr. J. R. Klein, Illinois Neuropsychiatric Institute, University of Illinois, Chicago, are the clinical and chemical investigators, respectively, on glucose metabolism in acute psychoses, isolation of an anti-insulin factor in schizophrenia, and the use of carbon-dioxide inhalations in the treatment of several forms of mental disorder. See "Science in Chicago," December 1947 issue, page 479.—Ed.

SHAKESPEARE'S PHYSIOLOGY

Noting Dr. Bradford Willard's article (November SM), I was struck by his reference to the omniscience of Shakespeare.

I felt much the same on several occasions while reading certain passages of William.

Dr. Willard refers to geological matters, but I have wondered how many scientists and medical men have realized that Shakespeare showed rather an uncanny ability, and had more than a little knowledge of many subjects that had no connection with the usual author's writing, especially in his time.

I draw attention to his passage in *Coriolanus* that shows that he must have been a great reader, or that he must have known personally many who delved into questions not usually of interest to authors. Harvey had only recently (or perhaps later), given his understanding of arteries and the circulation of the blood, whereas prior to Harvey it was believed that arteries were filled with air.

Here is what the layman, Shakespeare, said (italics mine):

> "True is it" [quoth the belly]
> "That I receive the general food at first,
> Which you do live upon; and fit it is,
> Because I am the storehouse and the shop
> Of the whole body: but, if you do remember,
> I send it through the *rivers* of your *blood,*
> Even to the court, the heart, to the seat o' the
>   brain;
> And, through the *cranks* and offices of man,
> The strongest *nerves* and small *inferior veins*
> From me receive that natural competency
> Whereby they live."

The foregoing shows an extraordinary interpretation of what was not very well known, even to many students of science, at that time. Shakespeare must have been an omnivorous reader, or associated with many men of letters, as of science.

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TECHNOLOGICAL NOTES

Moisture barrier. Dry heat and dry cold are always more comfortable than the same temperatures coupled with humidity. The reason heat feels better dry than damp is evident, but the why of discomfort from damp cold is not so well understood. March is a good time to recall the reason, given in an American Chemical Society paper last year. Moisture vapor diffuses through clothing faster than the molecules of nitrogen and oxygen do and carries body heat away rapidly. The suggested remedy when the air is cold and damp is a raincoat over the woolen garments, even if it is not raining.

City lime. Discovery that city water is softened with lime surprises some citizens who have heard that lime makes water hard. Some fear that every drink will load their bodies with chemicals. The chemistry is easily explained; the addition turns the hardness into insoluble calcium carbonate, or limestone, which settles out. City waterworks, although they produce limestone in the settling basins, are steady customers of the lime companies. New methods of drying the limestone sludge for agricultural purposes, and firing it to make lime that can be used in the softening process, may change the city waterworks from customers to competitors in the lime business.

Protest. Curiosity and a lively interest in new developments are certainly characteristic of most SM readers. We may—and we should—marvel at neat ways to turn nature's laws to advantage. But explanation rather than expression of amazement is proper for a discussion of such manipulations.

What prompts this observation is a release containing an irritating use of the word "wonder." This time it's about the mass spectrometer, "wonder tool of science," for detecting tiny leaks in gas-filled plumbing. A booklet about soybeans bears the title *The Wonder Bean.* A few years ago articles on sulfanilamide or penicillin often referred to them as "wonder drugs." Let's have done with this wonder business.

Gas detector. In garages, mines, and other places of employment there is frequent tragedy from carbon-monoxide poisoning. Two ingenious devices, recently reported, are said to give warning of dangerous concentrations of the gas.

One detector draws the air sample over a catalyst that oxidizes all CO to CO_2. When a predetermined setting is reached in the heat produced by this chemical reaction, a horn blows and a red light goes on.

The other device is described as "an electronic ear." A transducer sends sound waves through a tube of gas. Presence of CO—or other dangerous gas—changes the speed of the waves. When the difference is enough to denote dangerous concentration, the alarm sounds.

Buried danger. Fought-over land in Europe has to be freed of unexploded shells before it can be farmed with safety. Similar dangers exist in timber that has been hit in artillery range work; unexploded ammunition is dangerous, and buried metal plays hob with saws. An electronic coil, five feet in diameter, is used for testing suspected timber. The device indicates electrical unbalance when a log containing a piece of metal floats through it. More than 12 million board feet from 3,700 acres in the Ft. Lewis Military Reservation, Washington, will be salvaged by this method.

Country lights and phones. Electricity has come into use in rural districts more than the telephone has. Census figures give 1,385,000 farms with current but no phone, and only 464,000 farms with phones but no electricity.

Useful trolley. Disappearance of the street car doesn't help traffic congestion. Automobiles move fewer than two persons each in city traffic. Three lanes of automobiles will move only 2,700 persons per hour, but replacement of one lane of automobiles by street cars increases the number moved to 15,630 persons per hour.

M. W.

THE BROWNSTONE TOWER

MORE than a year has passed since we moved from our office in the Brownstone Tower of the Smithsonian to the A.A.A.S. Building at Scott Circle. We hardly need the enlarged picture of the Tower, which hangs in our present office, to remind us of the unique features of our room behind the clock face. Last fall I visited our old office, which is now occupied by Dr. Abbot's computer and is appreciated by her as much as it was by us. I had wanted also to see our host of the Tower, Dr. Earl S. Johnston, whose office was two floors above ours, but I had to hurry away. Now I know that I should have taken time to find him, for that was my last opportunity to see him. Dr. Johnston died on December 17, 1947.

I have called Dr. Johnston our host of the Tower because most of the rooms in it were a part of the quarters of his Division of Radiation and Organisms. He was a generous host and added much to our pleasant memories of the Tower. With my back to the elevator I could tell when he was going by, for my assistant would greet him with a smile and a wave of her hand. Now and then he would stop at our floor and come in for a chat. So often he had a humorous anecdote to tell that we would ask him for a yarn or a joke and were rarely disappointed.

The research of Dr. Johnston and his staff in plant physiology was carried on in the rooms of the west basement of the Smithsonian Building. One day he personally conducted us through his laboratories, explaining what was being done in each room and the intricate equipment that had been devised for the work. We registered the awe and admiration that any layman shows toward apparatus and methods that he does not fully understand. When the time came to prepare for our Smithsonian Centennial Issue (November 1946), we asked Dr. Johnston to write an article on the Division of Radiation and Organisms. When he promised to do it, we did not have to worry about its prompt delivery, for he was a man of his word. The nature of his work in the low-ceilinged basement is well described in that article.

The son of a Methodist minister, Dr. Johnston was born February 5, 1889, in Quarryville, Lancaster County, Pennsylvania. He graduated from Dickinson College, which was also the alma mater of the second secretary of the Smithsonian, Spencer F. Baird. In later years he would return on vacation to the beautiful countryside of southeastern Pennsylvania. It was obvious to all that he found life "full of a number of things." I have recently learned that his principal hobby was photography, and the money that most men, including the editor, waste on tobacco, provided film for his camera. He was a collector, too—of stamps, coins, etchings, and well-chosen books. Being a devotee of chess, he also collected ornamental chessmen.

The cause of Dr. Johnston's death was cancer of the lower alimentary tract. We did not know the nature of his illness at the time of his first operation. When he returned to the office, he told us cheerfully that his inner tube had been vulcanized. A second operation was required last year, and later we learned that he could never return to the Tower. His bright courage persisted to the end and he made plans and gave advice for the work of his division after he knew that he could not recover. His last view of the beloved Smithsonian came to him in his home when a motion picture, Treasure House, delineating the present activities of the Smithsonian, was shown for him. Thus a blithe spirit departed from the Brownstone Tower.

F. L. CAMPBELL

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GROWING PLANTS WITHOUT SOIL

NEIL W. STUART

Educated at Michigan State College and the University of New Hampshire, Dr. Stuart worked for a time at the New Hampshire Experiment Station. In 1932 he went to the University of Maryland on a fellowship and took his Ph.D. in plant physiology there in 1934. Since 1936 he has been with the U.S.D.A. and is now senior physiologist in the Division of Fruit and Vegetable Crops and Diseases, Bureau of Plant Industry, Soils, and Agricultural Engineering, Beltsville, Maryland.

THE possibility of growing plants without soil interests many people throughout the world. They include not only the commercial grower of flowers and vegetables, but the amateur as well. The home gardener, the city apartment dweller, the retired businessman, the disabled war veteran, the would-be promoter, all these and many others write thousands of letters asking for detailed instructions on growing tomatoes and orchids (these crops lead in interest), strawberries and cucumbers, gladioli and vanilla beans. Apparently, most of these letters are written with the best of intentions, an actual desire for information. Probably the number who make any use of the information so obtained is much smaller. Few of these people seem to realize that little or no information is available about many of their questions.

One of America's best-known plant physiologists, Dr. John W. Shive, has traced the development of our information on nutrition and methods of growing plants. His review, published in 1940,[17] divided the interval after 1700 into four periods ending in 1800, 1860, 1900, and 1940, respectively. A fifth period may now be added, beginning about 1929 and extending to the present time.

Each is marked by significant findings. Prior to the eighteenth century it was commonly believed that ". . . for nourishment of vegetables the water is almost all in all; . . . the earth only keeps the plant upright."[22] Had not van Helmont's willow, grown for five years in an "earthen vessel" containing 200 pounds of soil to which nothing was added except rain water or distilled water, produced a tree weighing 169 pounds at an expenditure of only 2 ounces of soil? An Englishman, John Woodward, disagreed with this opinion and published[22] in 1699 that "earth and not water is the matter that constitutes vegetables." His conclusion was based upon experiments in which he grew spearmint in glasses containing water from various sources, methodically recording the initial and final weights of the plants and the total amount of water consumed. He observed that the addition of an ounce and a half of garden soil to the water increased the "terrestial matter" and consequently the growth of the plants. As is the case with most research, original findings are often accepted with reluctance. In 1734 this engaging title appeared: *A Flower Garden for Gentlemen and Ladies; being the Art of Raising Flowers Without any Trouble, to blow in full perfection in the depth of winter, in a Bedchamber, Closet or Dining Room . . . As practised by Sir Thomas More, Bart.* (London, 1734).

During the seventeenth century an increasing awareness of the role of soil as a source of plant nutrients introduced the studies on ash assimilation in the next period. Methods of growing plants in water or artificial soils were published from 1860 to 1900, followed, at the turn of the century, by further development of solution formulas and study of nutrient deficiencies. Serious attempts at substituting soilless culture for crop production have taken place only during the past two decades and were, as we shall presently see, stimulated immeasurably by the second world war. In the late twenties, growing plants in jars of water or crocks of sand was a standard experimental method in plant physiology, but it seemed to have little commercial application. True, the greenhouse industry near the cities was annually experiencing increasing difficulty in obtaining adequate amounts of manure and suitable soil for use in their beds and benches. Much labor is also involved in handling soil in greenhouses and in growing plants in soil. These problems encouraged a few research workers to substitute ordinary builders' sand for soil and manure and to rely on application of fertilizers dissolved in water for growing the plants. In 1928 Robbins, working at the New Jersey Experiment Station, had suggested[15] the possibilities of sand culture for research and commercial work in horticulture, following extensive trials with vegetables and fruit-tree seedlings. At the same time Biekart and Connors, also of the New Jersey Station, predicted that "the possibility of growing carnations commercially in sand is not remote." A later publication by these men stated that the production of quality carnations in sand with nutrient solution and in quantities comparable with those grown in composted soil seems to be thoroughly practical and economical.[2] At Ohio State University, Laurie described the use of washed sand as a substitute for soil in greenhouse culture of ornamentals.[10] Recently, Robbins has reviewed the whole question of growing plants in sand cultures for experimental work.[16] It is in this field rather than that of commercial production that sand culture has been most useful. Many laboratories for nutrition research in agronomy and horticulture have profitably investigated problems of nutrient deficiency and balance by means of sand culture tests. The failure of this method to attract commercial attention may be attributed to inertia of the industry, lack of information about specific problems such as media, time and amount of nutrient application, etc., and the need for a better system of soilless culture.

The other possibility, that of growing plants in water, was actively championed at about the same time by Dr. W. F. Gericke in California.[7] His enthusiasm for "hydroponics" made thousands of converts, and the growing of plants in water became the victim of extravagant and sometimes unscrupulous propaganda. In its original form the installation for hydroponics consisted of a shallow basin, tank, or trough partially filled with water and dissolved nutrients and covered with wire netting. The netting was covered with a layer of some such material as wood shavings, straw, salt hay, rice hulls, or even soil. The plants were grown in this layer with some of their roots extending into the nutrient solution below. The mulching material maintained a high humidity around the root crown of the plant and supported the growth of feeder roots that could absorb oxygen. Originally, the nutrient solution was not artificially aerated. Since the nutrient solution supplies oxygen as well as nutrients and water to the plant and since only relatively small quantities of oxygen are soluble in water, lack of it undoubtedly was often a limiting factor in plant growth. It is now recognized that if plants are to be successfully grown in water culture some means of aerating the solution, either by circulating it with a pump that mixes air with it or by forcing air into the solution through perforated pipes, is imperative.[8] There are other rather obvious difficulties in using the method, such as the problem of supporting the plants and maintaining suitable nutrient balance. In experienced hands the method is capable of producing crops on a par with similar crops grown by conventional soil methods. Superior production by water culture is scarcely to be expected, and unless all precautions are observed crop yields may be much reduced below soil yields.

INTEREST in soilless culture might have declined had it not been for the development of a new method of applying water and nutrients to the plants. In this, the subirrigation method of culture, watertight beds or benches are filled with gravel or other suitable inert material, which is irrigated from the bottom of the bed. This system, in its simplest form, was conceived independently during the same year at the New Jersey and Indiana Agricultural Experiment Stations.[5,21]

Irrigation is accomplished by pumping the nutrient solution from the storage tank or cistern into the bench, the bottom of which slopes slightly from the sides to the middle and also lengthwise to the point where the solution enters and subsequently leaves the bench. Inverted half-round clay tiles or boards nailed together to form an inverted V are placed end to end lengthwise along the middle of the bench and serve as a channel for the solution. When the solution has nearly filled the bench, the pump is stopped and the solution drains back to the tank by gravity. In the newer benches built for subirrigation, a solution channel is formed by making a longitudinal depression along the lowest part of the floor "valley." The channel thus formed is covered with bricks or slabs of concrete provided with drainage holes at the sides. To facilitate rapid drainage these holes are covered with coarse gravel. This is known as the direct-feed system and is useful in greenhouses or small units. For larger installations it is more economical to employ the gravity-feed system. The beds or benches are built in series of 3 or 4 sections, each on a higher elevation and slightly longer than the one following it. Two solution tanks are used in this system. The larger one is located at the lower end of the beds and is below the bed level. It is connected with a somewhat smaller tank above the level of the beds. The smaller tank should have a capacity approximately one-half the total volume of the first sections of all the series. The nutrient solution flows into the first sections of the several series by gravity and then successively through the other sections, finally emptying into the sump tank. By this means only the solution for irrigating the first sections of the series has to be pumped, gravity flow irrigating the rest of the sections. The solution in the sump tank is analyzed, reinforced with the necessary chemical nutrients, made to volume, and pumped into the elevated tank in preparation for the next irrigation. Eastwood has described a modification of this construction known as the open-flume system, which was originated by Carroll Klotzbach in Florida.[6] All the solution is carried to and from the beds by means of a flume, so that no piping or valves are necessary except at the pump. The nutrient solution is stored in an above-

Photo by Signal Corps, U. S. Army

FIG. 1. LT. COL. EWING W. ELLIOTT

DIRECTOR OF HYDROPONICS, FAR EAST COMMAND.

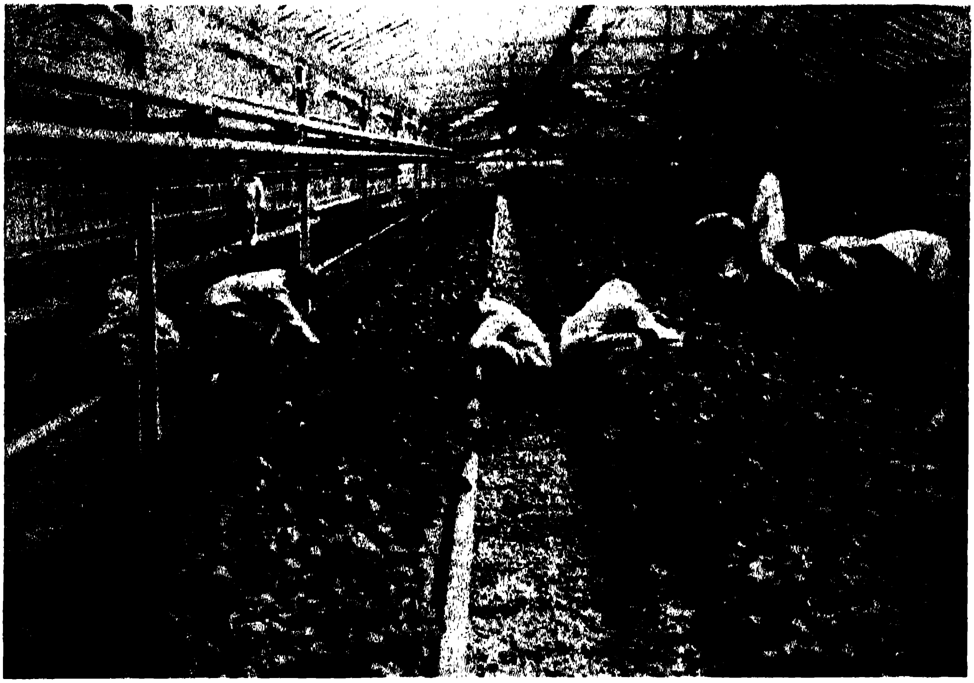

FIG. 2. SOILLESS RADISHES FOR THE U. S. ARMY IN JAPAN

JAPANESE WORKERS HARVEST A CROP GROWN AT THE 8002ND HYDROPONIC FARMING DEPOT, CHOFU, NEAR TOKYO.

ground tank, with a small sump tank for the pump, or a belowground cistern.

Interest in this subirrigation method of soilless culture was increasing rapidly at the outbreak of the second world war. Resultant shortage of construction materials and nutrient chemicals curbed further expansion by private individuals. Interest in the method was stimulated in another manner, however. In 1944 General H. H. Arnold, Commanding General, Army Air Forces, observed the soilless culture activities at the AAF Regional and Convalescent Hospital (now Pratt General Hospital) at Coral Gables, Florida, in connection with the AAF rehabilitation program. He became interested in the possibility of utilizing this method for supplying AAF personnel with fresh vegetables at isolated bases.

The first installation was established on Ascension Island, an isolated island in the middle Atlantic, early in 1945. This plant consisted of 25 beds 400 feet long and 3 feet wide; it was irrigated with distilled sea water. The yield of fresh salad vegetables, cucumbers, tomatoes, radishes, lettuce, and green peppers, during the first year of operation, was 94,000 pounds.

In the summer of 1945, 75 beds were constructed at Atkinson Field, British Guiana. Production of vegetables at this installation in 1946 amounted to 234,337 pounds. Costs of production ranged from 4 cents a pound for cucumbers to 22 cents a pound for peppers.[20]

A third soilless culture garden (the Army refers to them as "hydroponics" even though subirrigation is implied) was constructed on Iwo Jima in the summer of 1945 and began operation in November of that year. In the same month it was decided to establish two hydroponic garden projects in Japan, with a total area of 80 acres. This decision was reached because of the difficulty of providing highly perishable vegetables over extended supply lines, with the heavy shipping losses involved. It is known that much of the Japanese soil is contaminated, and it was considered unsafe to serve uncooked soil-grown vegetables to United States troops.

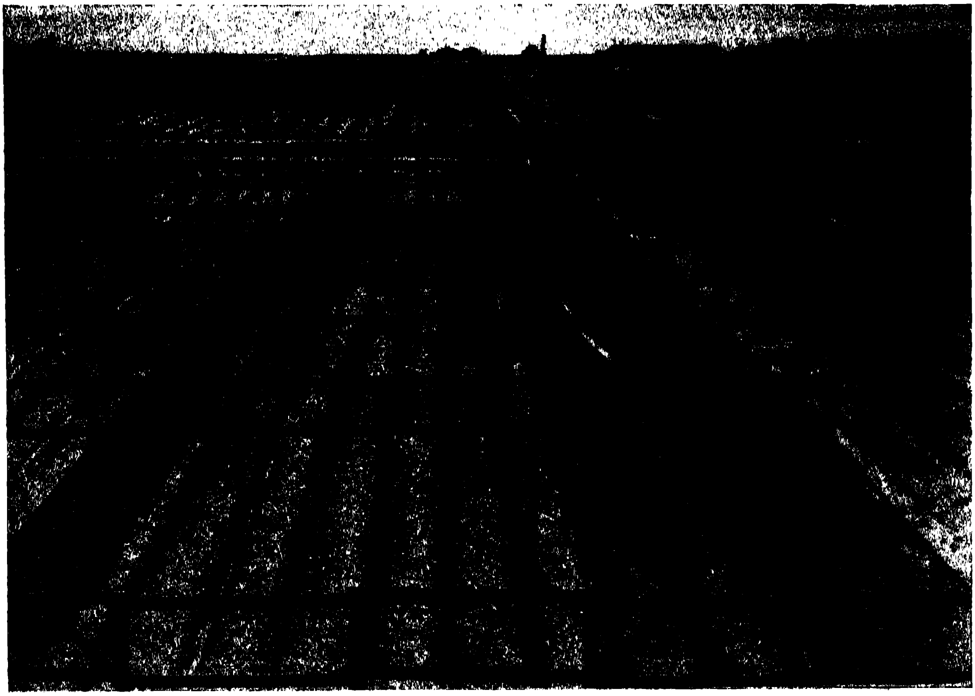

FIG. 3. CONSTRUCTION OF A 5-ACRE HYDROPONIC UNIT AT OTSU, NEAR KYOTO

THE WATER NEEDED FOR THIS PROJECT WILL BE DRAWN FROM LAKE BIWA, WHICH LIES IN THE BACKGROUND.

Lt. Col. Ewing W. Elliott, an Air Force Quartermaster officer, was appointed supervisor of the two Japanese Hydroponic Garden Projects, and he deserves much credit for planning the construction and directing the operation of this vast enterprise (Fig. 1).

The Japanese soilless culture gardens are constructed in units of 5 acres. Each unit contains 87 beds 300 feet long and 4 feet wide. The first section of each bed is 120 feet long; the second, 100; and the third, 80 feet. One of these units, at Chofu, near Tokyo, is under glass; the remaining 10 units at this location and 5 units at Otsu are in the open (Figs. 2, 3). It was expected that more than 10,000,000 servings of fresh vegetables would be produced during 1947.

Because of decreased appropriations and manpower, the Army Air Forces withdrew from active participation in the hydroponic program in 1946. The Quartermaster Corps was given authority to approve the establishment of new gardens where the following conditions exist:

a) No soil is available in which vegetables can be grown safely in the normal agricultural manner.
b) The installation is so located that no fresh vegetables can be supplied by shipping in adequate quantity or at reasonable cost.
c) The post camp or station is permanent or is expected to be in existence for a considerable period.
d) There is at least a four-month growing season each year.
e) There is an ample supply of suitable water.

The operation of all overseas hydroponic gardens was assumed by the respective theater commanders. A Technical Manual, *Nutriculture,* was published for the purpose of providing construction and operating data.[18] This Manual indicates that it should be possible to produce tomatoes, for example, at an Army installation in a favorable climate for about one-half the cost of shipping tomatoes to the base by refrigerated maritime shipping over a distance of 1,000 miles. This takes into account a 30 percent loss due to spoilage.

At the present time in the United States, there is widespread interest in soilless culture in southern Florida, where approximately 20 installations produce vegetables chiefly for the winter market (Fig. 4). In the Northern states the method is used in greenhouses for winter production of tomatoes and cucumbers and also for growing ornamentals, roses, carnations, and chrysanthemums. Shortages of critical materials and chemicals, as well as a serious lack of trained operators, are restraining the spread of soilless culture. Present installations have not produced outstandingly greater yields, and consequently there has been little inducement to change production systems. Recently the development of a method of subirrigating benches containing soil has given promise of achieving many of the same results as soilless culture with less difficulty than in the latter method. This method, in principle, was described by Jones and Doran in the *Florists' Exchange* for May 29, 1943[9] and by George Ball in the *Florists' Review* for May 27, 1943.[1] Kenneth Post, at Cornell University, has extended these results and applied them to entire greenhouse benches.[14] A concrete, or suitably waterproofed wooden, bench is partially filled with an inch or two of gravel and sand. A constant water level is maintained in the sand by means of an automatic float valve attached to the water inlet. The bench is filled with soil kept at any desired moisture content by capillary rise of the water from the sand and gravel in the bottom of the bench. Too high a level of water in the gravel results in saturated soil and the production of anaerobic conditions. Conversely, too low a level results in a loss of capillary rise. It is, however, relatively easy to adjust the water level so that favorable growth conditions obtain. Less attention is required for adjustment of the nutrient level in the soil than is required where soilless culture is used. The method holds much promise, although it has not been tested enough for complete evaluation.

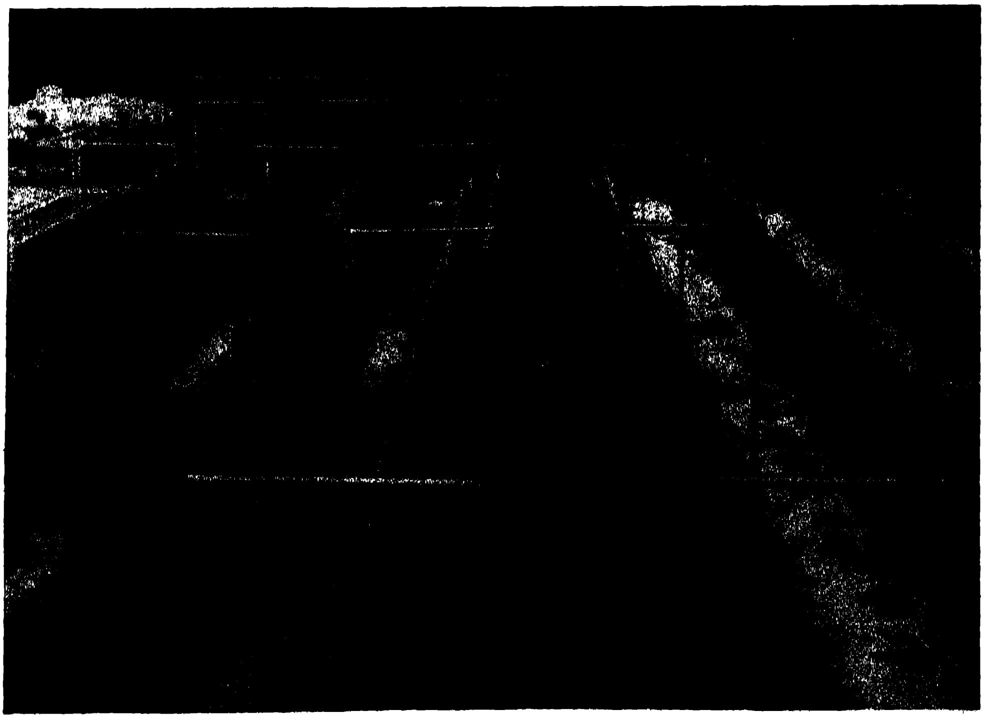

Photo by Tom Butler, courtesy of Joel Kuperberg

FIG. 4. TYPICAL SOILLESS CULTURE BEDS, SOUTH MIAMI, FLORIDA

HERE TOMATOES AND CUCUMBERS ARE BEING GROWN COMMERCIALLY BY THE SOLARIPE HYDROPONICS CORP.

Photo by Bureau of Plant Industry, Soils, and Agricultural Engineering, U.S.D.A.

FIG. 5. CHRYSANTHEMUMS GROWING IN SUBIRRIGATED GRAVEL

LIKE THE LILIES ON THE COVER OF THIS ISSUE, THESE PLANTS ARE BEING STUDIED AT BELTSVILLE, MD.

At the Plant Industry Station, Beltsville, Maryland, soilless culture investigations have been in progress since 1941. The section working with floriculture and ornamental horticulture of the Division of Fruit and Vegetable Crops and Diseases, Bureau of Plant Industry, Soils, and Agricultural Engineering, maintains 20 concrete greenhouse benches comprising 5,400 square feet and divided into 88 sections, each section being provided with a solution tank and pump for subirrigation, so as to give control of experimental treatments. These benches are used in growing plants in phytopathological, genetic, and physiological investigations.[3, 11, 12, 19] The last include studies on nutrient balance and absorption, effect of temperature, light, growing medium, etc. (Fig. 5). It is now generally recognized that rather wide limits of solution composition are capable of producing equally good growth with many plants. Less attention has been given in the past to the choice of the growing medium, other than soil, and its possible role in furnishing nutrients to the crop growing in it.

One of the soil substitutes now in use at Beltsville, expanded vermiculite, has proved superior to soil or sand for certain horticultural purposes.[18] This product is obtained from naturally occurring deposits in Montana and other locations in this country. It is classified as a hydrated magnesium aluminum silicate and is used extensively as an industrial insulating material. The ore is laminated and made up of two minerals, vermiculite and biotite. In the former the scales are bonded together with water molecules and in the latter with potassium. When the ore is heated to about 2,000° F., the water is converted to steam and expands the ore from twelve to fifteen times its original volume. The resulting product is sterile, light in weight, highly absorbent, and retentive of water and air. These physical properties make vermiculite well adapted as a medium for subirrigation. In addition, the available calcium, potassium, and magnesium are sufficient for seedling growth until they are of transplanting size. Vermiculite is also useful as a propagating medium in the cutting bench and as a growing medium in subirrigated nutriculture benches. A valuable property of the material is a water-holding capacity so great that nutrient solutions need to be pumped into the benches only once or twice a week.

Sintered shale, known as Haydite, a commercial product used in making low-density concrete, is another useful growing medium. It is porous, light in weight, and has a higher water-holding capacity than gravel. It, too, contains calcium and potassium that are available to plants growing in it. Haydite has the further advantage of being more durable than vermiculite.

Since phosphorus and iron in the nutrient solution tend to precipitate each other, the maintenance of a low level of the former is desirable in order to avoid iron deficiency. Such maintenance of a low, but adequate, level of phosphorus requires that frequent additions be made to the nutrient solution. Trials have been made at the Plant Industry Station since 1945 to determine whether untreated Florida pebble phosphate rock, as well as defluorinated phosphate rock, used as media in subirrigated nutriculture benches, would provide sufficient phosphorus for satisfactory growth of various floricultural crops. These phosphate-containing materials were obtained from the Coronet Phosphate Company, through K. D. Jacob, Division of Fertilizer and Agricultural Lime of this Bureau. Concrete bench sections 12 feet long, 57 inches wide, and 7 inches deep were filled with these two materials and others with washed pea gravel. The aggregates ranged from 1/16 to 1/2 inch in particle size. A complete nutrient solution, except that it contained no phosphorus, was pumped daily into the beds containing the phosphate media. Similar sections containing gravel received the same solution and in addition 3.8, 7.7, 15.5, or 31 parts of phosphorus (as P) per million of the solution, were added at two-week intervals. The entire solutions were replaced monthly. The phosphorus content of each solution was determined photometrically, using the amino-naphthol-sulfonic acid and ammonium molybdate method, just before new additions of phosphorus were made or the solutions were replaced. These data show that the amount of phosphorus in the solution from the phosphate media was always less than 1 and usually less than 0.5 p.p.m. More phosphorus and considerably more calcium were released from the defluorinated product than from the untreated pebble phosphate rock. The pH of the solution from the former tended to rise above 7.

Growth data show that six varieties of chrysanthemums were able to obtain sufficient phosphorus from the pebble medium to equal the growth of plants in gravel that received 300 liters of solution containing 15.5 p.p.m. of phosphorus at two-week intervals. Growth of the plants in defluorinated rock was inferior to that in the untreated phosphate rock. The same two materials have been used for the growth of snapdragons and Easter lilies with very satisfactory results.

In nutriculture tests nitrogen is the element absorbed by plants in largest quantities and is the one producing the greatest effect on growth. During rapid growth, plants rapidly deplete the nitrogen content of the nutrient solution. The use of natural organic nitrogen materials that would liberate nitrogen over an extended period is not feasible in soilless culture because of the disease hazard.[12] However, the same effect has been accomplished by the use of urea-form fertilizers prepared and made available by the Division of Fertilizer and Agricultural Lime of this Bureau.[4]

Four preparations with urea/formaldehyde mol. ratios of 1.15, 1.20, 1.27, and 1.31 (Table 1) were used as the source of nitrogen for growing nine species of plants in subirrigated greenhouse bench sections containing Haydite. Each section was 8 feet long, 57 inches wide, and 7 inches deep. Two hundred liters of a complete nutrient solution, except for nitrogen, were pumped into each section twice daily. One hundred grams of the urea-form materials were added to the solution tanks on April 2, April 16, May 14,

TABLE 1

Properties of Urea-form Materials Supplied by the Division of Fertilizer and Agricultural Lime

Sample	U/F Mol. Ratio	Total Nitrogen Percentage	Solubility Index p.p.m.
G–4	1.15	37.06	42
G–2	1.20	37.68	73
G–3	1.27	38.50	105
G–5	1.31	38.50	131

and June 11. Other elements were added to the tanks in amounts sufficient to provide for good growth. For comparison, other sections were provided with the same nutrient solution and 70 p.p.m. of nitrogen from sodium nitrate and ammonium sulfate. Additions were made weekly to this solution until a total of 406 p.p.m. of nitrogen was added between April 8 and July 3. The nitrate nitrogen content of the solutions was determined photometrically by the phenoldisulfonic acid method at weekly intervals until the plants were in bloom and after they were harvested.

The solution analyses show that more residual nitrate nitrogen was present in the solutions containing the higher mol. ratios of urea-form 1.27 and 1.31 than in the solutions with the lower ratios, 1.15 and 1.20. The plant weight data confirm the fact that more nitrogen was available from the higher-ratio urea-form than from the lower. The nitrogen in urea-form 1.15 and 1.20 was not sufficiently available for optimum plant growth. Urea-form 1.27 and 1.31, on the other hand, produced larger plants than did the inorganic nitrogen solution that was partially renewed every week.

After the plants were harvested on July 22, the nutrient solutions were pumped into the beds twice daily until August 26. Although no urea-form was added after June 11, nitrate nitrogen continued to accumulate in the nutrient solutions roughly in proportion to the mol. ratios of the four materials. The use of urea-form, together with some inorganic nitrogen, would seem desirable in soilless culture studies and should result in maximum growth with minimum adjustment of the solution.

In the two hundred and fifty years since Woodward grew his spearmint, much progress has been made in soilless culture. One of his conclusions is still worth repeating: "There is a procedure in every part of nature that is perfectly regular and geometrical, if we can but find it out; and the further our searches carry us, the more shall we have occasion to admire this and the better it will compensate our labour."[22]

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THE WEATHERMAN

The Weather Bureau now has maps recording daily conditions in the Northern Hemisphere for some forty years—the maps, with their meteorological whorls, much resembling large fingerprints—News report.

He has long fathered and excused
The prodigals and profligates—
The searing sun
That turns the fields a parchment brown,
The raiding rains
That steal the precious holidays,
The mayhem winds
That stave in roofs and smash at dykes—
And all the calms of in-between,
The means of fair and moderate.
But now he has a science system.
He fingerprints his sons
And charts their growth.
He's studied their heredity,
The homes from whence they come,
And knows from fingers' whorls
How aberrants are born.
And he can warn
Of sudden treachery in March,
Delinquency in charming April's youth,
Of silent felons in a summer's night
And arson in the fall.

JOSEPH HIRSH

THE RETURN OF BLUEBACK SALMON TO THE COLUMBIA RIVER

FREDERIC F. FISH

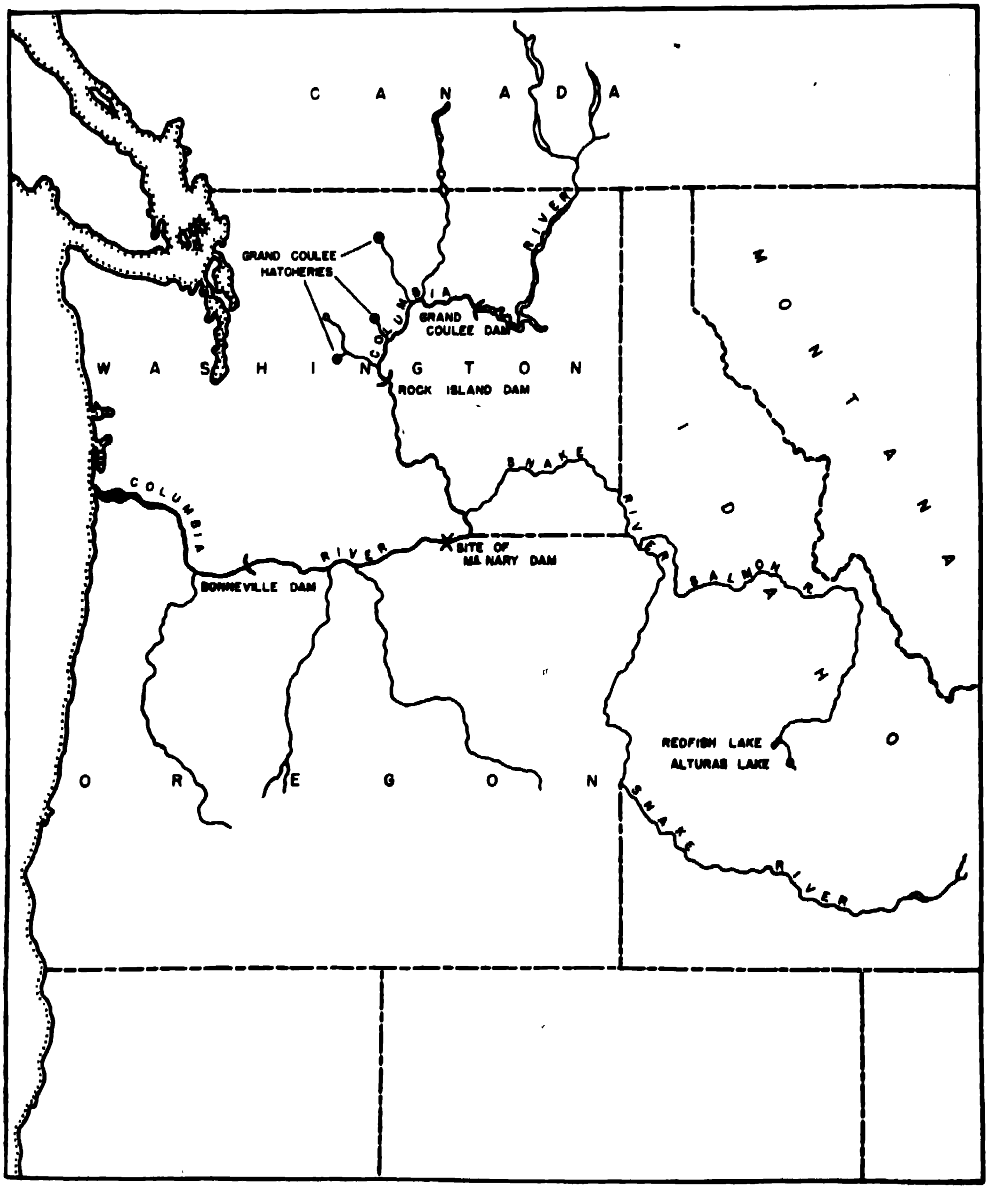
Dr. Fish (Sc.D., Johns Hopkins, 1931) received his graduate training as parasitologist at the School of Hygiene and Public Health at Johns Hopkins. He was appointed aquatic biologist in the old Bureau of Fisheries in 1931 and conducted research on the pathology of fishes in the East before being detailed to the Seattle, Washington, laboratory in 1935. As chief, Western Fish-Cultural Investigations, U. S. Fish and Wildlife Service, with headquarters at Oregon State College, he has directed the research program in the application and development of artificial propagation of salmon on the Pacific Coast, particularly at the Service's Grand Coulee fish-cultural stations.

THE year 1941 was a crucial one for the blueback salmon of the Columbia River. During that year, one brood came closer to extinction than was realized by more than a few individuals. The immediate causes were not overfishing, hydroelectric power development, or irrigation—although these factors continued to exert their long-standing effects. The direct causes can be attributed to an "act of God" plus—in large measure—lack of knowledge concerning the basic principles of effective artificial propagation.

With the security and assurance provided by subsequent developments, those concerned with the Columbia River blueback salmon may be interested in a brief recapitulation of events that transpired during the early 1940s. This particular piece of fishery history bears upon the problems of the immediate future on the Columbia River.

The commercially important species of salmonid fishes of the Columbia Basin are the chinook, silver, chum, and blueback salmon, as well as the steelhead trout. Of these, the blueback now contributes but a small bit to the total fishery resources—or approximately 2 percent of the commercial salmonid-fish yield of the river during recent years. In spite of its small part in the fishery resources, however, the blueback salmon offers a valuable "biological indicator" of the general over-all rise and fall of the fish populations, for it alone spawns exclusively in the upper river. For this reason, the counts of fish passing Bonneville Dam, which have been recorded continuously by the War Department Corps of Engineers since May 1938, include all blueback salmon escaping the commercial fishery of the lower river. The annual count at Bonneville Dam, plus the commercial catch of the lower river and minus the commercial catch of the river fishery above Bonneville, yields a reasonably accurate estimate of the total run of this species to the river for the year. A similar evaluation cannot be made for the commercially more important species because of their common characteristic of spawning in considerable—and unknown—numbers in the tributaries entering the Columbia below Bonneville Dam. The same factors—disregarding those of purely local consequence—affect all species of salmon in the same way, although to a varying degree. A factor adverse to the blueback will, in general, prove adverse to the silver or to the chinook as well. Likewise, general measures effective in rehabilitating the blueback runs presumably would prove beneficial if applied to the other species.

It is often assumed that the blueback salmon spawn rather generally throughout the upper Columbia Basin. The evidence is quite conclusive that such is not the case, for this species spawns only in streams tributary to a lake in which the progeny remain during their first year of life. All lakes of the upper Columbia Basin now accessible to migratory fishes lie above the Rock Island Dam—with the exception of Redfish and Alturas lakes in the Salmon River drainage. No doubt these two relatively small lakes contribute their share to the Columbia River blueback resources, but that contribution must be small in comparison with that from the larger and more numerous lakes of the upper Columbia. Although the construction of Grand Coulee Dam in 1939 blocked access to important lakes formerly frequented by the blueback salmon, still the runs belonging to those lakes continue to pass over the Rock Island Dam en route to the upper Columbia. Fish counts, maintained at the Rock Island Dam since 1933 by the U. S. Fish and Wildlife Service, are believed to



THE COLUMBIA BASIN

SHOWING THE LOCATION OF DAMS, HATCHERIES, AND LAKES MENTIONED IN THE TEXT.

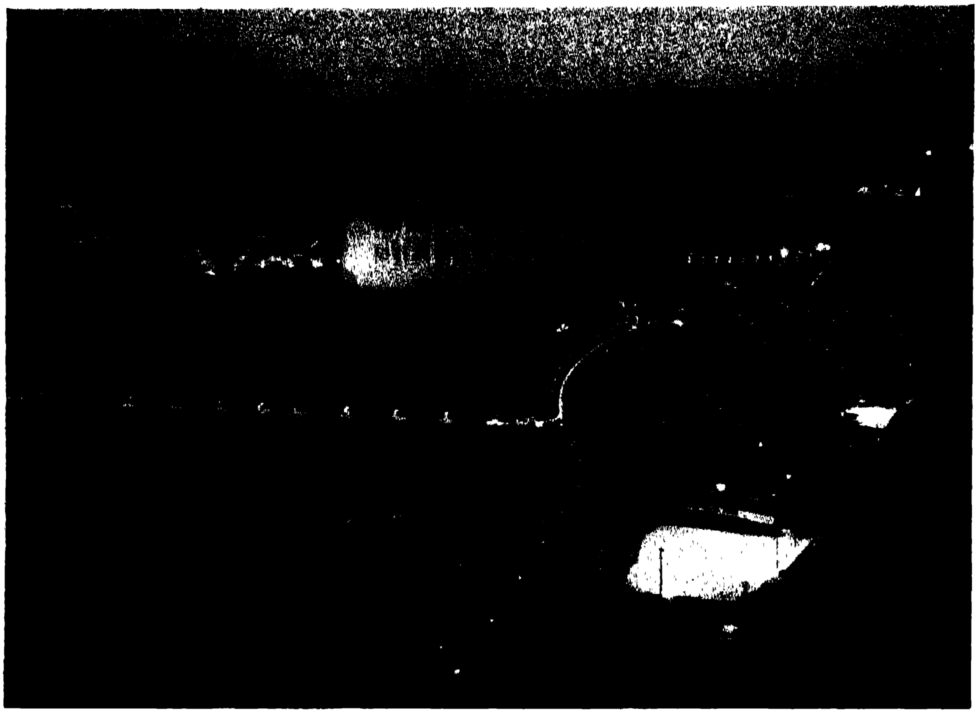
LEAVENWORTH (WASHINGTON) STATION, FISH AND WILDLIFE SERVICE

CENTER FOR BLUEBACK SALMON PROPAGATION IN THE GRAND COULEE FISH-MAINTENANCE PROJECT.

constitute a reasonably accurate index of the number of blueback salmon actually reaching the spawning grounds.

FROM all outward appearances, there was little to indicate that 1941 was an atypical year for the blueback salmon. The commercial pack of 33,070 cases and the Bonneville count of 65,741 both were well within the realm of normal. Although the commercial fishery above Bonneville took an estimated 65 percent of the fish passing Bonneville (in comparison with a "normal" take of about 40 percent), still the calculated escapement to the spawning grounds was not too far from the ordinary. It was not until it was learned that the run of bluebacks passing Rock Island Dam that year amounted to only 949 fish that definite evidence of an atypical development was at hand.

There still is no clear-cut explanation as to why, of the 25,000 fish estimated to have escaped the commercial fishery in 1941, only 949 appeared at Rock Island. Probably the most acceptable explanation lies in the abnormally warm water temperatures that prevailed that year. Between June 15 and August 31, 1941, the temperature of the Columbia River at Bonneville averaged 68.5° F. in comparison with a normal average temperature approximating 66° F. On July 20, 1941, the Columbia reached an all-time high temperature of 74.5° F.—hot water indeed for the blueback salmon, but a congenial temperature for certain of their microscopic enemies. One of these, the myxobacterium *Chrondrococcus columnaris*, markedly affected by water temperatures, is particularly lethal to blueback salmon. Below 60° F., columnaris disease is of little consequence, but between 60° and 70°, *C. columnaris* invades the inevitable cuts and abrasions on fish, quickly establishing a secondary infection that may prove lethal. Above 70° F., *C. columnaris* becomes a path-



Courtesy of the U. S. Bureau of Reclamation

GRAND COULEE DAM

IN 1939 THIS DAM CLOSED THE MIGRATION ROUTE OF THE UPPER-RIVER SALMON.

ogen in its own right and needs no mechanical injuries to open a door through the protecting mucous coating of the skin and gills. In the high sixties, a degree or two difference in temperature spells a great difference in the severity of columnaris disease. It was common talk during the summer of 1941 that blueback salmon were turning into Draino Lake, the Big White Salmon River, Eagle Creek, and other lower Columbia tributaries, in which they did not belong, presumably seeking relief from the abnormally warm water. It is assumed that *C. columnaris* affects the runs to some degree each year, but that it was particularly serious during 1941 because of the higher water temperatures.

The appearance of only 949 bluebacks at Rock Island Dam in 1941 offered definite proof that an abnormal situation had developed. When 851 of the 949 died in the Grand Coulee hatchery holding ponds prior to spawning, it was clear enough that the abnormality had reached cataclysmic proportions.

A digression is in order at this point to explain why the adult bluebacks were in hatchery holding ponds during the summer of 1941. When Grand Coulee Dam blocked the upper Columbia to migratory fishes in 1939, all fish were relocated from their ancestral spawning grounds by trapping them in the fish ladders at Rock Island Dam for distribution by tank truck to hatcheries, or to suitable tributaries entering the Columbia River between Rock Island and Grand Coulee Dams. The unprecedented relocation experiment was predicated upon validity of the "parent-stream theory," by which it was assumed that the progeny of the relocated fish would return to the streams in which they were spawned and reared and not to their ancestral spawning areas. If so, the trucking could be discontinued after one full cycle. The relocation of the upper Columbia salmon runs—known as the Grand

Courtesy of the U. S. Bureau of Reclamation

ADULT BLUEBACK SALMON EN ROUTE TO THEIR SPAWNING GROUNDS

THIS SPECIES AVERAGES ABOUT THIRTEEN INCHES IN LENGTH AND FOUR POUNDS IN WEIGHT.

Coulee Fish-Maintenance Project—was performed by the Fish and Wildlife Service between 1939 and 1944 and proved eminently successful. Because the blueback runs were obviously small at Rock Island during the summer of 1941, all but 22 of the 949 fish were taken to the hatcheries for artificial propagation—supposedly in the interests of greater protection.

The story of the Columbia River bluebacks at the end of 1941 added up to the following facts: the 1945 run would be small at best, for it would be composed of virtually nothing except the minority three-year and five-year age classes; and, short of a miracle on the Fish and Wildlife Service's Grand Coulee Fish-Maintenance Project, the Columbia River blueback was destined to join the ranks of extinct species at a very early date.

Both these prophecies were amply fulfilled in 1945. The commercial pack of blueback salmon that year amounted to a mere 112 cases. The Bonneville count hit bottom with but 9,501 fish of this species counted over the boards.

Small wonder it was, then, that on July 7, 1946, the *Oregon Journal*, a Portland newspaper, featured an editorial under the title "Blueback Swan Song" stating, in part:

> The concrete curtain that fell on the Columbia River salmon runs at Grand Coulee in 1938 has claimed its price in fish life. The principal victim has been the choice blueback salmon. The commercial importance of this fish virtually disappeared in 1945 in the Columbia River. Only 112 cases of this rich and high quality fish were canned in all the canneries from The Dalles to the sea. . . . Biologists knew the fate facing the blueback when the dam began rising from the bottom of the Columbia. Their only hope was transplanting matured blueback, headed above Grand Coulee, into blueback streams below the dam. The U. S. Fish and Wildlife Service in 1939, and for several seasons afterwards, captured upstream blueback migrants at Rock Island Dam below Grand Coulee and transplanted them to the Okanogan and Wenatchee Rivers which have lakes at their headwaters. . . . How successful these transplantings were cannot be determined but the obvious fact is that there has oc-

Courtesy of the U. S. Bureau of Reclamation

EGGS FOR ARTIFICIAL PROPAGATION

SINCE THE PACIFIC SALMON INVARIABLY DIE FOLLOWING SPAWNING, IN HATCHERY OPERATIONS THE SEXUALLY MATURE FEMALE FISH ARE KILLED AND THE EGGS REMOVED BY INCISION OF THE ABDOMINAL CAVITY.

> curred a wiping out of the major part of the Columbia's blueback population in the course of a few years. . . . It all adds up to the fact that Oregon and Washington have lost a valuable fishing industry.

That was the low point in blueback history. Two weeks later, on July 23, 1946, the Portland *Oregonian* carried an Astoria dispatch headlined BLUEBACKS IN COMEBACK. This dispatch stated, in part:

> The blueback salmon, choicest of all the varieties of fish that frequent the Columbia River, is making a comeback. The annual blueback run suffered a drastic annual decline in volume from 1940 through 1945 until, in the latter year, commercial fishing of blueback was almost nonexistent. . . . In the present season, the blueback count at Bonneville has already reached 51,451 and is expected to exceed 60,000 before the end of the run. This has taken place despite the fact that commercial fishermen, elated over the reappearance of the run, have taken 49,000 pounds so far this season.

Just thirteen months later—on August 23, 1947—the Associated Press carried this dispatch:

> The rejuvenated blueback salmon run resulted in a catch of more than a half million pounds for Columbia River fishermen this June and July. The Oregon State Fish Commission reported this and said a joint Oregon-Washington study would be made in an attempt to determine if the blueback is back to stay.

In short, the Columbia River blueback resources, which had dwindled since the turn of the century, really went into a tailspin during the forties and arrived at the threshold of extinction in 1945. Just two years later, the resources bounced back to provide a commercial yield equal to any of the past twelve years and still furnish a spawning escapement approximately twice that of any previous year of record. The obvious question arises: Is this spectacular development of the past summer merely the final burst of a skyrocket, or can the resources be sustained? A critical review of pertinent data may reveal the answer.

The blueback runs of the Columbia River unquestionably have diminished since the heyday of the nineties, when some 2,400,000 pounds constituted an average annual pack for this species. Packs of this magnitude became progressively less frequent following the turn of the century—the last one having been recorded in 1923. Presumably, competing water interests progressively eliminated many of the natural blueback spawning areas during the first two decades of the twentieth century, with the inevitable consequence that the yields diminished. The blueback fishery was more or less stabilized during the later twenties and thirties—only to drop hard in the early forties.

The story of the Columbia bluebacks during the past twelve years—a period that includes the end of the stable period, the drop, and the recovery—is presented by the data in Table 1. These data indicate that the blueback runs were reasonably stable until 1941. They also indicate that the virtual extinction of the blueback runs—bemoaned with good reason by the editor of the *Oregon Journal*—was a reality, and that the low pack of 1945 did not stem merely from the fish eluding the fishermen's nets. Finally, these data indicate that the recovery of the fishery mentioned in the press during 1946 and 1947 also was a reality, for *both* the take by the commercial fishermen and the escapement of fish to the spawning beds rose sharply to indicate a total run almost double that during the stable period between 1936 and 1941.

TABLE 1

DATA AND ESTIMATES OF COLUMBIA RIVER BLUEBACK RUNS, 1936–47

Year	Estimated Number of Blueback Taken in Commercial Fishery[1]	Effective Escapement to Spawning Grounds (Rock Island Count)	Gross Estimate of Total Run to River
1936	104,400	16,501	120,901
1937	83,800	15,087	98,887
1938	106,157	17,123	123,280
1939	67,157	19,591	87,072
1940	90,480	26,894	117,374
1941	126,242	949	127,191
1942	48,120	15,782	63,902
1943	36,336	17,665	54,001
1944	13,980	4,932	18,912
1945	2,149	7,120	9,269
1946	12,500[2]	46,563	59,063
1947	125,000[2]	79,489	204,489

[1] Poundage of bluebacks landed on Columbia River, as reported by the Washington State Department of Fisheries, divided by 4 to convert to numbers of fish.

[2] Numbers estimated from press releases—official data not yet tabulated.

The evidence is conclusive that only a great increase in the number of bluebacks entering the river during the past summer can account for both phenomena. The high count at Rock Island Dam (approximately one-half the Bonneville count) continues to indicate that the great majority of the 1947 bluebacks were bound for the upper Columbia, for approximately one-half of the fish passing Bonneville normally are caught in the upriver fishery. If these assumptions are tenable, the key to the exceptional 1947 runs should be sought in the history of the parent brood (1943) in the area above Rock Island Dam.

All fish passing Rock Island Dam since 1938 have been involved in the Fish and Wildlife Service's Grand Coulee Fish-Maintenance Project—from which considerable data are available. Pertinent data from this source are listed in Table 2.

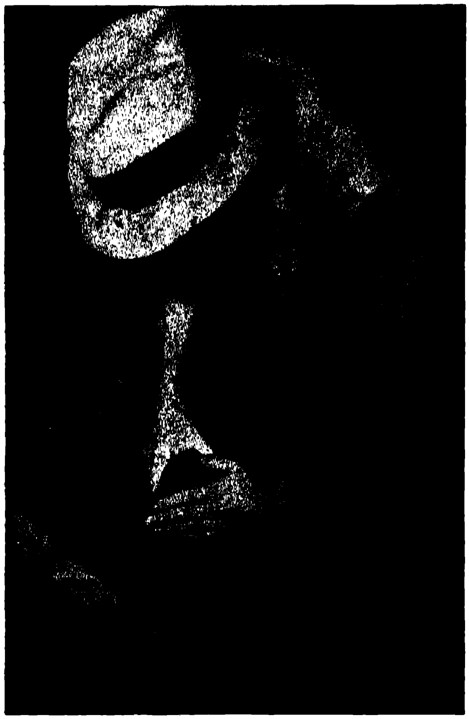
In Column 3 of Table 2, it will be noted that of the parent 1943 brood, 13,254 adult fish were estimated to have spawned naturally. This was by no means an exceptional number, for it was exceeded in 1933, 1935, 1936, 1937, 1938, and 1939. It is, in fact, less than the average number of bluebacks spawning naturally during the previous years of record. During the period 1933–38, the bluebacks had full access to their ancestral spawning grounds of the upper Columbia, as Grand Coulee Dam was not yet a barrier. None of the larger runs of previous years—including the 40,737 of 1933—ever produced a return in any way comparable to that by the 1943 run in 1947. It would appear reasonable to assume, therefore, that successful natural spawning of the 1943 brood did not produce the exceptional developments of 1947.

TABLE 2

PERTINENT DATA ON BLUEBACK SALMON—GRAND COULEE FISH-MAINTENANCE PROJECT—U. S. FISH AND WILDLIFE SERVICE

Year	Rock Island Count	ESTIMATED DISTRIBUTION OF FISH		Progeny Released from Hatcheries as Fingerlings
		Natural Spawning	Artificial Propagation	
1933	40,737	40,737	0	.. ..
1934	2,227	2,227	0	....
1935	14,013	14,013	0	....
1936	16,501	16,501	0	....
1937	15,087	15,087	0	......
1938	17,123	17,123	0	....
1939	19,591	19,591	0	....
1940	26,894	9,770	17,124	1,008,312
1941	949	78	871	12,459
1942	15,782	10,853	4,929	930,111
1943	17,665	13,254	4,144	2,629,767
1944	4,932	4,764	168	483,305
1945	7,120	6,573	547	721,525
1946	46,563	42,563	4,000	1,295,000[1]
1947	79,489	......	.....	....

[1] On hand, August 1947.



Courtesy of the U. S. Bureau of Reclamation

FERTILIZED SALMON EGGS
BEING PLACED IN A TROUGH FOR INCUBATION.

Another source of the phenomenal 1947 runs lies in the 4,144 adult bluebacks of the 1943 run that were spawned artificially in the Grand Coulee hatchery operations. Progeny of these fish—2,630,000 in number—were reared and released from the Grand Coulee hatcheries. These fingerlings may well have produced the run that was not approached by 40,737 fish spawning naturally in the entire upper Columbia ten years before.

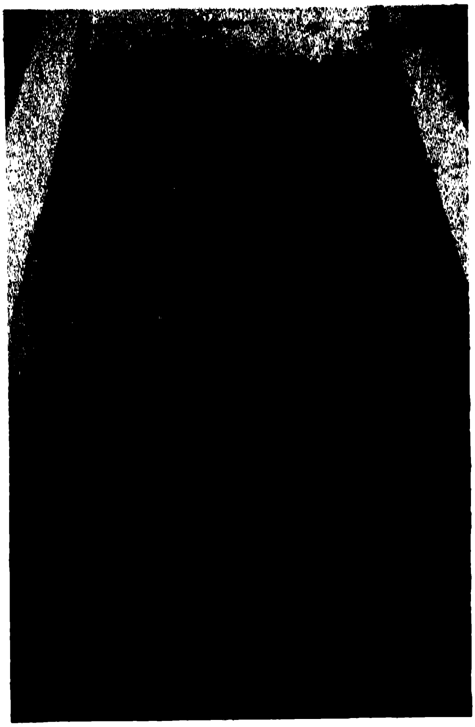
Following this thesis, a glance at the last column in Table 2 and the question immediately arises: If 2,600,000 fingerlings of the 1943 brood produced such a large returning run in 1947, why did not the 1,008,000 fingerlings of the 1940 brood produce a run approximately one-half that size in 1944? Familiarity with the history of the Grand Coulee hatcheries will effectively answer that question.

When operations were undertaken at the Grand Coulee hatcheries with the 1940 brood, the techniques used in ordinary short-term rearing constituted the sum of available knowledge about salmon culture. The inadequacy of fish-cultural knowledge became glaringly apparent during the first summer of operations. The diets ordinarily fed at salmon hatcheries proved nutritionally deficient upon prolonged feeding, and the fingerlings literally starved in spite of the enormous quantities of food being fed. In July 1941, I found nine different diseases—not including the nutritional deficiency—raging concurrently in the Leavenworth hatchery rearing ponds, with no effective control measures available for any. Under these circumstances, efficient artificial propagation could not be expected—certainly it was not obtained. Of the 6,065,800 blueback eggs collected from the 1940 runs, only 1,008,312 (or but 16.6 percent of the eggs collected) survived to be released as fingerlings. The remaining 83.4 percent of the egg collections were sacrificed upon the altar of fish-cultural ignorance.

An integrated program of laboratory research was instituted under my direction in July 1941, with the sole objective of determining how salmon could be efficiently propagated artificially. More than 100 diets were subjected to controlled tests, and finally one was compounded that has met all requirements to date. Fish-cultural techniques, which in themselves may cause severe losses of stock, were critically evaluated, and new and efficient methods were developed to replace those found wanting.

The problems offered by infectious diseases required developing an entirely new method of treatment. Formerly, hatchery disease treatments consisted in laboriously seining all fish from the rearing ponds and immersing them in a disinfecting bath. Too often, such handling killed almost as many fish as did the disease being treated. A method eventually was developed whereby a disinfectant could be added directly to the pond—thus not only treating the fish without disturbance, but sterilizing the pond at the same time. New disinfectants were required for this method of treatment, as those commonly used were found to kill fish before affecting the microorganisms. Dozens of potential disinfectants were tested, both from the standpoint of toxicity to fish life and their ability to kill bacteria. Finally, one was found that eliminated all infectious diseases at the Grand Coulee hatcheries. This disinfectant was not discovered until August 1943, and it was applied for the first time to fingerlings of the 1942 brood. Unfortunately, the armed forces were taking virtually the entire output of this disinfectant at the time, and little could be spared for treating fish. As a consequence, less than one-half of the 930,000 fingerlings of the 1942 brood could be treated as desired. It is believed, however, that the treated fish—stunted as they were by the earlier ravages of disease, but otherwise healthy at the time of liberation—accounted for the upswing in the blueback runs returning in 1946.

By the time the 1943-brood fingerlings came along, the disinfectant was available in ample quantities, and the fish were routinely treated from the time they started to feed until they were finally liberated. Diseases were continuously eliminated long before they could gain sufficient foothold to affect the fish.



Courtesy of the U. S. Bureau of Reclamation

SALMON FINGERLINGS

AT THE HATCHERY THESE FISH ARE PROTECTED FROM THEIR NATURAL ENEMIES UNTIL THE TIME OF THEIR MIGRATION TO THE OCEAN.

The results following elimination of infectious diseases, coupled with feeding a nutritionally adequate diet, were apparent in the exceptional vigor of the fish. As further evidence, 3,476,192 eggs had been collected from the 1943 brood stock, and 2,910,421 feeding fingerlings were produced. (The 16.3 percent loss on eggs and fry resulted largely from lack of methods for safely transporting recently collected eggs over rough roads—a problem that subsequently has been solved.) Of the 2,910,421 feeding fingerlings produced, 2,629,767 finally were released—a loss of but 10 percent during the fingerling stage. Even the over-all loss of 24.4 percent on the 1943-brood fingerlings constituted a great improvement over the 83.4 percent loss experienced with the 1940 brood. I well remember an impromptu remark by Mr. Joe Kemmerich, dean of the West Coast salmon-culturists and, at the time, district supervisor of the Grand Coulee stations: "I've been in this game for over 30 years and I never saw fish like these before." The reason for Mr. Kemmerich's statement about the 1943-brood fingerlings contains the probable answer not only to the phenomenal reappearance of the Columbia River bluebacks in 1947, but to the future of the Columbia River salmon fisheries as well.

Since early spring of 1943, the Grand Coulee stations have been turning out fingerling fish that are no mill-run hatchery product. All fish are under constant surveillance, with trained resident biologists checking for abnormal developments and immediately tracing any found to their source. The diets are prepared and fed according to exacting standards—even to the point of using a thermometer to ascertain that the food is of proper temperature to "hold its bind" and thus prevent leaching of the essential water-soluble fractions. All fish are fed a definite proportion of their body weight—depending upon prevailing water temperatures and the size of the fish—to avoid the undesirable consequences of over- or underfeeding. Disease treatments continue to be applied routinely at periodic intervals as prophylactic measures. Their efficacy is well demonstrated in that each summer one pond of fingerling fish has been left untreated as a proving ground for new and untried disinfectants. Invariably, the old story of a disease epidemic starts to unfold, usually within two weeks after treatments are withheld. The laboratory research continues, although now emphasis is being placed upon a reduction in capital investment and operating costs through the development of efficient types of fish-cultural equipment. This information is being acquired against the time when another salmon hatchery will be built.

The accomplishments of the Grand Coulee hatcheries, beginning with the 1943-brood fingerlings, offers one clear-cut explanation for the exceptional blueback runs of 1947. It is not the only explanation by any means, but it appears the most probable one. The factor of coincidence cannot be overlooked, and future developments alone hold the final proof. Should this explanation prove true, artificial propagation—if properly practiced—can, in the future, adequately assume a far heavier responsibility in maintaining the Columbia River salmon resources than previous accomplishments would indicate.

As for the immediate future, if the Grand Coulee hatcheries were primarily responsible for the 1947 blueback run in the Columbia River, another run of comparable magnitude cannot be anticipated until four years after another fingerling release of a size equal to that from the 1943 brood is again made. As shown in the column of fingerling releases in Table 2, blueback runs to the Columbia up to and including 1950 should be considerably smaller than the 1947 run, for there have been no fingerling releases comparable in size to that of the 1943 brood. Lack of fish and, subsequently, lack of funds have precluded egg takes at the Grand Coulee hatcheries numerically approximating that collected from the 1943 runs. Granted both funds and fish, presumably runs comparable to that of 1947 could be maintained indefinitely as long as conditions in the Columbia River remain essentially unchanged.

IN CLOSING, one word of caution appears in order. The construction of McNary Dam obviously precludes maintaining the *status quo* of the Columbia Basin. The effect of this structure upon the salmon resources will remain conjectural until the project has been completed. The proposal has been advanced—more or less in the nature of insurance—that a sizable fraction of the salmon runs now passing the McNary Dam site be relocated downstream into areas below most of the proposed main-stem development. In the case of blueback, relocation to the lower river would necessitate virtually complete dependence upon artificial propagation. Overoptimism toward the potentialities of artificial propagation would be dangerous at this time—even should my hypothesis that the Grand Coulee hatcheries were primarily responsible for the 1947 blueback runs ultimately be proved correct. Even the proved fact that artificial propagation at the Grand Coulee hatcheries did maintain a commercially exploited blueback run would not offer positive proof that the feat could be repeated in the lower river. The accomplishments attributed to the Grand Coulee hatcheries—as well as actual experimental relocations of upper-river bluebacks to a lower-river hatchery, which are producing comparable returns this year—must be accepted as potentially promising experiments at best. Certainly, the 1947 blueback runs should not be interpreted as proof that both bluebacks and dams can exist in the Columbia Basin.

ORIGIN OF THE MIMA MOUNDS*

TROY L. PÉWÉ

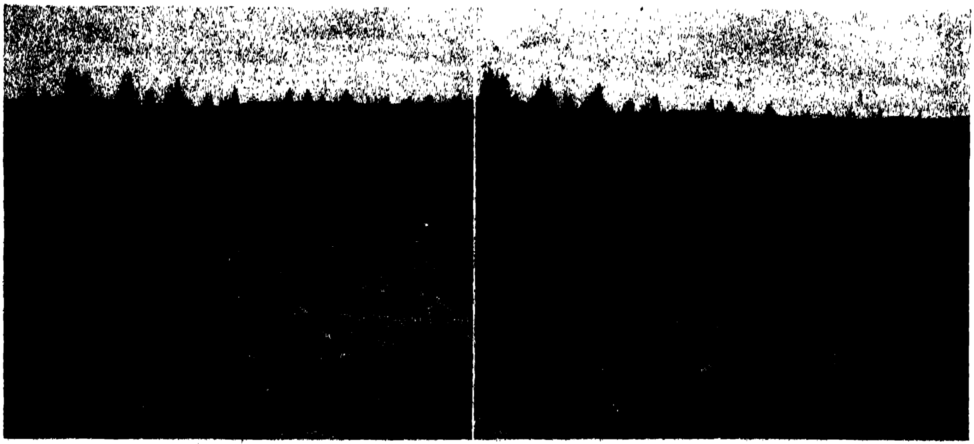
After taking his Master's degree in geology at the University of Iowa in 1942, Mr. Péwé became an instructor in that science at Augustana College. He remained there until 1946, when he joined the U.S. Geological Survey Permafrost Division, spending both summers since that time in Alaska: 1946 in the Yukon River Valley and 1947 in the Fairbanks area. During the winter Mr. Péwé is at Stanford University, assembling the facts gathered in the summer.

IN A recent paper in THE SCIENTIFIC MONTHLY (1947) and an earlier one in the *Journal of Geology* (1942), Dr. V. B. Scheffer discusses the origin of the large earth mounds on the prairies near the south tip of Puget Sound, Washington. He attributes these mounds to the work of gophers. This is, he states, only one of the many theories suggested for the origin of the mounds.

I have never seen the Mima Mounds but, after comparing descriptions and photographs of these mounds with mounds of a similar nature in the Fairbanks area, Alaska, I am of the opinion that the Mima Mounds may be periglacial phenomena. That these mounds may represent periglacial activity was earlier recognized by H. M. Eakin (1932), a geologist familiar with subarctic phenomena.

The mounds in the Fairbanks area were studied by me during the summer of 1947 in connection with the investigation of the U. S. Geological Survey on the distribution, characteristics, and genesis of permanently frozen ground (the present permafrost program). These mounds will be discussed in a forthcoming paper entitled "The Relation of Permafrost to Agriculture in the Fairbanks Area, Alaska." The Alaska mounds are in silts and fine sands. They vary in diameter from 10 to 30 feet and are 1 to 8 feet in height. These mounds are best developed in areas cleared for agriculture on the low slopes of the hills or on the flat-lying land between the hills and the Chena River floodplain. No mounds were noted on the floodplain.

Large masses of clear ice are very common



Photograph by T. Péwé, August 1947

FIG. 1. STEREOPAIR OF DEPRESSIONS FORMED BY MELTING ICE

IN THIS FIELD, ONE-HALF MILE NORTH OF THE U. S. DEPARTMENT OF AGRICULTURE EXPERIMENT STATION NEAR FAIRBANKS, ALASKA, DISCONNECTED DEPRESSIONS WERE FORMED WHEN UNDERLYING ICE MASSES MELTED.

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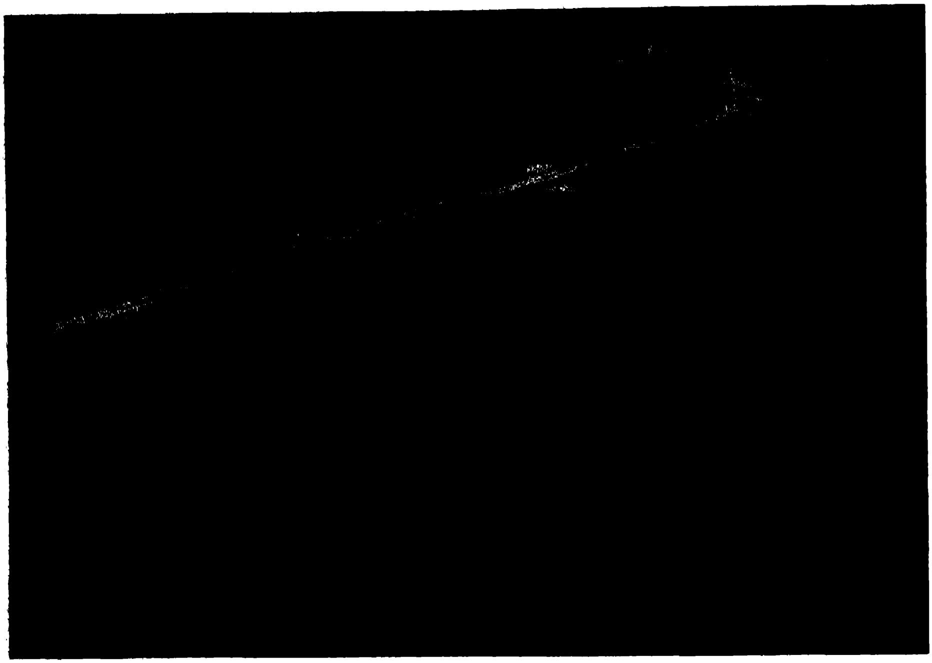
* Published by permission of the director, U. S. Geological Survey. The author wishes to extend his thanks to Mr. R. F. Black and Mr. A. I. Levorsen for calling his attention to the article on the Mima Mounds by Dr. Scheffer; and to Mr. S. W. Muller and Mr. E. Blackwelder, who kindly reviewed the manuscript. Mr. Blackwelder also discussed with the author the mounds of similar nature that occur in California.



Photograph by T. Péwé, August 1947

FIG. 2. MOUNDS NEAR THE UNIVERSITY OF ALASKA, FAIRBANKS

Photograph by T. Péwé, July 1947

FIG. 3. CLOSE-UP OF A MOUND NEAR FAIRBANKS

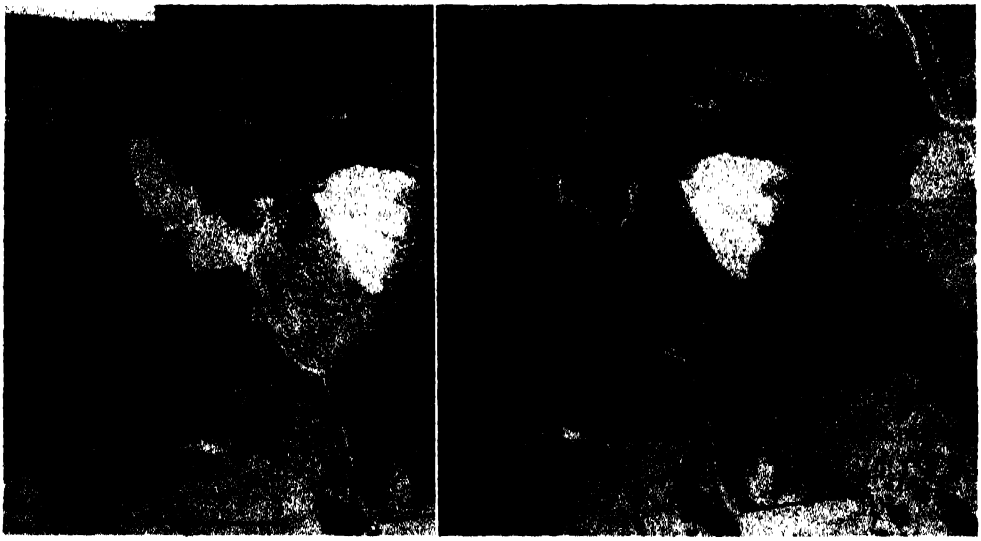
in permanently frozen ground in various regions of the world. Some ice masses occur in polygonal nets, as can be observed today in placer gold-mining operations in the Fairbanks area. Such ice masses also have been encountered by drilling. This common phenomenon has been reported (Leffingwell, 1919, 205; Taber, 1943, 1510; and Black, oral communication, August 23, 1947) from other parts of Alaska.

I believe the Alaska mounds are the result of the melting of a network of ice wedges in the ground. When the ice begins to melt, the overlying ground sinks down. Disconnected depressions in some places appear first (Fig. 1), and, if the ice is in a polygonal, or network, pattern, the ground in the center of the ice network is soon left standing in relief. Drilling into the low areas between mounds has revealed the presence of clear ice. The development of the mounds in the Fairbanks area, which has a mean annual temperature of about 26° F., seems to be dependent upon stripping of the protective vegetative cover.

Figure 2 is a low oblique aerial view of the mounds developed near the University of Alaska, three miles west of Fairbanks. The forested area in the foreground and to the right consists of trees eight to ten years old which have grown on the mounds. The diameters of the mounds are from 10 to 30 feet.

Figure 3 is a close-up of the mounds in Figure 2. The relief of these mounds is about 6 feet. Figure 4 is a stereopair of vertical aerial photographs of the same mounds as those in Figures 2 and 3. W. A. Rockie (1942) studied this field in 1938 and assumed the mounds were caused by melting of "ground ice."

In 1940 R. C. Newcomb presented a theory for the Mima Mounds which assumed the formation of ice wedges by water entering and freezing in cracks formed when the ground contracted in cold weather. The expansion of the freezing water causes bulging in the center of the polygon. He states that when the ice melts out, the interice blocks slump down into mounds. The fact that ice wedges do form in cracks in a polygonal network has been reported from the arctic coastal plains of Alaska by Leffingwell (1919, 205) and Black (oral communication, August 23, 1947). The mounds in the Fairbanks area, however, appear to develop from



Photograph by U. S. Army Air Forces, 1946

FIG. 4. STEREOPAIR OF AERIAL PHOTOGRAPHS OF MOUNDS NEAR FAIRBANKS

THE MOUNDS SHOWN IN FIGURES 2 AND 3 ARE IN THE CENTRAL FIELD OF THIS PICTURE. MOUNDS NOT SO WELL DEVELOPED OCCUR IN THE FIELD TO THE LEFT. THE BLACK BAR ON THE LEFT REPRESENTS 1,000 FEET.

melting of the surrounding ice masses and not from pushing of the earth into a mound by expansion of the ice.

Mr. Newcomb has kindly allowed me to examine his unpublished manuscript entitled "Refutation of the 'Gopher Origin' for the Mima Mounds, Thurston County Region, Washington." In this thorough work it is revealed that not all the mounds are smooth and round; some are polygonal and of various sizes and stages of development. The Alaska mounds are very similar in this respect. One feature brought out by Mr. Newcomb's paper is the existence of "prong depressions." A photograph of these Y-shaped depressions compares exactly with the polygonal trenches common in areas which contain ice wedges throughout Alaska.

Factors that seem to favor the periglacial origin of the Mima Mounds are:

1. The mounds in Washington compare favorably in appearance and development with those of the Fairbanks area, Alaska. The mounds of the Mima Prairie, which are pictured on page 285 of Dr. Scheffer's article (1947), appear very similar to those in an abandoned field on the Farmer's Loop road three miles north of Fairbanks.
2. Features that Dr. Scheffer calls "mound roots" and that appear to be armlike structures of black silt extending into the gravel, may be small fossil ice wedges. Such periglacial phenomena have been described previously (Edelman and Tavernier, 1940, 140).
3. The temperature that existed in the periglacial zone during the glacial period was probably favorable for the development of permafrost.

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REMARKS ON THE MIMA MOUNDS

The article by V. B. Scheffer on the origin of the Mima Mounds (October 1947 SM) has brought a response from readers even greater than that which followed our publication of C. Wythe Cooke's article on the origin of the Carolina bays (April 1945 SM). In present issue, the preceding and following articles, and letters on the subject in Comments and Criticisms, conclude the discussion in the SM of the origin of natural mounds. Of course the subject may be reopened later if it seems desirable to do so. In the meantime, interested readers should communicate with Dr. W. Armstrong Price, consulting geologist, 907 Medical-Professional Building, Corpus Christi, Texas. Dr. Price is critically reviewing the Mima Mound problem, on which he expects to publish elsewhere.—Ed.

THE SAND MOUNDS OF LOUISIANA AND TEXAS

FREDERICK C. KOONS

The article by Victor B. Scheffer, "The Mystery of the Mima Mounds," in the October SM elicited considerable discussion on the origin of these pimples of the earth's surface in the Pacific Coast states. We have already published in Comments and Criticisms some of the communications supporting or opposing Dr. Scheffer's conclusion that the mounds are not of geological origin but were formed by the activities of pocket gophers. The most remarkable communication relative to Dr. Scheffer's article has come from Mr. Frederick C. Koons, a retired petroleum geologist, who, at the age of sixty-three in 1926, took a Master's degree in the Department of Geology and Paleontology of the University of Chicago, presenting a thesis entitled "Origin of the Sand Mounds of the Pimpled Plains of Louisiana and Texas." Mr. Koons, writing from his home in Wharton, Texas, submitted to the editor a copy of his "ancient thesis" and gave us the privilege of using it as we wish. Regarding his thesis Mr. Koons wrote in part as follows: " . . . I thought I had proved that the mounds were made by the pocket gopher. But if Dr. Scheffer meets with as much opposition as I did, he may appreciate any support. In this old thesis he will find confirmation on every point; and I have the advantage of having seen it done." As Mr. Koons' observations and conclusions were entirely independent of those of Dr. Scheffer, having been made much earlier and in a different region of the United States, we thought a part of his thesis, published below, would be of interest to our readers.

THE small mounds that dot portions of the coastal plains of Louisiana and Texas, aptly called "the pimpled plains," have aroused speculation on their origin. Most of the observations so far reported have been superficial, and much of the evidence is merely theoretical or hearsay. When I first saw the mounds on a trip to Texas in 1903, I was told that they were caused by gas blowouts. Later my prolonged residence in Texas afforded opportunity for careful and lengthy study. It is my purpose to review briefly the literature on the subject, to state the results of my investigations on the composition of the mounds and their relation to the underlying strata, and to present my conclusions based on observations covering fifteen years.

The mounds under discussion extend from southwestern Louisiana along the coastal plain southward and westward toward Mexico. I have not observed them south of the latitude of Port Lavaca, Texas. The width of the belt of their occurrence is less than 100 miles and was about 60 miles wide where I made my study. They occur invariably on level plains having a surface soil of clay more or less mixed with sand, little or no drainage, and in regions of abundant periodical rainfall.

The surface soil of the type plain where my studies were made is about 18 inches in depth, underlaid in places by lenses of quicksand from 1 inch to 4 inches in thickness. Then comes 15 feet of clay so impervious to water that after a prolonged wet season of as much as three months' duration the clay at a depth of 3 or 4 feet will be apparently dry and present the same appearance to eye and touch that it does after an extended dry season lasting for months. This clay bed is underlaid by a sand stratum of 12 feet and that by another clay stratum of 4 feet. Under the latter is a stratum of water-bearing gravel and sand. The sand between the clay strata is dry at all times, regardless of the surface conditions. Moreover, the amount of water in the water-bearing stratum is in no way affected or controlled by local conditions of precipitation. There is no connection between the underground water and the surface.

The mounds themselves are circular, and their height varies as their diameters, but their relief is not over 24 inches. In composition they are identical with the adjacent soil, which is a clay loam with a varying percentage of sand; but often the percentage of sand is slightly greater than that of the outside area. There is no rock of any kind in the soil nor in the underlying strata until the bottom of the second clay stratum is reached. However, where there is an underlying sand lens there may be found at the bottom of the sand a few pebbles of water-worn gravel, and in some localities there is a small percentage of ironstone nodules, or "buckshot."

Generally, for each mound there is a corresponding depression at one side, and sometimes, but not often, on two sides. These depressions are oriented in no particular way, are oblong, and their width and length vary as the diameter of the mound.

Some of the mounds are pitted at or near the center, usually by one, and rarely by as many as three, small pits.

The mounds show no regularity of distribution and exhibit no trend. There is no evidence of stratification of any sort. In diameter they range from 4 to about 30 feet, but a few are larger.

[*At this point Mr. Koons discussed and rejected eight hypotheses that were suggested in previous literature to account for the origin of the mounds. They were: (1) Differential settling of sediments, (2) segregation of mineral matter, or concretion on a large scale, (3) vertical seepage of water under hydrostatic pressure, (4) gas blowouts, (5) wind drifts, (6) anthills, (7) uprooted trees, (8) man-made. We are omitting his arguments on these hypotheses and other references that he made to the literature because we are primarily interested in his own observations and conclusions.*—Ed.]

As a result of my own observations and investigations on the mounds I offer the following solution to the problem of their origin: The mounds were made by the pocket gopher, *Geomys bursarius,* to enable the builder to keep out of the water. Where there is surface or underdrainage the mounds are not made because they are not needed.

Since the pocket gopher is found on the Great Plains from the south side of the Saskatchewan to the Gulf of Mexico, it may be objected that the gopher does not build mounds on other areas that are sometimes flooded. That is true; but it is also true that both surface and underdrainage are vastly superior to that of the pimpled plains. The surface gradient of the latter is from 3 to 6 feet per mile, which to the eye is a flat surface. The soil, as before mentioned, is thin. The mean annual rainfall at Houston, Texas, is about 50 inches, about twice that of the Great Plains, and is not evenly distributed throughout the year. When the weather is wet it is apt to be very wet for a long period, and of course the dry periods may also be quite long. The underlying clay subsoil being impervious to water, as before explained, the thin stratum of topsoil soon becomes saturated and then flooded. So there is a considerable difference in this respect between the Great Plains east of the Rocky Mountains and the pimpled plains. One familiar with the Gulf coastal plain may urge the objection that not all of it, even in the pimpled plains area of Texas, is of the character described. While that is a fact, it is also true that unless it is of the type character there will be no pimples. For example, there are localities where the type soil borders on an area of heavy, "black-waxy" land, Beaumont clay, which is from 6 to 10 feet or more in depth. The line between the two is often rather sharply defined. There will be mounds on the one and not on the other. Why? The deep soil has a certain amount of underdrainage and, being deeper, much more water is required to saturate and flood it. Furthermore, few gophers will be found there. The gopher's favorite food does not grow very well on black land. Being like putty when wet and very hard when dry, it is difficult to work.

I had in mind, in mentioning the foregoing type locality bordering on black land, the southern part of Colorado County, Texas, southwest of the village of Garwood. Going west from Garwood, we traverse about a mile of black land, very sticky and very deep. It is moundless. After crossing the Mustang Creek we are suddenly out of the black land and on the pimpled plain, which is more or less sprinkled with the typical mounds. A journey of about 14 miles southwest brings us to Sandy Creek, which flows through a timber belt about 2 miles wide at that point. We are still in the type locality as to soil, except that in places the creek has built up levees. Gophers are abundant on these higher lands as well as on the plain, but there are no mounds.

The manner of constructing a mound is this: the gopher opens his burrow to the surface and piles up a mound of loose earth 2 to 3 feet in diameter and often as much as 18 inches high. He does not use this mound as a residence unless the water forces him to do so. As the plains are usually pastures and there are many cattle on them, the cattle paw and horn down the newly made mounds and scatter the fresh earth. The gophers rebuild it as occasion demands, and it is as often torn down. If the cattle do not demolish it, the weather eventually will; but I have never known the cattle to fail. Before the advent of the cattle there were buffalo, which served equally well.

The material that was piled up was not taken from immediately under the mound, but was carried from a distance through a runway excavated to one side. Usually but one runway was made in the early history of the mound. This was extended, and the excavated material pushed up into place on the dump until the carry became too long; then a branch was made from the first run and earth carried from that until the distance again became too great, whereupon another branch was started; and so on. In that way the depression, which was caused by the eventual settling of the excavated area, came to be on one side of the mound and not all around it. It was simply a borrow pit from which material was taken for the fill: i.e., the mound. In that way the mound came to be made round and the depression oblong. The gopher in his tunneling sometimes went well down to the clay stratum, and if there happened to be a sand lens there he used it, for it was easy digging. In that way it came about that he sometimes put a trifle more sand in his structure than one finds in the surrounding soil. Often he made his mound near some small, shallow lagoon or natural depression. The bulbs of certain lilies were to be had there, and they were harvested when the water evaporated, as it was sure to do in summer. Around such places also grow sedges, *Cyperus esculentus*, which furnish desirable tubers, if we may judge by the amount harvested and stored.

I have now accounted for the mounds and the depressions, but what about the small pit or pits often found near the center? They are secondary, and the gopher had nothing to do with them. We must remember that the gopher uses the mound as an island of refuge and does not generally live there. He prefers to locate his living quarters and storehouse elsewhere, ordinarily. After he has first built up his small mound, and each time thereafter when he works on it, he plugs the opening to the surface when work stops. And he plugs it so skillfully that three minutes after he has finished one may cut a section through it with a spade and find it very hard to determine which part had been hole and which undisturbed soil. But he has neighbors, and when after some years of labor a fair-sized mound has been built up, it oftens happens that some skunk, civet cat, or mink decides that the mound is a nice, high site for a residence and makes a burrow. The newcomer may, and often does, strike the gopher's tunnel, which is enlarged to meet the needs of the invader. But the new burrow, unlike the gopher's, is kept open, since the occupant goes out and in nightly. It is this opening, enlarged by dogs or other animals digging at the entrance or by cattle stepping on the earth just over it and caving it in, that causes the small pits.

To prove the gopher hypothesis, I noted the initiation of mounds and made observations on their progress for some years. Two were noted about three miles apart on trails that I frequently traveled, and their locations marked for identification. They were observed shortly after the initial mounds were thrown up and were kept under observation some years. After five years both had attained fair size and would readily have been classed by the casual observer as "sand mounds." One of them was abandoned when it was included in a rice field and partly plowed down and later flooded by irrigation.

I have never observed a new mound in an old rice field, no matter how long it may have been abandoned for rice culture, provided the check levees had not been plowed down. The gophers appropriated the levees and, not needing mounds, did not build any.

If this solution of the origin of the mounds in question be the true one, then a geological problem has been solved only to raise a biological one. Dare we charge an animal with such a radical change of life habits to meet special environmental needs? Does that noted hydraulic engineer, the beaver, neglect to build a dam where there is plenty of water? How did he become a dam builder in the first place? May not another rodent become an engineer?

I have been over a good deal of the Great Plains region from the Dakotas to the Gulf; the pocket gopher is found more or less in the entire territory, but nowhere except on the pimpled plains have I noted any such work as I have herein attributed to it.

In well-drained regions where gophers are present, they are evidenced by many small earth heaps, which are waste dumps for material excavated in making runways, but these are not enlarged and are merely byproducts in the construction of tunnels; whereas on the pimpled plains it would seem that the mound is more than that and that tunneling is sometimes carried on for the purpose of borrowing material from one place for use in another.

Obviously, we must call in expert evidence in the case and, since we have chased our geological constructor into a biological hole, we must consult the biologist.

The biologist says yes in answer to the foregoing questions and presents some evidence in support. The ground squirrel of the Columbia plateau occupies a region of abundant food, but water fails in midsummer. So he simply digs deep, shuts himself up, and goes to sleep until the following spring, according to Shaw. This habit is a local one. Allee, of the University of Chicago, cites the case of the blue heron, which in the forest regions of the East nests in tall trees, but in the treeless wastes of the Great Basin builds on the ground a structure of reeds and mud 4 or 5 feet high in order to place the nest above the water—an entire change of habit. Osgood, of the Field Museum [now the Chicago Natural History Museum], answered the questions in the affirmative and cited the case of the fire ant. On the flood plain of the Amazon thousands of these ants assemble into a compact ball that floats until the flood subsides. The outer individuals drown, but the queen in the center and the inner ones survive. Thus many animals adapt themselves to unusual environments.

MY OBSERVATIONS may be summarized as follows:

Mounds of the type under discussion are found only on flat, undrained regions that are subject to periodic floods of considerable duration.

Dissection of more than one hundred failed to show any structure whatever, any variation of composition (which is that of the surrounding soil and the latter in turn is formed by disintegration and decomposition of the clay), any disturbance of the fine-textured, very compact underlying 15 feet of clay, or any disturbance of either clay or the 12 feet of dry sand below the clay in three instances where pits 8 feet in diameter were located on mounds and excavated through both clay and sand to the water level in the underlying gravel.

The observation of the habits of the pocket gopher led to the marking of certain initial mounds, keeping check on their manner and rate of growth, and the certainty, in my opinion, of the method of their construction.

THE MEASUREMENT OF MENTAL SYSTEMS

(CAN INTELLIGENCE BE MEASURED?)

W. ALLISON DAVIS and ROBERT J. HAVIGHURST

Dr. Davis (Ph.D., Chicago), associate professor of education at his alma mater, has spent a number of years in social science research. A two-year survey of community organization in the South resulted in his coauthorship of Deep South. *During 1938–39 he directed a study of the personality development of Negro adolescents* in *Southern cities, under the sponsorship of the American Youth Commission. The results were published under the title* Children of Bondage. *Dr. Havighurst has been professor of education at the University of Chicago since 1941. After taking his Ph.D. in chemistry at Ohio State, he spent some years in teaching, later becoming interested in experimental education. He is joint author of two recent books,* Who Shall Be Educated *and* Father of the Man. *Last summer Dr. Havighurst was in Paris as a staff member of the UNESCO Seminar on Education, and in the fall he made a study of social and educational conditions in Germany for the Rockefeller Foundation.*

THE crucial problem raised by the attempt to compare scientifically the capacity of any two individuals to learn is that of finding situations with which the two individuals have had equal experience. To state this issue more exactly, two major systems of behavior are involved in problem-solving. They are (*a*) the individual's genetic equipment for problem solving; and (*b*) the individual's particular cultural experience, training, and motivation, which have developed certain areas of his mental behavior and certain skills more than others. In a test of general hereditary capacity, the second factor must be equalized for all those tested.

Consider two children with equal hereditary mental factors. The child who obtains more practice in working on mental problems of a certain type will prove superior in that particular area of mental functioning. If one of these equally endowed children is reared in a family of high socioeconomic status, and the other in a slum family, we should expect that the first child will prove superior on academic types of problems, such as nearly all those in "intelligence" tests. This actually proves to be the case, as Newman, Freeman, and Holzinger have shown with identical twins, separated early and reared in different socioeconomic strata.

But if one considers any two children, not genetically identical—and this is the chief task of mental tests—the fact that one child proves superior within a certain narrow range of academic problems does not imply that he is superior likewise in solving more realistic, lifelike problems. Mental processes are highly varied; so are the types of specific mental problems presented by our society. Thus problem-solving activities in man exist as a configuration of systems of activities. In one child, systems A and B may exist at a high level, but systems C and D may exist at a low level, as a result both of genetic factors and of training. A second child may have this pattern of strong and weak activities exactly reversed.

Any scientific effort to compare the mental status of individuals throughout the whole cultural range of the population of the United States, therefore, must satisfy two conditions. Unfortunately these requirements have not been met by the makers of intelligence tests or tests of "mental ability."

The two requirements for such a test may be stated thus:

Since, in life, human beings engage in an incredible variety of mental activities, the testmaker must discover those systems of acts that are most representative of all mental behaviors. In practical terms of testmaking, this requirement means that those types of problems that are put into tests must represent the wide range of mental systems exhibited by normal human beings (and not merely a sampling of their academic or linguistic activities).

In each mental-system area, the testmaker must select problems common to the culture and practice of all socioeconomic groups in the population to be tested. In addition, the testmaker must learn how to express these problems in symbols common to all the socioeconomic groups to be tested. He must likewise find problems that motivate equally all the groups to be tested.

Although Alfred Binet, who founded modern tests of intelligence, was keenly aware of the danger that his test problems were too scholastic and too closely related to home training, he did not live to face the extremely complex task of creating new tests that would be less biased in these respects. During the past twenty-five years, therefore, many research psychologists have criticized all such intelligence tests as including a strong cultural handicap for pupils of the lower socioeconomic groups.

There has been little serious consideration given, however, to the first criterion mentioned above. In the thirty-five years since Binet's last work, virtually no new types of problems have been included in either the individual or group intelligence tests. The few recent additions have merely extended the old, abstract types of problems, which are dear to the heart of academicians, but seldom or never met by the individual as basic, real-life problems.

We regard mental behavior as the interaction of many systems of mental acts. The Spearman theory considers intelligence as composed of one general (or *G*) "ability" and several specific "abilities." We raise the question whether the testee will have a fair chance, however, to show how much of *G* he possesses unless he has had equal access, with all other testees, to the situations and symbols by which he is tested, and unless he is equally as much motivated as all other testees.

Thurstone's multifactor theory of intelligence supposes that there is a rather small number of independent primary abilities. The necessity for culturally common situations, symbols, and motivation would apply to Thurstone's tests of Primary Mental Abilities also.

Our chief criterion for a measure of general mental behavior is that the problems must represent a wide range of mental systems and mental problems. We do not believe that the present tests meet this criterion. Thurstone isolated his primary abilities by analysis of fifty-six "current psychological tests," constructed by many persons, but all limited, we believe, to a few systems of mental behavior, and formulated in academic abstract problems. Thurstone quite frankly makes no claim that his tests cover a wide range of "mental factors" or problems. F. L. Goodenough has made the same point: "Thurstone's primary abilities are not presumed to cover the complete range of human talent—they involve only the abstract abilities needed for success in the conventional types of intelligence tests and in the academic pursuits of the classroom."

Very recently, Thurstone himself has stated that his "primary mental factors" may vary in their definition. Some may "be defined in endocrinological effects," others in effects of the central or autonomic nervous systems; "still others may be defined in terms of experience and schooling."

Authorities like Ralph W. Tyler, former director of Examinations Staff, U. S. Armed Forces Institute, and Otto Klineberg, of Columbia University, have long emphasized the defects in the present tests of general intelligence. Used widely as a basis for segregating, and discriminating educationally against the lower socioeconomic groups, the tests have levied an irreparable toll upon a whole generation of children from these groups. Tyler has attributed the inadequacies of both the tests and public education to the same blind spot: an overacademic view of human learning. One of the foremost authorities on test construction, Tyler has said:

> The public schools, as well as the standard intelligence tests, fail to tap many vital aspects of intelligence. Both the tests and the schools are caught in a circular kind of educational philosophy.

They assume that only certain kinds of problem-solving activities are diagnostic of "intelligence." Then the schools, supported by the test results, conclude that only those pupils who rank high on this limited, highly specialized range of activities, are "intelligent." This is a narrow, ritualistic concept of mental behavior which causes our communities and our industries a tremendous loss, owing to the failure of the schools to uncover and to train many other kinds of mental activities in all our children. The true learning-potential of the millions of children in our lowest socio-economic groups is not yet known. The efforts of the schools to develop this potential are weak and inept.

The sociologist and social anthropologist have been convinced, through studies of a great many human societies, that cultural learning runs through nearly all the "mental" behavior of human beings. Social anthropologists therefore strongly doubt that cultural behavior can be eliminated from any intelligence-test response.

We hope, however, that cultural bias in the tests, favoring any socioeconomic group, can be eliminated. This position requires the construction both of test problems and of total testing situations that so control all the major cultural elements in these tests that no cultural advantage is offered to any socioeconomic group in the United States.

Cultural systems in the United States: Fortunately, sociologists and social anthropologists now have developed a typology of the cultural systems within the United States. We know that Americans exhibit three major types of cultural behavior. These are:

a) The common American cultural traits and behaviors.
b) The various cultural behaviors of the different socioeconomic (social-class) groups.
c) The cultural patterns of the different "ethnic" or nationality groups.

The culture of the United States, in which the test problems inevitably must be expressed, is therefore composed of (*a*) common problems and behaviors and (*b*) limited, "subcultural" traits or problems. Here, then, in this discrimination between common cultural problems and more limited subculture-group problems, lies the way to improve our tests of general problem-solving activities. We must try to control or equalize the cultural factors in test problems if we hope to use these problems to measure comparative ability (that is, mental activity as developed in similar learning environments). One way to attack this problem would be to try to avoid all types of problems in which one socioeconomic group (and this means, in present test practice, the middle-class group) has had more training and experience than some other group. We might choose problem situations that are equally general in all our socioeconomic levels of pupils. Second, we might express these problems in symbols that are equally common, and in a manner that is equally motivating, to all socioeconomic groups.

Our position with regard to the present tests may be summarized in a series of four interdependent hypotheses:

1. All responses to all items in all tests of general intelligence are necessarily and inevitably influenced by the culture of the respondent.
2. In a test of general mental ability to be used in the United States, the problems should be selected from the common culture, expressed in cultural symbols common to all native inhabitants of the United States, and selected from that common culture only.
3. In all available tests of general intelligence, however, there are numerous items implying experience that is part of the culture of the higher socioeconomic groups, but not equally a part of the culture of the approximately 60 percent of all Americans who grow up in the lower socioeconomic groups.
4. Therefore, the basic cultural flaws in all available tests of general intelligence may be overcome by including only those problems and symbols that imply experience that is part of the general American culture.

Cultural influences in intelligence tests: The University of Chicago research: Starting with these very general hypotheses, the senior author of this paper originated a large-scale research dealing with "Cultural Influences in Intelligence Tests." The research has been directed by the present authors, with substantial aid from Ralph W. Tyler, W. Lloyd Warner, Virgil E. Herrick, Ernest A. Haggard, Walter I. Murray, and Kenneth Eells. Work was begun in 1945 and will continue for several years. Stated briefly, the aims of this research are:

1. To measure the relative success attained by different socioeconomic groups of pupils

on the specific questions ("items") in eight of the most widely used group tests of general intelligence. This part of the research enables us to identify those test questions that prove relatively easy or difficult for pupils of each socioeconomic stratum in the population. For this purpose, in a Midwestern city of about 115,000 people, all children of ages nine, ten, thirteen and fourteen were tested. To define the socioeconomic status of each pupil's family, we have used a quantitative index recently developed and validated by Warner and Eells. This Index of Status Characteristics correlates highly (over 0.8) with the social-class position of families in Midwestern cities.

TABLE 1

DISCRIMINATION BETWEEN SOCIOECONOMIC GROUPS

TEST	NUMBER OF PUPILS		PROPORTION OF ITEMS SHOWING SOCIOECONOMIC DIFFERENTIAL (PERCENTAGE)
	High Socioeconomic Group	Low Socioeconomic Group	
Given to nine- and ten-year-old pupils:			
Henmon-Nelson	226	322	93
Otis Alpha (nonverbal)	223	316	46
Otis Alpha (verbal)	223	326	70
Kuhlmann-Anderson (Grade III)	225	327	56
Kuhlmann-Anderson (Grade VI)	225	321	85
Given to thirteen- and fourteen-year-old pupils:			
Terman-McNemar	233	361	100
Otis Beta	235	364	91
California Mental Maturity	235	352	69
Thurstone Spatial	235	352	84
Thurstone Reasoning	232	358	100

The results of our item analysis, carried out by Eells, of eight standard, paper-and-pencil group tests of intelligence will be published later in a monograph. For our purpose here, it is necessary only to report the following results: We discovered that a large proportion of the items in each of these tests "discriminated between" children from the highest and lowest socioeconomic levels. In several tests, the proportion on such items was overwhelming; for instance, in the very popular Otis Beta test, 73 of the 80 items on the test showed statistically highly significant differences between the performances of children from the two levels (Table 1).

It will be seen that on only one of the tests did less than half the items show significant discrimination between the two socioeconomic groups, and that on four of the tests, more than 90 percent of the items showed such discrimination. Moreover, the proportion of items discriminating between socioeconomic groups is probably considerably higher in the Otis Alpha tests and in the Kuhlmann-Anderson (Grade III) than our sample reveals. These tests were standardized for grades (and therefore for age groups) that are lower than the mean grade placement and mean age of our sample of testees. We should expect, therefore, that many of the items would be too easy for our sample; such items would not allow the higher socioeconomic group to demonstrate the superiority over the lower in this type of problem. The figures for the California Mental Maturity test are based on only four of the six subtests. Two of the subtests were omitted because the test was too long to give in full in the time available.

The figures indicate the proportion of items that showed a difference between the responses of the two socioeconomic groups large enough to be significant, but they do not show the actual amount of discrimination between the two groups. In order to study the actual amount of discrimination, the percentage of pupils in each socioeconomic group answering the item correctly was converted to an index based on the normal curve. The socioeconomic discrimination of the item was then computed in terms of this index (Table 2).

TABLE 2

Average Amounts of Socioeconomic Difference

Test	Average Difference between Index for High Socioeconomic Pupils and That for Low Socioeconomic Pupils
Given to nine- and ten-year-old pupils:	
Henmon-Nelson	14.1
Kuhlmann-Anderson	10.3
Otis Alpha (verbal)	6.9
Otis Alpha (nonverbal)	5.9
Given to thirteen- and fourteen-year-old pupils:	
Terman-McNemar	16.4
Thurstone Reasoning	15.2
Otis Beta	14.1
California Mental Maturity	9.9
Thurstone Spatial	9.8

These figures show that the actual amount of difference between the two socioeconomic groups was much greater on certain tests than on others. In general, the tests for the older pupils showed larger differences, although the California Mental Maturity and the Thurstone Spatial tests indicated differences smaller than those found for the younger pupils by the Henmon-Nelson and Terman-McNemar tests. Since there is such wide variation, for the same ages, in the amount of difference between the high and low socioeconomic groups, depending upon what test is used, it is apparent that at least part of the difference must be due to the nature of the material in the tests themselves.

By means of this same index, it is possible to describe any item as showing a "small," "medium," or "large" amount of difference between the high and low socioeconomic groups. Although the definitions of these terms are somewhat arbitrary, they will serve to illustrate the variation in kinds of items in the different tests.

Figure 1 shows the proportion of items with "small," "medium," or "large" differences found in the various tests. It will be seen at once that there is marked variation in the different tests. Nearly half the Henmon-Nelson items, for example, show differences between the socioeconomic groups that are classified as "large," whereas only 3 percent of the Otis Alpha nonverbal items show "large" differences. One-fifth of the items in the Henmon-Nelson show "small" differences, whereas more than four-fifths of the Otis Alpha nonverbal items show this relatively "small" difference. Among the tests for the older pupils, there are likewise marked differences from test to test. In both the Terman-McNemar and the Thurstone Reasoning tests, approximately two-thirds of the items show "large" differences, whereas only one-quarter of the California Mental Maturity items—and only 6 percent of the Thurstone Spatial items—show "large" differences.

This difference between the proportion of each group answering the questions correctly is what we refer to as a socioeconomic differential. It reflects the extent to which pupils from the high groups excel pupils from the low groups on each individual item. The wide variation in the amount of socioeconomic difference found may be illustrated by citing two individual items (see Fig. 2). In the first case, 74 percent of the pupils in the high socioeconomic group answered the item correctly, but only 28 percent of the pupils in the low group did so—a difference of 46 percentage points. In the second case, the percentage answering the item correctly was practically identical for both socioeconomic groups, so that there was no statistically significant difference. The content of these two items suggests the reason for the great variation in their socioeconomic differential. The first item requires the pupil to be familiar with the term "sonata"—clearly a word that will be more often heard in a home in the high socioeconomic brackets than in a family from the low socioeconomic group. The second item, on the contrary, requires the pupil to apply the concept of a "cutting tool" so as to distinguish between this type and several other types of implements common to all socioeconomic groups.

The first major part of our research identified, therefore, those specific test questions that have proved relatively most difficult for pupils of the lowest socioeconomic group. In doing so, it demonstrated that the standards used for selecting items in present tests have resulted in a very high degree of dis-

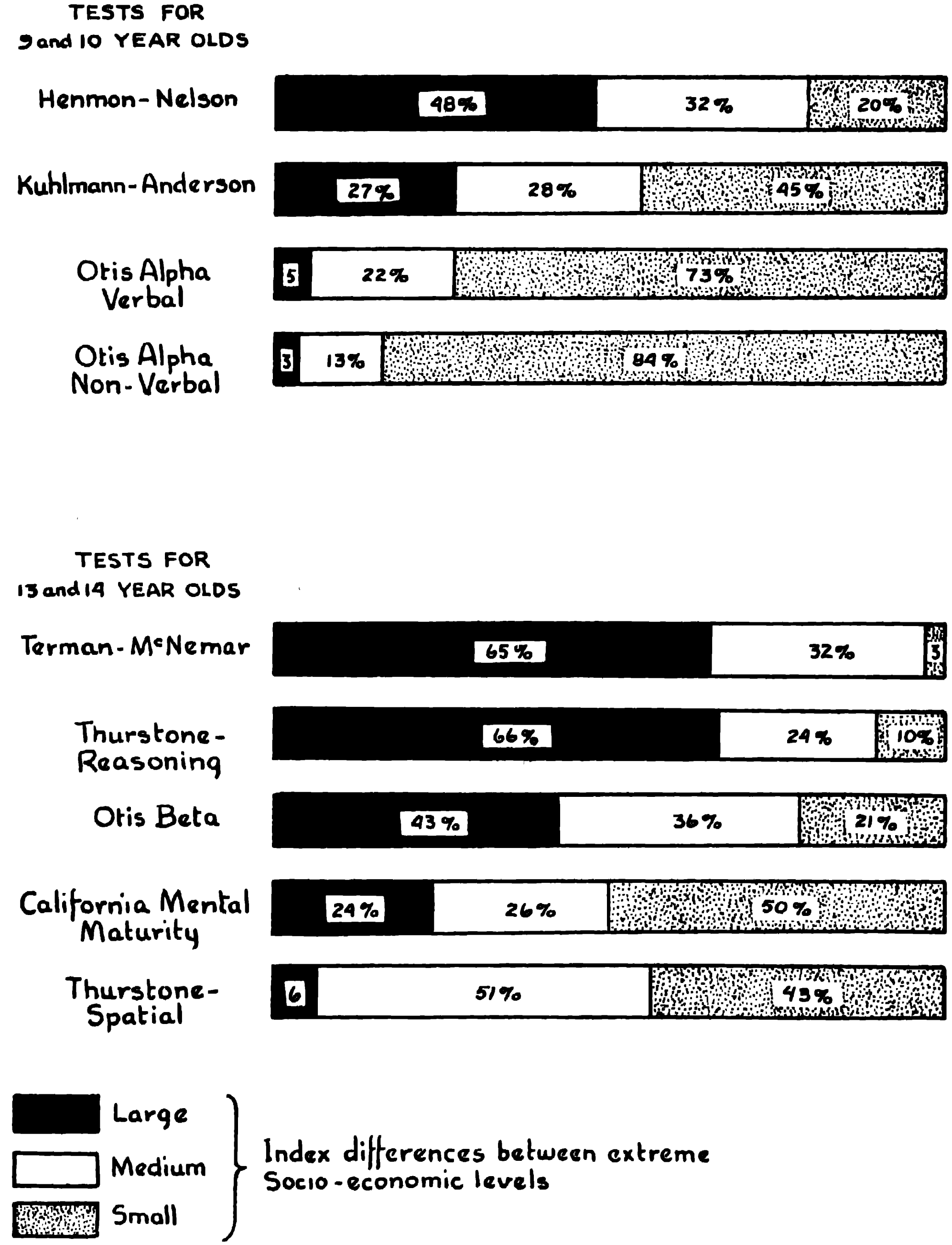

FIG. 1. DIFFERENCES IN RESPONSE TO VARIOUS INTELLIGENCE TESTS

THE LARGER THE BLACK AREA, THE GREATER THE DIFFERENCE IN RESPONSE OF CHILDREN FROM HIGH AND LOW SOCIOECONOMIC LEVELS TO THE PARTICULAR TEST. THE DEFINITION OF SIZE OF DIFFERENCE IS ARBITRARY.

crimination between socioeconomic groups.

2. Furthermore, an analysis was made independently by Walter I. Murray and Kenneth Eells of the relative performance of the highest and lowest socioeconomic groups on different categories, or types, of problems in these tests. Murray and Eells did not agree in their definitions of the types of problems, but each classified the problems upon the basis of the chief mental process that seemed to be involved in the questions, such as "classification of concepts," "recognition of opposites," "recognition of analogies," "syllogistic reasoning," etc. Both researchers found that there was no type of mental problem on these tests that did not show a significant differential in favor of the highest socioeconomic group.

We interpret this finding to mean that the present intelligence tests really measure a very narrow range of mental activities—the range of academic problems. This characteristic of the tests has been recognized by many other researchers. Indeed, the test-makers themselves have used a method of "validating" their selection of problems that insists upon high correlation with scholastic skills.

3. The crux of our problem, however, was to discover why the standard test items proved far more difficult for pupils of the lowest socioeconomic group. To answer this question, we have conducted a large-scale experiment with pupils of ages nine to ten, to measure the relative importance of various cultural influences as between the high and low socioeconomic groups. This central experiment, designed to test the basic hypotheses underlying all the various parts of our research, will be described later.

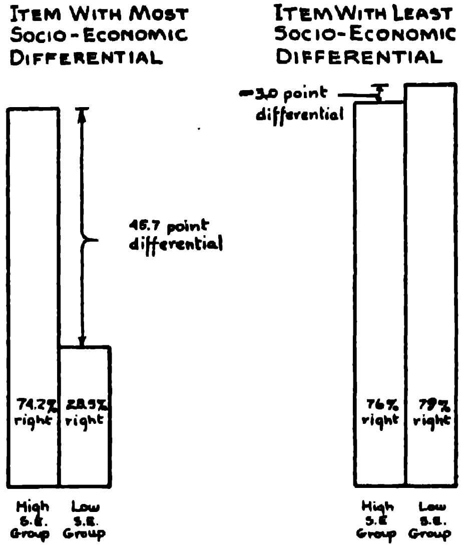

FIG. 2. SONATA VS. CUTTING TOOL

THE HIGH-LEVEL GROUP WAS FAMILIAR WITH "SONATA," WHEREAS THE LOW- WAS NOT. BOTH GROUPS WELL UNDERSTOOD THE CONCEPT "CUTTING TOOL."

At the biochemical level, the total mental system of a human being must be presumed to be an organization primarily of activities of the brain, the central nervous system, and certain of the endocrine glands, extending only secondarily to the autonomic nervous system. Of this biochemical level, where mental behavior originates, we know very little.

At the level of gross, or molar, behavior, however, where observation of learning sequences and records of "conscious" thought are the pertinent phenomena, we may study mental behavior in terms both of learning theory and of measurement.

At this molar level, mental or intellectual behavior is a natural system of phenomena, occurring within the individual organism. It is a system because all mental phenomena function interdependently. Furthermore, all such phenomena interact so as to maintain both processes of change (learning and unlearning) and processes of continuing organization or stability.

Mental behavior, in this molar system, is presumed to result from both genetic and developmental phenomena. The developmental aspects of mental behavior vary according to the amount and intensity of the mobilization (functioning) of the genetic and other organic processes, as stimulated and guided by both the natural and the cultural environments. Most, but not all, developmental mental phenomena, we hold, are learned.

Learned mental systems include a great range of subsystems. One of these subsystems of learning, namely, "problem-solving" activities, has been made the chief subject of intelligence testing. In every individual's problem-solving activities, his culturally learned acts influence all the other acts and skills in the system. Therefore, no problem in a mental test may be considered to measure mental ability apart from cultural learning.

We assume that the following phenomena enter into the system of acts that constitute a pupil's response to a test problem; they are, therefore, responsible for the status of one pupil on a test, as compared with that of another pupil:

H Hereditary phenomena, determining certain complex organic structural relationships and functional patterns, probably organized by the brain and central nervous system. These hereditary factors are almost never the same in a child as in either of his parents, because the number of possible different combinations of the parents' genes is more than 1,000,000. There is no evidence that such hereditary factors are segregated by socioeconomic levels.

C Cultural phenomena involved in the pupil's degree of experience, obtained in his family or play group, with that particular social environment that determines both the content and the symbols of the test problem.

C_1 Training phenomena in school and home, involving more specific, repetitive, and purposeful experiences with cultural situations and symbols that are closely related to the test problem.

C_2 Cultural motivational phenomena, or "drive'; those rewards and punishments that exist at a level sufficient to impel the pupil to use his full pattern of activities to solve the test problem.

E Emotional phenomena, such as the type and degree of the pupil's anxiety in the test situation.

S The phenomena of "speed," which are complex functions of the hereditary factors, the physical condition and stamina of the testee at the particular time, his cultural habits of work, his familiarity with the cultural form and content of the problem, and his previous experience or training with this specific type of problem.

The relative success of two persons in solving a given test problem, therefore, depends not simply upon their hereditary equipment, but also upon their relative familiarity with the cultural situations and symbols in the problem, their relative degree of training with similar problems, and the relative strength of their motivation. The factor of time may or may not be involved, depending upon whether the test is a speed test or not.

Cultural influences upon problem solving: The lifelong process by which culture helps to guide, develop, limit, and evaluate all mental problem solving has not received serious attention from either testmakers or educators. They continually make the error of regarding middle-class culture, and even more narrowly, middle-class school culture, as the "true" culture, or the "best" culture. More than 95 percent of our teachers and professors are middle-class in their socioeconomic status. Like all other cultural groups, teachers and professors regard that particular version of culture (those mores, emotional patterns, and social values), which they have learned from their middle-class families, friends, and teachers, as the "best" and only "true" culture. This attitude is powerfully reflected in school curriculums, in intelligence tests, and in teachers' judgments of their pupils. It is an attitude that is fatal to the development of the full mental capacity of either the teacher or the pupil. The belief, for instance, that the skills of reading, or writing compositions, and of learning the usual academic materials are the primary essentials of "education" destroys the real aim of education, which is to learn how to think so as to solve life problems. Most schools do not teach pupils how to explore even the simplest real-life problems, how to define their crucial issues, and how to proceed to solve them. The slum schools, especially, seek to teach pupils only how to read, how to carry out memorized, routine arithmetical operations, and how to paraphrase textbooks. Furthermore, intelligence tests seek to predict this kind of behavior; therefore, they include chiefly those problems that are closely related to the culture of the schools.

In acquiring this middle-class academic culture, children of low socioeconomic groups, on the average, do not perform well, either on scholastic tasks or on the intelligence tests that are designed to measure types of learning closely related to scholastic problems.

These low socioeconomic groups fail because their parents themselves have not been trained to read; nor do they regard reading or school curriculums as important. Moreover, neither the parent nor the child's social group urges the child to practice reading or school exercises, nor sets him an example for attainment in this field. The parents and friends of the average child in the high socioeconomic groups, on the other hand, do offer him a powerful example of their skill in reading and in the school type of culture. Furthermore, this latter group of parents consciously and unconsciously reveals an interest in the child's learning this behavior; they likewise afford him practice in such problem solving. For this very reason, Binet and Simon continually stated specifically that reading and other school-type activities were suspect as devices for measuring the intelligence of such children.

This example of reading skill should focus our attention upon the primary role of culture both in identifying those problems that a specific human group regards as important, and likewise in training members of this group to think about such problems. "Culture" means all behavior and thought learned in conformity with a social group. Pupils coming from the top and bottom social strata live in cultures which, though alike in certain fundamental American activities, are yet different in many other cultural habits and motives. At a great many points, therefore, their cultures differ with respect to the types of problems they teach each group to recognize and to solve.

Every human being goes through three essential steps in solving any mental problem. First, he must become aware that there is such a problem. Second, the problem solver must define the essential issues of the problem; that is, he must recognize exactly what type of relationship (such as that of oppositeness, similarity, or analogy) he is asked to discover between two or more variables. (Culture sets and teaches this convention by which the crucial aspects of a relationship are defined. Thus, "opposites" are defined by academic culture as the extremes within a given category or continuum, and not as simply completely unlike concepts. In intelligence tests, moreover, a large proportion of all questions admit of two or more interpretations; only cultural experience with intelligence tests or similar academic problems will enable a pupil even to define the problem as the testmaker requires.)

Third, the individual who solves a mental problem must learn a method, which so correlates the decisive elements of the problem as to reveal their "true" relationship. (Culture states this method for nearly all mental problem solutions, for nearly all human beings. Only one human mind out of millions can discover valid new methods; these in turn are taught by the culture.)

Extremely powerful, then, is the force of culture in determining both the types of mental problems that human groups discover and explore and, second, the methods and skills by which these problems are solved. If the same tests of mental problem solving are to be used with children of all socioeconomic cultures—and this is inevitable in public schools—how is this problem of cultural difference to be solved?

WE HAVE tried to state the general cultural issues involved in any effort to measure the comparative level of problem-solving activities in different individuals. It may clarify our grasp of the issues further if we examine the course actually followed by testmakers with respect to eliminating cultural bias.

Binet and cultural influences: Alfred Binet excelled all his successors both in the realism of his test problems and in his desire to control the cultural aspects of these tests.

Binet made it perfectly clear that he had been greatly concerned over the danger that large sections of his test measured chiefly the effects of cultural training rather than of "mental capacity." He had eliminated certain tests, he wrote, because their solutions could be greatly facilitated by the home or school training of the child.

> There are tests which require a knowledge outside the intelligence of the child. To know his age, count his fingers, recite the days of the week indicate that he has learned these little facts from his parents or

friends; we have thought well to suppress these three tests. There are tests too exclusively scholastic, as that of reading and retaining a given number of memories of what has been read, or copying a written model, or writing from dictation. We suppress these.

Even more radical was his decision to eliminate his one test involving reading: "In our new series of tests, that of reading is eliminated, because it belongs to the degree of instruction. . . ." In seeking to avoid problems whose solution could be influenced by either home or school training, Binet went so far as to eliminate questions requiring a child of six years to tell his age, or a child of seven years to copy a sentence, or a child of nine years to "read and retain six facts." Those tests eliminated by him because they had scholastic and other cultural bias included:

Tests of 6 years:	Tell the age. Distinguish evening and morning.
Tests of 7 years:	The fingers of the hand. Copy a written sentence.
Tests of 8 years:	Read and remember two facts. Write from dictation.
Tests of 9 years:	Days of the week. Read and retain 6 facts.

Binet then considered the research of Decroly and Degand, which indicated that 47 children tested in Brussels, of "a social class in easy circumstances," showed an average advance in test level of one and one-half years over Binet's "working-class" children. Binet could not face the implications of this fact, when considered in the light of his own views that home and school training influenced the responses to most of his test problems. First, he himself conducted a better and more conclusive research, in which he had M. Morlé test one group of children of high socioeconomic status and another of low socioeconomic status. This research also revealed that the children of low socioeconomic groups did less well on Binet's tests, and that they also did less well in scholastic work. At the same time, Binet pointed out that nearly all the problems in his tests were "correlated" with cultural "aptitudes," which he called "home training," "attention" (motivation), "language," "habit of looking at pictures," and "scholastic exercise."

At this point, Binet stopped reasoning. He simply ignored the facts in making a final estimate of his tests. He could not, or would not, follow through to the obvious conclusion that other kinds of problems were needed. Although he said that these types of training were more highly developed in the schools and homes of children of high socioeconomic status, and although he knew, from his own research, that children of high socioeconomic status had a much easier time with his type of tests, he did not face the clear inference that his tests were not a fair basis for comparing the mental capacity of children of different socioeconomic levels. Intellectually honest and self-critical as he had shown himself to be in his previous work, Binet's failure in this respect may perhaps be attributed to his final illness. Yet his insight into this crucial issue, namely, that of the cultural content and form of mental problems, was far greater than that of subsequent testmakers.

THIS brings us to a consideration of the "mental tests" now widely used in schools. How have the problems in these tests been selected, and what is the evidence that they are valid measures of "mental ability" or of "capacity to learn?" In analyzing these issues, one must remember that, in spite of the vast amount of research done with regard to other aspects of the present tests, there has been no adequate research carried out upon—

a) the choice of a representative cross section of mental systems and problems, or
b) the discovery of problems and symbols that are equally familiar and motivating to all socioeconomic groups, or
c) the discovery of the proportion of items that favor rural as compared to urban groups, or
d) the discovery of the proportion of ambiguous problems having two or more correct answers. (Such problems exist in all present tests.)

How and why have the present problems been selected? Binet, and many of his successors, chose only those problems that were answered correctly more often by "good" pupils than by "poor" pupils. "Goodness" or "poorness" was judged by reference to a pupil's school achievement. A "good" pupil was one who received high marks in school or who was not retarded in his grade placement. Thus the validity of an item was based upon its correlation with school achievement.

Since testmakers have been unaware of the cultural influences upon both school and test behavior, they have not understood that this method of so-called validation is circular. To prove that a given problem, or test, measures problem-solving behavior, one must find a significant positive relationship between that test and some independent measure of problem-solving activity. But teachers' ratings of pupils are not independent of the activity measured by the standard tests. Consider that the tests include chiefly verbal or picture problems, based upon highly abstract academic concepts. So does the school curriculum. Therefore, a teacher's rating of a pupil is an estimate of the pupil's performance on the same kind of problems as those in the standard tests. Grade placement, likewise, is a measure of a pupil's success on this same kind of academic problem. Neither the teachers' ratings nor grade placement, therefore, may be used as an independent measure against which a test of general problem-solving behavior can be validated.

Furthermore, as Binet himself pointed out, school achievement depends largely upon traits of character, rather than upon mental capacity—upon attention, drive, desire for success in the middle-class culture. Lack of this drive causes the children of low socioeconomic groups to do poor school work. Thus, teachers' ratings are partly an index of work habits and motivation, as well as of mental capacity.

The mistake into which Binet and other testmakers fell, in using grade placement or teachers' ratings, was still more serious, because teachers' ratings differ in their meaning from school to school. Thus a good pupil in a slum school usually is not equal in scholastic achievement to a good pupil in a school for high socioeconomic groups. The teachers' ratings of the former pupil means usually that he is good only as compared with the pupils in that school.

Exactly this error was the cause of Binet's failure. The pupils upon whom he attempted to validate his test all attended schools for working-class children. Thus, his good pupils were not as highly skilled or motivated, for his type of tests, as were pupils in the richer neighborhoods. Therefore, judging the proper age level for a test by his working-class sample, Binet set the age levels of his problems too high. When Decroly and Degand found that children from high socioeconomic groups solved most of these problems at an earlier age, Binet saw his own error. But he was unable to find a way to correct it, for he could not think of any more objective way of selecting problems.

That is, Binet could think of no independent measure by which to judge the validity of his tests. Nor has any testmaker since Binet been able to validate his tests against some other measure of problem-solving activities, which is different in kind. To say, for instance, that a test is a measure of intelligence simply because it correlates highly with another standard intelligence test is to reason in a circle. For all such tests are obviously based upon the same academic type of culture. They correlate highly with one another for the same reason that the individual problems in all such tests also correlate highly with one another and with the test as a whole.

In seeking to justify their selection of problems, testmakers have made arbitrary claims, with no research evidence; or they have grasped at expedients peripheral to the main issue. Terman, when he made the first Stanford-Binet test, followed Binet in choosing only those problems highly correlated with school ratings and school progress. Like Binet, furthermore, Terman selected only those problems that distinguished between adjacent age groups. Whereas Binet considered a problem "normal" for that age group in which 50–60 percent of the respondents answered correctly, Terman's standards for age placement varied more arbitrarily. In Year VIII, he placed some problems that only 55 percent of the respondents answered correctly; in the same year, he placed other problems that 90 percent of the respondents answered correctly—a range of 36 percentage points!

The basic aim in selecting test items should be to control "socioeconomic differentials." For it is the socioeconomic bias in the selection of problem areas and of specific test items that determines, in turn, the "age" differences. Many persons certainly will be astounded by this view. Yet, before rejecting this analysis, they should consider carefully the closely related problem of "sex differences" as handled by testmakers. For problems showing different levels of performance by boys and girls are thrown out by the testmakers!

Why? Terman's remarks are interesting: "This was done for the sexes separately as a basis for eliminating tests which were relatively less fair to one sex than the other."

How can a mental test be "less fair" to boys than to girls? It involves no organic sexual differences, so far as we know. There is here no question, apparently, of different cortical structure, or glandular secretion. Clearly, then, if a question is unfair to boys, as compared to girls, it must be that their social experience is different, on the average. Their training as males or females in the family, school, and play group differs, and therefore the amount of experience they have had with various types of mental problems differs. Their social motivation, as boys or girls, to learn how to solve such problems must also differ. Terman certainly did not believe that girls were genetically superior to boys in mentality, or vice versa.

Thus, upon what must have been a purely social and cultural basis, that is, upon the conviction that any problem was "less fair" if either sex proved "superior" to the other sex in solving it, Terman eliminated all such problems from his test.

Upon exactly the same principle as Terman used to control sex bias in tests, one might throw out all problems in which any socioeconomic group proves superior. There is no evidence, and no theory shared by the leading human geneticists, to the effect that the underprivileged socioeconomic or racial groups are genetically inferior to the more prosperous socioeconomic groups. There is abundant scientific evidence, advanced by sociologists and social anthropologists during the past twenty-five years, that a child's particular social and cultural environment directs, trains, and motivates his behavior. Thus, any differences between the average response of different cultural groups to a mental problem may be attributed to their unlike cultures. Therefore, all problems that show socioeconomic differences in performance should be ruled out of tests as unfair, just as Terman threw out all problems showing sex differences. Both types of differences—sex and socioeconomic—when existing between large groups, may be assumed to be the result of unlike cultural training and not of genetic sortings.

The recent makers of group tests have erred in selecting problems, as a rule, just as did Binet and Terman. For his Alpha nonverbal test, Otis selected only those problems that discriminate against "retarded" pupils. He explains—

> pupils were divided into two groups—a "good group" (in school) and a "poor group"—according to whether the pupils were advanced or retarded for their ages. The value of each item was then investigated as follows: The percent of the "good group" which passed each item was found and the percent of the "poor group" which passed each item was found. Only those items were retained which showed a distinct difference between those two percents.

But we know that those groups of children in elementary schools who are retarded in grade placement are overwhelmingly from the lower socioeconomic groups. Therefore, Otis' method, like Binet's and Terman's, rules out all problems on which the lower socioeconomic groups are superior!

Otis likewise says that he preferred geometric items for the same reason; i.e., they discriminated more clearly against the "retarded" pupils: "Since the first investigation showed a tendency for 'geometric' items to be more valid [i.e., to be relatively harder for low socioeconomic groups] than purely pictorial items, more geometric items were added." To validate the second and third experimental editions of this test, Otis used a "good group," which ranked highest on a composite score: this composite score was based upon a pupil's scores in earlier forms of Otis' tests and upon the pupil's grade-age position. Both these criteria for selecting problems, of course, also penalize the low socioeconomic groups; for such groups are more often retarded, and the previous forms of the test had been made so as to give them low average scores.

Otis used the same method to select items for his Alpha verbal test. Unlike most other testmakers, however, Otis states specifically that there is a heavy cultural influence in verbal tests. But he does not recommend that his verbal test should therefore not be used to measure the mental ability of the lower socioeconomic groups in the United States.

Otis likewise chose only those problems for his Beta test (grades 4–9) that were easier for those pupils "who were making rapid progress through school" and much more difficult for those pupils "who were making slow progress through school." The same error exists, therefore, in the validation of this test.

Henmon and Nelson used the same kind of circular reasoning in selecting their problems. They explain: "Only such items as proved to differentiate between pupils of known superior and known inferior mental ability were retained." In other words, no independent measure of validity was found.

Edward B. Greene, author of a standard text on measurement, illustrates in a chapter on the selection of test items, the "best method" for selecting items. This method divides the group of pupils (on whom validation is being based) into thirds, judged by their total scores on the experimental form of the test. An item is considered a valid item if a higher percentage of the top third of pupils (judged by total scores) passes it than of the lowest third. Any item on which the performance of the lowest third is as good as, or superior to, that of the highest third is to be thrown out.

Thus, it is impossible to get a range of types of problems in the test. Since the author always starts with academic types of problems, and since the top third of the pupils on these academiclike problems will be chiefly the middle-class pupils, the test inevitably will include more and more such problems. The lowest third of pupils, who will be chiefly in the lowest socioeconomic group, will not be allowed to show, in the final test, any of the activities at which they are superior or equal.

Terman and McNemar selected for their group test only those items which showed: ". . . successive increments from Grades 7 to 9 to 11, in the precentages [of pupils] giving correct responses. . . . The average per cent passing an item for the three grades was used as the final measure of item difficulty." Although they did not use pupils' ratings and grade placement as criteria for selecting items, they got the same result as those testmakers who did. For their group test has a higher proportion of items that discriminate between high and low socioeconomic groups, at age thirteen or fourteen, than either the Otis or Henmon-Nelson.

The reason for this high socioeconomic discrimination is that Terman and McNemar chose a very narrow range of academic problems, all of which were verbal. As we have pointed out, middle-class pupils really are "older" mentally on such problems. Therefore, if one restricts his test to academiclike areas, he will get very high discrimination between socioeconomic groups, even if he overtly selects problems merely because they show age differentiation. He has already discriminated effectively against the low socioeconomic groups by deciding to use only verbal analogies, verbal opposites, syllogisms, etc. He gets significant increases in the percentage of pupils passing such items between grades 7 and 9, and 9 and 11, because the pace of middle-class school and home training increases in such language areas at these ages. Furthermore, since he chooses only those items that show the largest increment, from age to age, in percentage passing, he selects exactly those items relatively easiest for the children of the higher socioeconomic status. For it is the middle-class pupils who are responsible for the big jump in percentage passing this kind of item. As has been said, their progress in verbal school training is accelerating at these ages, whereas the rate of progress of the low socioeconomic groups is decreasing.

Another criterion, used by Terman and McNemar for selecting problems was—

> the tetrachoric correlation of each item with the total score based on all three test forms [computed for Grades 7, 9, and 11 separately]. No item was retained which yielded an average tetrachoric correlation [for the three grades] of less than .30 and only 10 per cent of the final items have validities of less than .40. The average coefficient for all retained items was .53.

This is still a circular validation. It does not indicate that the whole test is a representative sample of mental problems, nor does it set up any independent measure of mental ability. It simply says that the test is of one piece. But that is exactly why the test does not measure mental ability; it is restricted to verbal systems of academic problems. Kuhlmann and others likewise have used this criterion of significant correlation between an item and the total score as a basis for selecting each item.

The authors of the California Test of Mental Maturity have stated that they have found "no purely objective criteria" to use in validation. Instead, they have relied upon significant correlations with other tests of mental ability (from our point of view, this indicates that the test is a poor one) and upon "samplings" of "factors."

This second effort to validate follows Thurstone's method. Thurstone himself, however, has depended upon conventional academic types of problems. To select problems so as to obtain "factorial saturations" does not of itself meet the basic requirements for a fair test. First, one must sample a wide range of "factors." One must also, and above everything else, see that the factors are expressed in common cultural symbols. Verbal comprehension, for instance, must be tested in the language of the respondent. If there is a mental factor for verbal comprehension, certainly it is equally distributed genetically throughout all socioeconomic groups. The lower socioeconomic groups, however, talk in their own language, which is a dialect.

By far the best theoretical analysis of the unsound basis of validation used by test-makers has been that of F. Kuhlmann and Rose G. Anderson. These authors see clearly the errors in present tests, and, by common sense, they have done a little to improve them, but nothing to validate adequately their choice of items. They have simply selected those items for which the percentage of correct answers increases most rapidly with age of pupils. We have already shown that this method, when applied to academic types of problems, inevitably leads to the selection of those specific verbal, picture, or geometric problems in which middle-class pupils are most superior.

Kuhlmann and Anderson recognize the bias that exists in the selection of mental areas, and of specific problems.

> In selecting tests, the utmost care should be exercised in finding what is universal and common to all in both school and home. The final answer to how near any test comes to meeting this requirement can be reached only through extensive research, of which relatively little has yet been done. The present selection of our tests as regards this factor was done, of course, by inspection. The research necessary to determine this question experimentally would have been far more extensive than all the rest taken together.

Since they have not done this research, they do not know how to get "common problems."

The difficulty of solving the problem of test validity has led to some novel arguments. Thus Terman and McNemar say:

> Validity. In the early days of the development of group tests of mental ability an attempt was made to validate them by correlating the scores with teachers' marks. As has been pointed out many times in the intervening years this procedure is unsatisfactory because of the serious shortcomings in teachers' judgments of mental ability. The best evidence of the validity of the Terman test is to be found in its successful use over the period of years since the test was first issued. Many instances may be cited where the Terman test has been used with great success in guidance and administration. In some situations where the use of the Terman test with entering high school students has been made a standard practice, it has been found that year after year those students who graduated with honors were those who made scores in the highest range of the test. The correlation of the revised test with the original test is .91, which indicates that the new test can be considered to be measuring essentially the same basic abilities covered by the original forms.

Exactly what is it that pupils who ranked high on this test have also accomplished? They have received high ratings from their teachers.

E. B. Greene gives the usual definition of validity: "Thus a valid test of intelligence is one which distinguishes correctly between those who are judged by competent judges, to have various degrees of this ability." In practice, these "competent judges," presumably, are teachers. But what guarantee have we that any group of judges knows, and will be guided by, the basic requirements of (*a*) range of mental systems, and (*b*) cultural communality, of the problems within each area? On the contrary, we know that the "judges" have not recognized these criteria, but have used largely middle-class cultural problems as their guide.

Experimental study of cultural influences: We have now reviewed the methods employed to select both problem-solving areas and also specific items for the present tests, as well as the efforts to validate these selections. These methods, as we have shown, have resulted in tests that discriminate significantly between high and low socioeconomic groups. The crux of our problem, then, is to discover why the present tests have proved far more difficult for the lowest socioeconomic group. In seeking an answer to this question, and following an experimental design set up by Ernest A. Haggard, we have conducted a large-scale experiment with pupils of ages nine and ten, in order to measure the relative importance of various cultural phenomena in test responses, as between the highest and lowest socioeconomic groups. These cultural phenoma were:

a) Cultural symbols (words or pictures) used in test problems.
b) School-training of pupils in related problems.
c) Specific rewards for pupils.
d) Oral vs. written presentation of problem to pupils.

This central experiment, designed to test the basic hypotheses underlying all the vari-

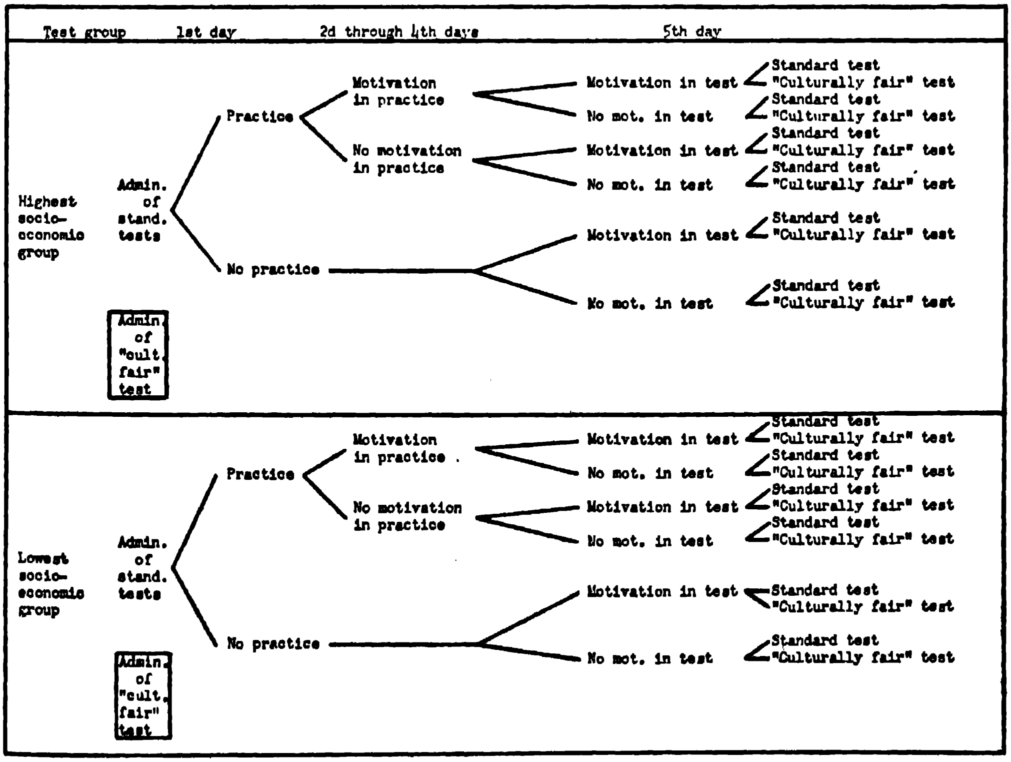

FIG. 3. DESIGN OF AN EXPERIMENTAL STUDY OF CULTURAL INFLUENCES

THIS EXPERIMENT WAS DESIGNED TO FIND OUT WHY CHILDREN OF THE HIGHEST SOCIOECONOMIC GROUP SCORE BETTER IN CURRENT INTELLIGENCE TESTS THAN THOSE OF THE LOWEST. SOME 500 CHILDREN TOOK PART.

ous parts of our research, will be described in a later article, and more fully in a monograph.

Here we may report, however, that we experimented with 516 pupils, equally divided between the highest and lowest socioeconomic groups. Using groups (cells) matched for chronological age, I.Q., and socioeconomic index, we tested pupils with standard test items and also with similar items, which we had made fairer culturally. This testing occurred on a Monday. On the next three days, we gave to half of each test group a total of two hours of instruction and practice with problems similar to, but not identical with, those on the tests. The other half of the testees received no practice.

Each of these halves ("practice" and "nonpractice") had been further divided into half, so that one-half (one-quarter of total sample) received the promise of a movie ticket if they worked hard in the practice sessions, whereas the other half of each practice and nonpractice group were told nothing of a reward. Finally, on the fifth day, half of each such group were told that they would receive a movie ticket if they did well on the retesting that day, and the other half of each cell were told nothing of a reward. Half of all the cells then were retested with the standard tests, and the other half with the "culturally fair" items. On the retesting, two of the cells were given the culturally fair tests orally, so that reading by the pupil was not necessary. Figure 3 may help to clarify the design of this experiment.

Until a detailed report of the analysis is published, it is important to notice for our purposes here that the lowest socioeconomic group improved at the same rate as did the highest socioeconomic group. Motivation during practice decreased the attainment of the highest socioeconomic group significantly, on the standard test items. Motivation during the test helped the lowest socioeconomic group significantly more than the highest. Practice, only, did not help the lowest socioeconomic group significantly on the standard test items, but practice, only, did help the highest socioeconomic group significantly on these items. Both socioeconomic groups performed better on the culturally fair test than on the standard tests, but there was a strong tendency for the lowest socioeconomic group to gain more than the highest socioeconomic group on this culturally fair test. (The items were verbal, paper-and-pencil items.) Finally, it is important to notice that on the culturally fair tests, the lowest socioeconomic group performed better on the oral (no reading) than on the regular (reading) presentation, whereas the highest socioeconomic group did slightly better on the regular than on the oral presentation.

It seems clear to us that if they wish to construct tests to determine the relative mental status of individuals who have grown up in different socioeconomic levels in the United States, the testmakers must take three major steps:

1. They must sample a far wider range of mental activities and thus greatly reduce the importance now given to academic types of problems.
2. They must choose problems that are equally common and equally motivating to all socioeconomic groups, and they must express these problems in symbols that are culturally common.
3. They must discover an objective method of validating their choice of problems, to replace the circular validation now used.

PHYSICAL ANTHROPOLOGY AND RACE RELATIONS: A BIOSOCIAL EVALUATION*

WILTON MARION KROGMAN

Dr. Krogman (Ph.D., Chicago, 1929) assumed, on September 1, the chair of physical anthropology in the Graduate School of Medicine, University of Pennsylvania, after many years as professor of anatomy and physical anthropology at the University of Chicago. He is a specialist in racial differences and prehistoric racial movements in Europe and Asia Minor.

HUMAN paleontologists are agreed that the hominid split-off from a basic Primate stem occurred many millions of years ago. They differ only in their interpretation of just how many millions of years are involved; some go back to the Oligocene, others to the Miocene or even Pliocene. But there is one thing upon which there is now pretty general agreement, namely, that the first fossil hominids, found in the first half of the Pleistocene, dating to only one million years ago, are already quite uniformly assignable to our present genus, *Homo*, even though we still retain the time-honored *Pithecanthropus erectus* and *Sinanthropus pekinensis.* This we may interpret as signifying that basic morphological (osteological and odontological in fossil specimens) characteristics were shared more or less equally by all the then-living hominids. Whatever differences existed were only at specific levels (but I might point out that it has been suggested that the fossil men of China and Java be called *H. erectus pekinensis* and *H. erectus javanensis,* respectively—so that even then differences were possibly at subspecific level). The march of human evolution in the past one million years has been in the direction of a basic morphological and generic identity. From *H. erectus* to *H. neanderthalensis* to *H. sapiens* has been a logical procession that has seen our own species emergent by at least the last 50,000 years and probably somewhat earlier. (There is increasing evidence of a very considerable antiquity for *sapiens* characters.) Be that as it may, we are now in a position to state an important biological fact: all mankind living today—there are no exceptions—belong to the same genus, *Homo,* and the same species, *sapiens.* The biological implications of this fact are as clear as they are simple; that is, that all peoples of the earth are infinitely more alike than they are different. I have used the comparison—more for rhetorical effect than as a precisely determined percentage—that we are 99.44 percent alike, only 0.56 percent different. The percentage ratio may ultimately prove to be 98: 2, or 95: 5, or even 90: 10, but the ratio of biological identities is still a preponderant one.

Let us, then, examine this possible 0.56 percent, this fraction that seems to loom so large in social awareness. I am reminded of a few lines from Shakespeare:

> Strange is it, that our bloods,
> Of colour, weight, and heat, pour'd all together,
> Would quite confound distinction, yet stand off
> In differences so mighty.
> —*All's Well That Ends Well,* II, 3

These "differences" that "stand off" as "so mighty"—what are they?

The physical anthropologist, even though he is prepared to accept the fundamental biological equality of man, cannot but believe his eyes—he notes observable differences in external appearance (and, to a lesser degree, in the skeleton) that do in fact permit of a general categorization of mankind, a breakdown below the level of the species.

There are three, or possibly four, great groups of mankind. These groups, which taxonomically are subspecies, have variously been called "stocks," "major divisions," "ethnic groups," or "races." Obviously, this plethora of terminology has led to some confusion. In the past I have used "stock," but in recent years I have used "subspecies" or, as in this article, "great groups." These great groups, then, are the peoples of Europe, the Caucasoids; the peoples of Africa and parts of Oceania, the Negroids; the peoples of Asia, the Mongoloids; and possibly the peoples of Australasia, the Archaic Caucasoids. Each of these groups is set apart by a fairly uniform (within each group) complex of external traits: pigmentation of skin, hair, eye; form and texture of hair; size and form of nose, mouth, face; certain limb and limb-body proportions; and so on. At the risk of overemphasis it must be stated that it is a *complex of physical traits*—not one, not two or three—that is used in setting up the groups. Any classificatory trait, used singly, is so variable as to lose completely its potential diagnostic value. There is no such thing as trait uniqueness.

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* From the third annual Charles Sumner Lecture of the Medico-Chirurgical Society of the District of Columbia, Washington, D. C., May 22, 1947.

The questions may well be asked, When, where, how, why did these natural hominid groups arise? We may give approximate answers to the when and where, but only speculate upon the how and why. It has been suggested that the Mongoloid group stems back to *Sinanthropus* of China, about 1,000,000 years ago; that the Negroid group stems from *H. rhodesiensis* (a Neanderthaloid of South Africa), about 200,000 (?) years ago; that the Caucasoids stem back to early *sapiens*like forms in Europe, possibly 300,000–500,000 years ago; that the Archaic Caucasoids stem from *Pithecanthropus,* about 500,000 years ago. If these assumptions be warranted, then infrageneric variation and selection have a respectable antiquity. In the evolutionary process, however, the universal hominid attainment of species equality is relatively late.

The *modus operandi,* the how, of the differentiation above implied can trace only to genetics, i.e., either mutations or differences in gene frequencies. With generic identity as basic, then it is logical to assume that infrageneric variation is dictated by genetic impulse. Certainly it cannot be denied that today the traits we use in group classification have a genetic basis (even though we still use phenotype, rather than genotype). As to the why, we can only observe that the reason must have been in the direction of adaptation.

In the past, physical anthropologists have made much of "adaptive *vs.* nonadaptive" traits in classification, feeling that the former are useless, the latter dependable. We are now coming to a realization that this is a time-linked problem, that what we are really concerned with is the *rate* of adaptability. The process of evolution is essentially dependent upon adaptability and modifiability, not so much of the whole organism as of its systems and/or parts. There are some, over a period of time, that are more labile than others—these we have called "adaptive;" the others, more resistant to change, slower-rated, as it were, we have called "nonadaptive." Essentially, therefore, it is the limitation of our time-linked finiteness that has given rise to the dichotomy. Be that as it may, we must assume that such differences in man as do exist are part of an adaptive evolutionary mechanism.

It is here that we must come to grips with the possibility that the adaptive differences may have a qualitative value. Recent studies on the skin, for example, seem to confirm the fact that pigmented and nonpigmented skins are, under certain environmental conditions, of greater adaptive value, and hence superior traits, with respect to survival in a given environment. Thus, it may be adduced that a pigmented skin is superior in a tropical clime, a depigmented skin in a temperate one. There may be, and probably are, other such biochemical and/or physiological "superiorities," but it must be emphasized that they are almost certainly directed toward a better biological balance in a specific environment; the traits are integrated into the entire organic response. There is much we do not know of metabolic gradients, physiological variants, biochemical balances, among the peoples of the world. There are undoubtedly many adaptive differences, either in single traits or even in an entire system of traits. That we still must study and learn.

The foregoing evaluation seems to recognize adaptive biological "superiorities" and "inferiorities." Does this, then, warrant the assumption of "superior" and "inferior" peoples? I would answer with a vigorous No! Again I refer to the fact of biological evaluation (or classification) by an entire complex of traits, not by single traits or a few traits. One physiological or biochemical superiority, aimed at a precise adaptive response, does not permeate into a complete group superiority. I also say No! vigorously because ethnic superiority, as used today, is less a biological, more a social, evaluation. If the physical anthropologist says anything, he says, "There are no biologically superior groups of people." There may be superior *cultures,* but that's another problem.

IF WHAT has been said up to now applies to the three or four major groups of mankind, it applies with equal vigor to the minor divisions of such groups, generally called "races." These divisions within divisions—that are literally at sub-subspecific level—are much less clear-cut, albeit they are, as far as we know, amenable to the same biogenetic principles as are their parent groups. The Nordic race, as an example, is a group of northwestern European peoples representing a type-concept breakdown within the Caucasoid. It is, by its very construct, less clearly defined, far more genetically homogeneous. Again, it has a much more demonstrable record of intermixture, at inter-Caucasoid level, so that although all its constituent members resemble closely the supraparental group, and many resemble the parental group, others may resemble adjacent (Caucasoid) groups with which they were admixed.

It is generally at the level of race that the adjectives "superior" and "inferior" are used. We hear "race superiority" referred to all the time, e.g., *Das Herrenvolk,* the "master race." What the physical anthropologist has said about the larger groups he affirms here: "There are neither biologically superior races nor 'pure' races."

Yet so many groups react to one another as though observable physical differences—the hallmarks of group classification—were of import, somehow, in the entirety of the socioeconomic pattern. The judgment (really prejudgment) seems to be that the measure of physical difference is a yardstick for cultural aptitude. "A man is what he looks like, not what he can do" (if he gets the chance), is the credo of this type of evaluation.

Some years ago I read a poem by Dunsany that to me epitomizes the stealthy, insidious menace of race prejudice. It is called "The Calamity":

It came without a sound,
  Without the slightest tone
Of warning to be found
  By which they might have known.

With neither trumpet call
  Nor finger beckoning,
With nothing said at all
  Aloud or whispering

To wake their faintest fears,
  Beyond what they had read
Each day for years and years
  And had not credited.

It seems to me that we in the United States have in the past been as blind about race prejudice. It has been there in manifold guises but seen not, heard not, because we did not wish to see or hear. The continuation of such willful sensory denial is calamitous. We cannot, in our role as the champion of freedom throughout the world, continue to be blind and deaf to the menace within our own fold. And, as a matter of fact, I think that in recent years we have made , and are making, exciting and gratifying progress in intercultural relations. Our fears *have* been awakened, and we *are* crediting what we have read, what we have seen and heard.

The genesis of prejudice—for it is acquired, not innate or inherited—is difficult to understand, let alone to trace. In the United States, and with respect to Negro-white relationships, it seems to have had its origin in the socioeconomic imbalance induced by the invention of the cotton gin. There is considerable evidence that the plantation owners of that day, and entrepreneurs generally, rationalized that this vast Negro pool of labor was "destined" to serve by virtue of "biological inferiority." We are told by psychiatrists that prejudice is most often rooted in a more or less subconscious or unconscious feeling of insecurity, of inferiority, which is overcome—at least in part—by a conscious assertion and behavior of superiority. The dominating group achieves a sort of therapeutic relief in its ascendant role. No one would claim that this is the sole factor, but it is probably more basic than we realize.

One may well ask the question, If prejudice is irrational, with no real basis in biologic fact, then what next? The answer, as I see it, is direct and forthright: Educate people to the essential biological facts and integrate that knowledge into the pattern of cultural response. I want to make it clear that I do not envision this as a simple process, or as the major one; nor may one expect an overnight reversal of attitude. We act before we think; we must combine action with knowledge so that action and thought, with respect to race problems, are synchronous.

There are several social processes that will at least ameliorate the intensity of prejudiced feeling. The feeling of socioeconomic insecurity, which is so prevalent in Negro-white relations, can be aided if the white worker realizes that the Negro doesn't demand *his* job, but merely the chance to compete fairly for *one like it.* Today, with nearly 60,000,000 employed, the tension of competition is greatly lessened, and socioeconomic prejudice correspondingly reduced.

Prejudice is lessened if the hallmarks of race difference—the physical traits of skin, hair, nose—are, or become, less obvious. This is a process, involving biological mixture with consequent dominance and recessiveness of certain traits, that has gone on in the United States for some time, so that today the American Negro is a pretty distinctive blend of his African ancestry, white, and American Indian. There is reason to believe, however, that this process of mixture is slowing down, so that it will play a minimal role. Indeed, Du Bois, in *The World and Africa,* says: "I dream of a world of infinite and invaluable variety, not in the laws of gravity or atomic weights, but in human variety in height and weight; color and skin, hair, nose and lip."

There are innumerable instances where active contact and joint participation between the two groups has been its own guarantee of acceptance. The white man on the assembly line, who feared (and because he feared, hated) the Negro who joined him soon learned (as a labor leader once told me) that he was "a reg'lar guy. All he wanted was a job to support his wife 'n' kids, same's me." The exceptions, once made, soon add up to acceptance. In a sense, the group was judged by individuals who, fortunately, were representative of the group to which they belonged.

We have witnessed, in recent years, a very potent factor in the alleviation of race prejudice, namely, the cohesive result of war, a common threat from the outside. At such a time it is not whites, not Negroes, not Mexicans, not this-or-that, but Americans all, rallied and united in a common effort. And the gains in understanding, in sharing, in a more equal participation, must be held and solidified.

When it comes right down to it, such a program is not "radical," "iconoclastic," or "foundation-shaking"—to use some of the evaluations one may encounter. If we accept the essential oneness, the essential humanness, of all mankind, then the human birthright to equality of opportunity follows naturally. The whole issue centers upon human dignity, the dignity of the individual. I must confess I see little that is daring in that, and nothing that we don't preach (on Sundays). The human right to freedom from want; the human right to employment vouchsafing a living; the human right to adequate and decent living and housing conditions; the human right to an education; the human right to equality before the law—these are all to be shared by man, not by men who arrogate specific and special rights to their own group alone. The "son of Man" can know no arbitrary biosocial refinements which make for inequality, unless we betray and deny our own humanity.

I might well summarize what I have been trying to say in a "Decalogue of Race" that I wrote several years ago. (Alpenfels, E. J. *Sense and Nonsense about Race*).

1. Thou shalt not bow down before the false god of "racial superiority;"
2. Thou shalt not vaunt thyself that only thy race is "pure;"
3. Thou shalt not preach that races or peoples are at different levels of physical development;
4. Thou shalt not claim that racial differences are of fundamental biological import;
5. Thou shalt not establish racial groups as fixed or unchangeable;
6. Thou shalt not, to the detriment of thy neighbor, assert that cultural achievements are based on racial characteristics;
7. Thou shalt not hold that "racial personality traits" are innate and inherited;
8. Thou shalt not demean thy brother because his appearance differs from thine;
9. Thou shalt not, because a man is of a different religion, insist that he belongs to a different race;
10. Thou *shalt* faithfully and sincerely observe the foregoing admonitions and then, indeed, wilt thou love thy neighbor as thyself.

Is it asking too much to practice what we preach? To put biological fact into social interaction?

WHERE SILENCE SINGS

The bats go singing . . . who have ears to hear?
The crystal chants its magic songs below
And lulls male ants to sleep. Aphides draw near
And more prolific grow.

The ant can hear in one small second round
Tone bells vibrating one half million times!
Oh unknown powers of love and light and sound,
Lend me ears for your chimes!

Vibrate them slower, dull have grown my ears;
Ten thousand harp strings strain to catch the sound.
Lift me into the ultrasonic spheres
Where mystic powers abound!

Vibrate my songs, let echoes guide my ways
In sound realms other than my ears have known,
Pulsate the weird, wild music through my days
Until I hear the stone!

ERIS GOFF

A GREAT AFRICAN PROJECT

EDITH TILTON PENROSE

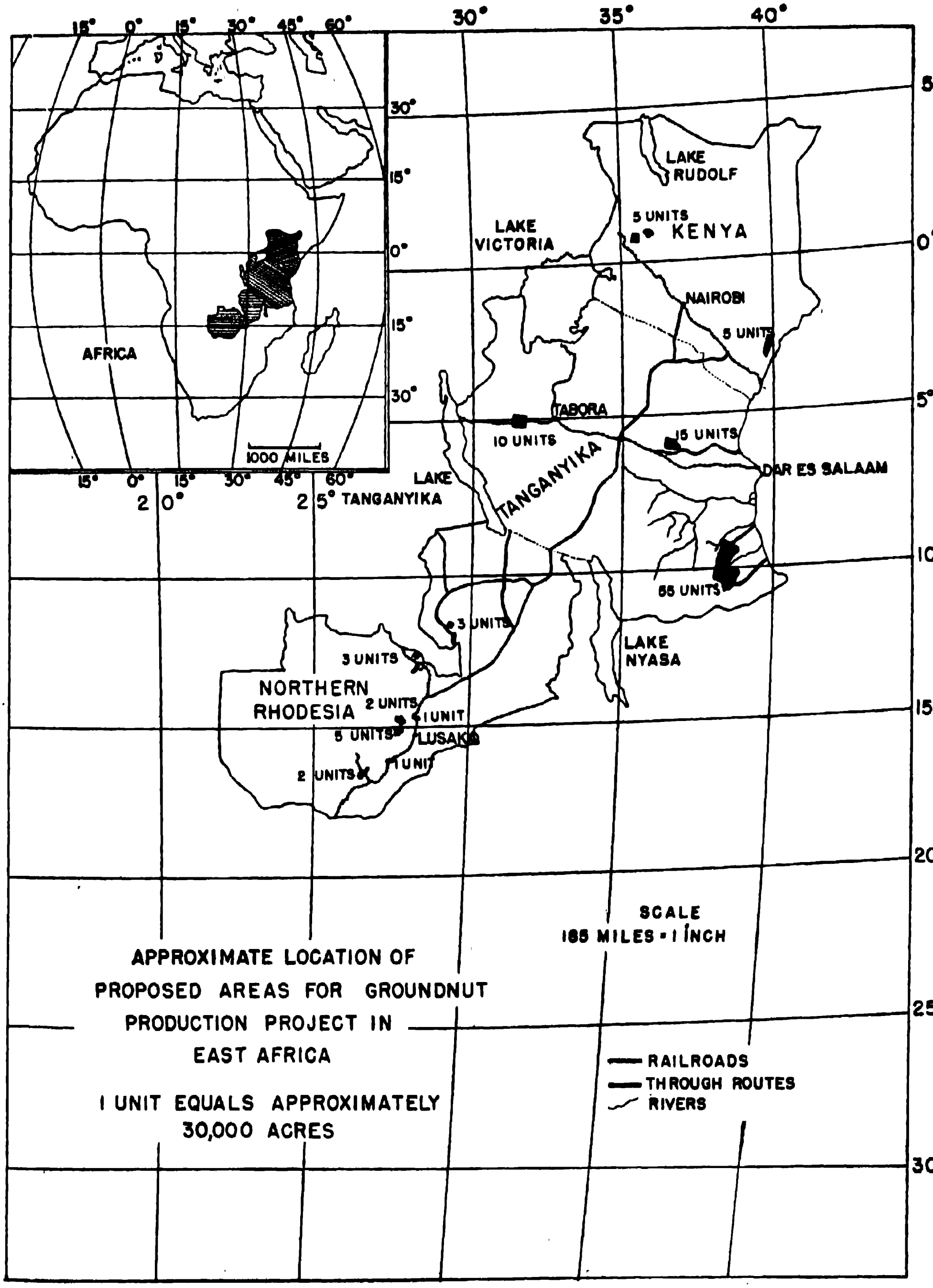
Now studying at Johns Hopkins for her Doctor's degree, Mrs. Penrose has worked with the International Labour Office in Geneva, in London as assistant to the American ambassador to Great Britain, and as assistant to the Advisor on Economic Problems, U. S. Delegation to the Economic and Social Council of the United Nations. She is the author of Food Control in Great Britain *(1942).*

WHILE Americans talk easily of a "declining" British Empire, the British have been going quietly ahead with a great agricultural project in East Africa—a project of immense significance for the development of this area. The government has recently announced plans for the mechanized production of groundnuts, or peanuts, on more than three million acres of land in Tanganyika, Northern Rhodesia, and Kenya, most of which is at present uninhabited, infested with the tsetse fly, and endowed with insufficient ground water even for the domestic purposes of agricultural settlers. The announcement has been enthusiastically received by British government officials, members of Parliament, and others interested in colonial policy and the welfare of dependent people, but it has attracted little notice in this country in spite of its importance for the world food situation and in spite of the interest Americans profess in the colonial policy of other countries.

Today there is a severe shortage of edible fats and oils in the world, of which one of the most important sources is the groundnut, or peanut. This shortage will probably last for many years unless special steps are taken to increase production. In East Africa there are millions of acres of potentially good agricultural land now almost completely uninhabited—controlled not by man but by the bush and the dread tsetse fly, the carrier of sleeping sickness to man and the deadly trypanosomiasis to cattle. The native peoples, equipped with primitive hand tools, with the axe and hand hoe, and using fire to clear the bush, have no chance of making this land productive. Finance, equipment, and technicians are essential for the productive use of this land, so urgently needed not only by the Africans, but at present by the entire world.

In March 1946, Mr. Frank Samuel, the managing director of the United Africa Company, Ltd., presented a plan to the British Secretary of State for Colonies and to the Minister of Food for the large-scale mechanized production of groundnuts in Tanganyika Territory, a British Mandate in East Africa. The government immediately sent out a technical mission to investigate the possibilities of the plan, and in September the mission reported that such a project was agriculturally sound, financially justifiable, and would be of great benefit to the African populations of the areas concerned. It recommended the mechanized production of groundnuts not only in Tanganyika, but also in Northern Rhodesia and Kenya. One hundred and seven mechanized "farm" units of 30,000 acres each were proposed, with villages to house the workers according to healthful modern standards. The possibilities of this type of project for the future of Africa excite the imagination, and the sober British "White Paper" (*A Plan for the Mechanized Production of Groundnuts in East and Central Africa.* London: His Majesty's Stationery Off. Cmd 7030.) in which the project is outlined frequently breaks away from the "official" tone and becomes frankly enthusiastic.

The serious British food position gave the initial impetus to the project, but its greater significance lies in its potential contribution to East African development. And this in turn depends on the way in which the project is carried out. It is clear to the British government that, even apart from any socialist



PEANUTS IN AFRICA

SHOWING IN THE UPPER LEFT CORNER THE LOCATION OF KENYA, TANGANYIKA, AND NORTHERN RHODESIA IN AFRICA AND, TO THE RIGHT, THE LOCATION OF AREAS IN THESE COUNTRIES MARKED FOR PEANUT CULTURE.

ideology, this is not the type of project that can suitably be entrusted to private enterprise. Not only do the amount of capital involved and the scope of the activity required make private enterprise an unlikely sponsor, but the concession of private rights in more than three million acres of African land would be severely criticized today. Plans for the development of the groundnut project are accompanied by plans for the general long-run development of the areas involved in the interest of the indigenous population. And this is properly a governmental function. The initial capital required to finance the project and to maintain it in the early years will come from the British government, but it is planned to transfer the enterprise to the local governments in time and eventually to the African people themselves, possibly on a cooperative basis. Cooperative farming is well suited to the African culture, in which communal agricultural activities have long been carried out, and the project would not be economically practicable if individual subsistence farming should replace large-scale mechanized farming when the enterprise is turned over to native management.

The British government proposes to lease the land from the local governments in the first instance for a period of not less than twenty-five years. The difference between the estimated cost of production of the groundnuts and the estimated world price in the near future is expected to be sufficient to repay the British Treasury for its initial outlay. The management of the project will be provided by a government-owned and -financed corporation with "a responsible Board of Directors answerable to the Government but with the fullest scope for initiative." Until this corporation can be formed, the United Africa Company, Ltd., one of whose officers originally proposed the scheme, will act as managing agent for the government.

In order that no time shall be lost, the project has already been started, but its final scope and the exact method of its execution are still being studied. Scientists and technicians, nutritionists and social welfare workers, doctors, sanitary engineers, and administrators are being sent out to develop the practical details, and all present plans are subject to continuous review as the project develops.

Operations will begin in Tanganyika Territory, where 75 percent of the total acreage will eventually be located—eighty units of 30,000 acres each. The accompanying map shows the approximate location of these areas. Tanganyika lies in East Africa just south of the equator and contains 342,000 square miles. Nearly two-thirds of this area is entirely uninhabitated, and on the remaining one-third nearly six million people are distributed in a highly localized manner. The uninhabited area is largely lacking a permanent supply of domestic water and is covered with a vegetation in which the tsetse fly flourishes. Two-thirds of the total population of Tanganyika are concentrated on one-tenth of its area, and unless the "wide-open" but useless spaces can be brought into productive occupation serious limits are placed on the possibilities of developing the territory and raising native standards of living. Population pressure throughout East Africa is already causing uneasiness to responsible officials, and the development of such large areas in Tanganyika would make possible the absorption of people from other more densely populated parts of East Africa.

The first problem is to clear the bush. The only instrument available to native cultivators is fire. This destroys all organic material, exposes the soil to the baking effects of the sun, and leads to serious erosion, in which much of the ash from burning is washed away. Mechanized bush clearing by means of large tractors carrying bulldozers or treedozers, with subsequent construction of contour ridges and other soil-conservation measures, would eliminate these evils. Soil erosion is one of the most serious factors in East African agriculture, large tracts of land having become desolate and virtually uninhabitable as a result of erosion arising from primitive methods of cultivation. Water conservation and the maintenance of soil fertility are allied problems that could be dealt with by appropriate cultivation and the use of fertilizers, and by rotation of crops. The clearing of the bush will tend to raise the ground-water level, and domestic water supplies will thus be more easily available. Rainfall catchments and storage tanks can be laid down in the absence of sufficient surface or ground water. Under the project, mechanical planting or mechanical harvesting techniques would be adopted, appropriate rotation policies are planned, and after two years of groundnuts the land would be placed under grass, dwarf sorghum, or other suitable crops for a period.

As the volume of production increases, new facilities for handling the produce will have to be installed. These will include a new railway, deep-water berths, bulk storage depots, and port installations in Tanganyika, improved roads and seagoing lighters in Kenya, and improved roads or a new railway in Northern Rhodesia. The development of many parts of Africa is handicapped by inadequate transportation facilities, but in large, unproductive, sparsely populated areas it is difficult, if not impossible, to establish transportation on an economic basis. A large productive enterprise, on the other hand, may make possible the economic development of transportation facilities that will have an effect on the economy far beyond the immediate enterprise.

THE first and most obvious advantage of the project to East Africa is the tremendous increase in the income of the territories concerned that would result from the export of the oil seeds. For Tanganyika alone, under present plans, the total value of its exports would be more than doubled. The income thus available would provide a basis for economic and social development at present lacking. Insofar as the income accrued to the native population directly, it would raise their purchasing power; and insofar as it was made available to the local governments, plans for developing social services and public works could be substantially expanded.

The second obvious advantage is the elimination of the tsetse fly from thousands of square miles. The tsetse fly cannot survive when the bush is cleared. Large areas of Africa were once cultivated by man, who laid the land bare to erosion, exhausted its fertility, and then abandoned it to the encroachment of the bush and to the tsetse fly. To eliminate the fly, man must keep his clearings open. The groundnut project provides an economic basis for the permanent elimination of the bush, and therefore the fly, from large areas—even beyond the actual area of the project itself.

The third obvious advantage is the introduction of modern agricultural techniques on a vast scale. There is no doubt that so long as African peoples are dependent on primitive methods of agriculture it will be impossible appreciably to raise their standards of living without large and continuing subsidies from outside. Many experienced observers of African development are convinced that large-scale modern farming is the only way in many areas to produce enough to maintain or improve the living conditions of increasing populations. The groundnut project itself will not give employment to extremely large numbers of Africans—at the peak of the demand for labor for clearing and for agricultural operations it is estimated that some 57,000 Africans would be employed. When the clearing is completed, around 32,000 will be needed to do the agricultural work. Ancillary operations based upon the project will probably provide employment for more people. Where adequate water is available, livestock on the two-year grass lea, or intensive dairy feeding, using the groundnut tops, might be introduced. Some of the land within the units is not suitable for groundnuts—in Tanganyika, extensive flat bottom lands, or *mbugas,* are too heavy for groundnut production—and these could be used as dry-season grazing grounds for beef cattle or, by using heavy field implements, to produce sorghum grains. Eventually, domestic oil mills could be established to provide for local consumption or, conceivably, when the food position is no longer pressing, for the development of a local plastics industry.

Extensive research activities are envisaged as a part of the project. In the first instance, research in the fields of meteorology, soil fertility, soil surveys and mapping, crop diseases, and variety testing is required. Such research can be planned to provide valuable data on East African soil and crop problems generally. Later, studies of other crops and of livestock problems linked with the nutritional needs of Africa, and of natural resources and power supplies for industrialization would be included. The White Paper even envisages that "in the later phases the project could turn to the task of realizing the untapped food production potentialities of inner Africa; such as the exploitation of swamps and grasslands, forests, mineral deposits and power resources of the continent on planned and scientific lines."

The development of colonial territories in the interest of the native peoples requires, perhaps more than anything else, the development of the skills and capacities of the people themselves. The groundnut project, properly administered, should be able to make a great contribution to this end, particularly since the eventual goal is the cooperative running of the project by the natives. A permanent labor supply is required, and an opportunity is afforded for the development of communities with adequate housing, medical and sanitary facilities, welfare programs, and educational facilities. Given access to educational facilities and introduced to modern agricultural techniques, the Africans would be enabled to develop skills now extremely rare among them. Of the 24,000 workers who would be required for normal operations in Tanganyika alone, some 5,600 will be in a skilled or semiskilled category. Even this small figure is important in terms of the labor supply of Tanganyika, where African opportunities for skilled employment are very limited. The Africans employed will be encouraged to take the responsibility for running their own welfare services and to participate in the management of the agricultural, commercial, and industrial aspects of the project.

Workers and their families can be accommodated in communities around the project that can provide, in the words of the White Paper, "a model of better standards of living for the African people." The people working on the project should be self-supporting in foodstuffs, and families could probably have access to some land to supply part of their own needs. Research into food production and nutritional needs would be undertaken by the research staff of the project.

Here is a great African program. It is one for Americans to watch with interest, for it is time that American concern with the colonial policy and the so-called imperialism of other countries became more constructive. There are large numbers of people who apparently believe that all colonial policy is "imperialist" in some vague sense and therefore sinister. The motives for every act of a colonial government are examined suspiciously, and the straightforward reasons for it are rejected if some hidden, more devious motive can be elaborated. These critics would do well to examine more closely the trend of British colonial policy in recent years. The chief difference between modern colonial policy and older imperialism is the emphasis in the former on the importance *for its own sake,* of the social, economic, and political development of the native peoples. In all aspects of British colonial policy, this question is prominent. It is not possible for the British government today to plan a purely commercial exploitation of the colonies, even to relieve the domestic food situation.

But in the development of all backward areas, outside capital, technical assistance, and managerial skill are required. The motives for supplying them may be mixed—financial profit, the necessity of obtaining urgently needed supplies, and a simple sense of responsibility for the welfare of dependent peoples will all play a part. There should be no stigma attached to any of these motives, provided that the assistance is rendered in such a manner that the economic, social, and political welfare of the native peoples is advanced. Economic developments in colonial areas are in many ways more significant than political developments, but they should be examined on their merits and not in terms of vague and general political and economic ideologies. The groundnut project is neither socialism nor imperialism. Nor is it private investment for profit. It is an imaginative attempt to develop an extremely backward area of the world in the interest of its own people and of the rest of the world.

RURAL CROSS SECTION

OSCAR LEWIS

Dr. Lewis (Ph.D., Columbia, 1940) compiled the material for this article in 1945, during a three-month survey of Bell County, Texas, under the auspices of the Bureau of Agricultural Economics, U.S.D.A. It is printed by permission of Washington University, which will shortly publish On the Edge of the Black Waxy, *of which this is a brief summary. Dr. Lewis is associate professor of anthropology at Washington University.*

Bell County, on the edge of the Black Waxy in the western part of the Cotton Belt, contains within its borders much of the diversity and color that is Texas. It is characterized by a great variety in topography, types of farming, ethnic groups, and cultural patterns. The county embraces two distinct and contrasting topographical regions, namely, the Black Prairie and the Grand Prairie Divisions. The line of separation between these two regions runs approximately north and south near the center of the county. The eastern half of the county, in the Black Prairie Division, is a treeless, level to gently rolling prairie, with rich black sticky soils locally known as "black-waxy." This land has made the county famous as one of the best cotton counties in Texas. The western half of the county, in the Grand Prairie Division, is rolling to hilly land, with lighter, sandier soils covered with cedar and mesquite grass. Much of the land is rough and stony, and in some places rises to 200 feet above the surrounding country.

Associated with the striking difference in topography and soils between the eastern and western parts of the county are significant differences in population density, type of farming, levels of living, size of school districts, neighborhood structure, social participation, and the effectiveness of agricultural agencies. This has made for a type of local regionalism that is an important aspect of the life of the county.

The diversity of Bell County is in large part a function of its marginal position, which has exposed it to two distinctive influences, that of the Cotton South and the Cattle West. As in the South, cotton has dominated the economy and has made a deep impression on the life and customs of the people. But the plantation system, which was the core of Southern culture, never took root. This, together with the early importance of stock raising as against cotton growing, was responsible for the development of cultural characteristics distinct from those of the deep South. Indeed, King Cotton never reigned alone in Bell County. The tradition of the cowboy has come down from the early days when stock raising was the major occupation, and today every schoolboy knows that the Chisholm Trail ran through the center of the county. Unlike the deep South, there was no leisure class to romanticize cotton farming, and it could at no time compete with ranching in capturing the imagination of the people as an ideal way of life.*

As in the South, there is an extremely high incidence of tenancy; but unlike the South, there are few sharecroppers. Landlord-tenant relationships are not characterized by the dominance-submission pattern of the deep South, and tenancy carries much less of the stigma of social inferiority. Levels of living, social participation, and leadership do not closely follow tenure lines. In this respect Bell County resembles the West and Midwest.

The absence of the plantation system, the prevalence of family-sized farms, and the fact that Negroes are not basic to the agricultural economy and are but a small part of the total population, make for much less rigidity in social controls and the class structure. The relative recency of the pioneer period which, because of the late settlement of the eastern portion of the county, lasted until the early 1900s, has had a leveling influence and gives the county some of the characteristic qualities and spirit of the West. The comparatively high standard of living and educational level is also in contrast with the deep South. Bell County ranks second among thirteen sample Cotton Belt counties in level of living, and ranks first in general educational achievement of both farm and nonfarm people.

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* The people of Bell County generally do not think of themselves as "Southerners" nor as "Westerners," but as Texans. However, they identify with the South when speaking with "Yankees." It is interesting to note that on the eve of the Civil War the county was about equally divided on the issue of Unionism versus Secession.

Although Negro-white relationships are governed by the folkways of the South, the social structure does not revolve around race relations; and although Negroes are segregated socially and have separate schools, low-income Negroes, Mexicans, and whites may live next door to each other in the towns. Furthermore, Mexican children attend white schools.

The greater ethnic heterogeneity of the population further differentiates Bell County from the deep South. The Czechs and Germans with their distinctive institutions lend a variety to the social structure similar to that found in the West and Midwest. The presence of a number of Catholic, Czech Moravian, German Lutheran, and German Methodist Churches also gives a distinctive quality to the church life of the county.

The position of ethnic minorities in Bell County differs somewhat from both the deep South and the West. In the deep South, where the rate of assimilation is exceedingly slow, ethnic groups tend to form clusters of culture islands. In the Midwest and far West, particularly in Oregon, Idaho, and Washington, where settlement came late and was completed in a comparatively short time, where settlers represented a great variety of ethnic backgrounds and spread out in loosely organized open-country neighborhoods, ethnic differences disappeared rapidly. After a few years, nationality backgrounds became quite unimportant in local life. Bell County can be placed about midway between these two types of ethnic adjustment. Unlike the deep South, the Czechs and Germans of Bell County are becoming assimilated and acculturated at a fairly rapid rate. In this respect they more nearly resemble the West and Midwest. Until recently, however, they were cultural islands and they still live in their own communities and have their distinctive churches and social organization.

The concept of the United States as a melting pot of peoples of different nationalities and cultural backgrounds has been applied almost exclusively to urban areas. As a matter of fact, this process has also been going on in rural areas, although more slowly and less perceptibly. Bell County lends itself to a study of this process, and in this survey we shall compare the customs and value systems of the Czech and German farmers with those of the old-line Americans and wherever possible will indicate the changes that have occurred in the process of Americanization.[†]

Two other factors have had a very important influence on the life and people of Bell County. The first factor is the presence of Temple, a large urban center, within the county, and the proximity of the county to large metropolitan centers like Dallas and Fort Worth, a hundred miles to the north, and Austin less than seventy miles to the south; the second is the excellent means of communication of the county with the outside, including railroads, good highways, buses, an airport at Temple, a radio station, newspapers, telegraph and telephone. The Gulf, Colorado & Santa Fe and the Missouri-Kansas-Texas railroads run north and south and east and west, respectively, through the county and provide direct contacts with the Pacific Coast on the West, and with the large cities of the East. No town or neighborhood is more than ten or fifteen miles from a railroad, and all railroad stations have Western Union offices. U. S. Highway No. 81 runs through the county and connects with Waco in the north and Austin in the south.

Farmers in Bell County show a Western disregard of distances. Many think nothing of traveling a hundred miles or more to go on a hunting trip, to attend a football game, or to visit a stock show. The proximity of large urban centers, combined with excellent means of communication, has exposed the rural people to frequent contacts with the Great Society. The townspeople have furthered this trend by making Bell County a cultural center and a meeting place for civic, business, agricultural, and social organizations of the region. The townspeople have also been very active in the support and leadership of farm organizations and thus have been the cultural middlemen between the farmers of Bell County and the outside world. These factors have made for much less provincialism than is characteristic of rural areas in the deep South.

[†] Throughout this paper the term "old-line American" is used to refer to third-, fourth-, fifth- (or more) generation individuals of English, Scotch, Irish, or Welsh descent, most of whose parents or grandparents came to Bell County from Southern states.

A further word of introduction is necessary. The social organization of Bell County is in a state of great flux. Rural neighborhoods, schools, and churches are changing; new adjustments and integrations are taking place. These processes are occurring on various levels, between town and country, between ethnic groups, and between different parts of the county. In part, this is a result of the recent trends in agriculture toward greater diversification, and in part it represents a delayed reaction to improvements in the means of communication, greater contacts with the outside, and other developments that have had an accumulative effect over the years. In general, the movement has been away from localism and provincialism toward identification with larger groups over larger areas.

Bell County was selected for study to represent one segment of the Cotton Belt, but it also represents a cross section of a functioning bit of America. Negroes and whites, Mexicans and old-line Americans, townspeople and country people, cotton farmers and ranchers, cedar choppers and charcoal burners, rich and poor—all live together under the common political and administrative rubric of the county. While a county study cannot probe as deeply into all facets of life as might a study of a single community, it has the advantage of focusing attention upon the dynamic interrelationships *between* communities, between racial and ethnic groups, between town and country, and between distinctive regions within the county. It is in terms of the interplay of all these factors that the social organization of Bell County is here studied.

IN THIS survey we have attempted to show how the factors of physical environment, history, ethnic composition, and cultural heritage of the farm population have combined to give the rural life and social organization of Bell County its distinctive quality. In the following discussion we shall summarize the role of these factors under three headings: regionalism within the county; the role of cultural factors in farming practices and rural social organization; current trends and problems.

Regionalism within the county. The differences in topography and soils between the eastern and western parts of the county have had far-reaching influence on the social and economic life. In the west, the combination of rolling to hilly land, light, sandy soils, and large wooded areas along narrow river valleys and uplands has made the area particularly suited for livestock and small-scale diversified farming. Since pioneer days this has been the dominant pattern of land use. The distribution of these two types of farming depends largely upon the density of cedar; in the more open country in the southwest, we find a mixed grazing of sheep, goats, and cattle, with cedar chopping as secondary; in the more heavily wooded northwest area, there is small-scale diversified farming based on corn, cotton, and small grains, with cedar chopping and charcoal burning important. In both these subareas only a small proportion of the land is in cultivation, and the sparse population, almost exclusively old-line American, lives along the river bottoms where there is a convenient supply of wood and water. Most of the small wild game that remains in Bell County is found here, and hunting, fishing, and trapping are still important economic activities. Except for the small areas of rich bottom land, the western part of the county is unsuited for mechanized farming.

In the northwest, the farms are small and are generally owner-operated, whereas in the grazing area of the southwest, ranches are large and are mostly owned by townspeople and operated by tenants or hired labor. In both these areas, the standard of living is low: housing and sanitary facilities are poor (about 30 percent of these farmers have no toilet facilities); and few farms have running water or electric lights. Folk practices have persisted, and many farmers still plant by the signs. Here, too, we find the lowest educational standards in the county.

Localism has persisted in this area, particularly in the northwest, where communities have tended to be more stable and local institutions such as the school, the church, and the store have been retained. In the southwestern part, where tenancy is high and where there has been a greater movement of population, communities and local institutions are much weaker. Throughout the western area, however, there are more one-room schoolhouses, fewer school consolidations, and more hamlet-centered communities than in the rest of the county.

It is interesting to note that the farm families of this area, because of their greater reliance upon subsistence agriculture and their lower standard of living, were better able to weather the depression of the thirties. Agricultural agencies have been least effective in this part of the county, particularly among the poorest farmers—the cedar choppers, charcoal burners, and tenants. Here, too, are the fewest F.S.A. loans, the least soil-conservation work, and the lowest A.A.A. payments for good farm practices. The only considerable payment of A.A.A. money has been for the clearing of cedar and the planting of grasses for sheep and goat raising.

In the eastern part of the county two-thirds of all farm families are concentrated on one-third of the land. The gently rolling, treeless prairie, with its rich black soil, is the center of intensive agriculture and cotton production. Over 85 percent of this land is in cultivation, less than one-third of which is in cotton. During the pioneer period, this part of the county was devoted to stock raising, but since 1880, when the prairie was settled by the Czechs and Germans, cotton has dominated the economy. From 1900 to 1930, cotton occupied about 80 percent of the total planted acreage. Only recently has cotton production declined in favor of more diversified farming and a reversion to the early land-use pattern of stock raising.

The eastern part of the county is characterized by smaller, more town-centered communities, where school consolidation has gone ahead rapidly and local services have declined. The proximity of Temple has contributed to the breakdown of the rural communities and has made for a more urbane population. The Czech and German communities are concentrated in the east. The standard of living is generally higher in this part of the county.

The role of cultural factors in farming practices and rural social organization. Throughout this article I have tried to show the effects of cultural factors upon types of farming and rural social organization. To this end I have compared the customs and value systems of farmers of Czech and German descent with those of old-line American descent. Before summarizing this material it should be recalled that many of the traits here described as typically Czech and German are found in varying degree among some segments of the old-line American population, especially among the small-scale subsistence farmers who live in the western part of the county.

The farmers of Czech and German descent generally have small farms, which they work intensively and carefully. Johnson grass is rarely seen on their farms, and this has given them the reputation in the county of being excellent farmers. They generally have a greater percentage of their farm land in cultivation and grow more of their own foodstuffs and feed for livestock. It is significant that most of the hammer mills for grinding feed are owned by the Czech and German farmers.

In most Czech and German families there is a greater reliance upon family labor—women and children are expected to work in the fields. Among old-line Americans the tendency is to hire considerable nonfamily labor and to gamble between high costs and high farm prices.

The farmers of Czech and German descent seem to have the strongest attachment to the land and seek security through landownership. Landownership is a more important determinant of status among them than among the old-line Americans. Farm tenancy is lowest among the Czechs and Germans. They also show the lowest percentage of town farmers. Among the old-line Americans the form of tenure is much less important; they seek security for themselves and their children through the operation of larger farms and by taking chances on the cycles of seasons and prices.

Love of the land has made the Czech and German farmers more receptive to government agency programs for the improvement of farm practices. We know that whereas German farmers own or operate only 10 percent of the farms in the county, they represent 30 percent of all soil-conservation district cooperators. Also, boys' and girls' 4-H Clubs in preponderantly Czech and German areas are among the best in the county.

The Czech and German communities are among the most cohesive in the county and have the greatest amount of social participation. Social activities center around lodges, beef clubs, insurance and benevolent associations, beer parlors, dance halls, schools, and churches. The church is particularly important as a unifying force in community life. Unlike most old-line American communities, Czech and German communities are not split up along many denominational lines. Furthermore, there are fewer social cleavages in these communities because of the higher incidence of farm ownership, the greater uniformity in size of farms and incomes, and the presence of large extended families.

The picture presented above is necessarily of a generalized nature. It applies most accurately to the present-day older generation and to the conditions in the county that prevailed twenty-five years ago. Today many families of German descent are thoroughly acculturated, and acculturation and assimilation is going ahead at a rapid rate among the Czechs and the rest of the Germans. The greater contact with the outside that has resulted from the improvement of roads and means of communication, the consolidation of schools, the greater use of radios and the movies, and the experiences of the young men in the armed services in World Wars I and II are some of the factors that have incorporated the farmers of Czech and German descent more and more into the main stream of modern American life.

The drive for a higher standard of living seems to be the key to understanding many of the changes that occur in the behavior and value systems of the Czech and German families in the process of Americanization. As the Czech and German families become more acculturated, there is less emphasis upon thrift and self-denial and more money is spent upon labor-saving devices, household conveniences, clothing, and entertainment. The greater cash income necessitated by this higher level of living has made for a trend toward larger farms and bigger operations, more mechanization, and greater reliance upon hired labor. But the great majority of Czech and German farms are too small to support these newly felt needs, and many of the younger generation are leaving the farms or depending more heavily upon off-farm work. A related trend is a greater reliance upon education as a source of security. A high-school education is now considered the desirable minimum by most farm families irrespective of nationality backgrounds. These changes have also affected the traditional family division of labor. In the more acculturated Czech and German families the women no longer work in the fields, and they assume a role of greater equality with the men in family and community affairs.

Another significant development is the gradual disappearance of the Czech and German language. Czech is still the first language learned by children in most Czech homes, and it is frequently spoken by adults, but practically all Czechs are bilingual. In contrast, German is rarely spoken, although most adults are still fluent in the language. German is no longer used in church services, whereas Czech frequently is. There is growing pressure from Czech youths to have all services held in English.

Intermarriage is steadily increasing despite the resistance of both Czechs and old-line American parents. In the German communities intermarriage with old-line Americans has gone farther than is the case among the Czechs.

Current trends and problems. The most noticeable trends are the following:

1. Decrease in rural population: The rural population of Bell County has been on the decline since 1930. The war has furthered this process. At present, approximately 60 percent of the total population of the county is concentrated in three towns—Temple, Belton, and Killeen. This condition will probably persist unless there is an industrial depression with an accompanying back-to-the-farm movement. All evidence points to the fact that a large percentage of the younger people are planning to leave the farm if they can find opportunities elsewhere. The basic reason for this is not dislike of farm life, but dissatisfaction with the low standard of living.

2. Shift away from cotton growing, and related trends: Probably the most significant single change in agriculture in the county in recent years is the marked shift away from cotton growing. In former years cotton acreage was usually 60–80 percent of the total acreage. In 1944, a year in which there was no government restriction on cotton growing, the cotton acreage was less than one-third of the total acreage. It should be noted that the farm labor shortage and the relatively high wages of agricultural workers were contributing factors in the reduction of cotton acreage. Other important and related trends are: (a) more diversified farming, (b) the wider use of cover crops for the improvement of soils, and (c) the increase in livestock both on large commercial livestock farms and on family-sized farms.

3. Fewer and larger farms: The number of farms has decreased with the movement of rural population to the towns. Many farms were abandoned during the war because of the excellent opportunities for off-farm employment within the county. Near-by war industries provided a major source of income for about 50 percent of the farmers.

Since 1930 there has been a steady trend toward larger farm units. In 1940 the average size of farms was 152 acres, an increase of 21 percent over 1930. One-sixth of all farms, principally stock farms in the western part of the county, were over 1,000 acres. However, in the eastern part of the county most farms are still well below 140 acres, which is considered the minimum size for a well-rounded program of diversified farming. The small, uneconomical size of farms is becoming a serious problem among the Czechs, whose average farm is 80–100 acres.

The trend toward larger farms was first given impetus by the A.A.A. Since the war this development has continued with the consolidation of farms, especially in the southwestern area. However, more mechanization was the most important contributing factor in the increase in size of farms.

4. Mechanization: The trend toward mechanization of farms that began in the 1930s has gone forward despite the war. Since 1940, there has been an increase of approximately 500 tractors and 20 corn pickers. There are as yet no mechanical cotton pickers, but some of the wealthier farmers, as well as some of the townspeople, expressed a desire for cotton pickers after the war. If the cotton picker is to come into the county at all, it will have to be a machine of small size. Conditions of terrain rule out any large machinery.

5. Decline of rural communities: A large number of rural communities in Bell County are losing their local services, and the rural population is increasingly going to town for shopping, entertainment, and school. This decline has been accelerated by the consolidation and contracting out of schools, the improvement of roads and means of communication, off-farm employment, movement of farm population to the towns, consolidation of farms, high rate of tenancy, and the improvement of town services.

Rural communities are almost always coterminous with the school district, and the school is one of the most important community institutions. Where the local school is operated, there are generally a greater number of other local services and community life tends to be more active; where the school is contracted out, the community is declining; and, where the school has been consolidated, community life is weakest and there are few local services.

Czech and German communities are the

most tightly knit in the county and have the most active social life. The breakdown of communities is greatest in the southwest where the land has suffered most from erosion. School consolidation has progressed more rapidly in the east because of the greater proximity of the towns.

Of a total of sixty-one communities there are nine trade-centered communities, including Temple and Belton; five village-centered communities; twenty hamlet-centered; twenty-one school-centered; and six store-centered communities. Forty-five rural communities have declined so much within the past two decades that they are now open-country neighborhoods that depend entirely upon town services.

The most important current problems are the following:

1. Farm tenancy: In terms of its long-range effects, tenancy is the No. 1 problem of the county. In 1940, 56 percent of all farm operators were tenants. This percentage has decreased somewhat since the war, but it is still extremely high. Tenancy has existed for so long in the county that it is taken for granted as part of the order of things and generally is neglected as a problem by most government agencies working in the county. Nevertheless, tenancy has been the greatest single obstacle to the spread of improved farm practices. The exploitative use of the land by tenants threatens the very basis of the agricultural economy. This does not mean that tenants are more resistant to culture change than landowners, but rather that the government programs in the county (with the exception of the F.S.A.) were not designed to meet the needs of tenants, who hesitate to improve land not their own. The F.S.A. is the only organization set up to deal with this problem at present. Because of lack of funds it has reached only a very small number of tenants, but as far as it goes, it has been highly successful. What is needed in the postwar period is a greatly expanded F.S.A. program.

2. Soil conservation: Soil conservation is a major aspect of the programs of all the agricultural agencies, such as the Extension Service, Soil Conservation Service, Agricultural Adjustment Administration, and the Farm Security Administration. Since 1939 Bell County has been divided into soil-conservation districts. The work up to the present has been more successful in preventing soil erosion than in improving the fertility of the land. The changes have, on the whole, been limited to landowners and better-to-do farmers. There are elaborate postwar plans for continued soil-conservation work, with special emphasis on soil improvement.

3. Need of farmers' organizations and farmers' cooperatives: At present, there is a lack of farmers' organizations in the county. There is no grange or farmers' union; a chapter of the Farm Bureau was organized recently, but the local reaction was a passive one. Many farmers express the need for farm cooperatives, but in the past their efforts to organize cooperatives have met opposition from town merchants.

4. Need for roads and flood control: The need for better farm-to-market roads is felt especially by the farmers in the eastern part of the county where the sticky black-waxy soils, together with the poor drainage, make dirt roads impassable by car for about five months during the year.

Flood control is badly needed along the Leon River in the western part of the county and along the Little River in the southeastern part. A flood-control project is planned for the postwar period and will probably give employment to a few hundred workers.

5. Need for public-health facilities: Although Temple, Texas, is known as the hospital center of the South, there are inadequate medical facilities in Bell County for the farm population. The shortage of doctors in the small towns and the absence of a county hospital work hardships on the low-income farmers. Some form of health insurance is needed by the farmers in the county.

6. Problem of returning veterans: It is questionable whether any large portion of the 3,500 young men who left the farms for the armed services can be reabsorbed into agriculture in the county. This judgment is based upon the following facts:

a) Despite the exodus of so many young men, farm production has been maintained at approximately prewar levels.

b) The trend toward larger farms and more mechanization has meant a reduction in the amount of necessary farm labor.
c) Many farmers now living and working in towns will, in all probability, return to their farms, thereby limiting opportunities for farming on the part of returned veterans.
d) The present inflated land prices will be a serious drawback to most veterans. Furthermore, most of the best land was bought up by townspeople—doctors, lawyers, and merchants—during the war.

Another aspect of the problem of returning veterans is the question of whether or not they *want* to return to the farms. In the opinion of some informants in the county, a large number of veterans would prefer industrial employment within the county or elsewhere. However, in my survey of thirty-three sample rural communities in the county, it was found that 141 out of 466 returned veterans, or about 30 percent, planned to go back to farming or had already done so. This proportion was higher than anticipated. Of the veterans going back to farming, the highest proportion live in communities in the poorest land areas in the western part of the county, where stock raising and diversified farming predominate. This regional difference in attitude toward staying on the land was also evident in our study of high-school students. Fewer boys from the highly productive black-waxy land areas in the eastern part of the county desire to go into farming.

In the opinion of farm parents interviewed, the only way to ensure the return of the veterans to the farms and to keep the present youth from leaving them is to provide a higher standard of living and more of the conveniences found in towns and cities.

AMEBA

Protean form whose line of ectosarc
Defines the living from the lifeless slime,
A vortex generant of metazoan time
And symbol of our vast primordial dark.

To microvision, gel, and nucleus,
The flow of pseudopod engulfing dust,
Growth and division, struggle to adjust,
Summate ameboid aims subaqueous.

Evolving paradox! Some mutants rose
To stellate beauty, others went
Through gastread and notochordal planes
Where thousand stable types repose.
Itself unchanged! Is this but accident
Or law of subatomic Will to Brains?

JOHN G. SINCLAIR

THUNDERSTORMS

STEPHEN S. VISHER

Professor Visher (Ph.D., Chicago, 1914), long professor of geography at Indiana University, has contributed to The Scientific Monthly, *since 1921, numerous articles, including twelve on various phases of the climate of the United States, with a total of nearly one hundred maps. His latest book,* Scientists Starred 1903–43 in "American Men of Science," *has just been published.*

THUNDERSTORMS are a significant environmental influence in the United States. They are perhaps the most impressive atmospheric phenomenon commonly seen, highly useful in the aggregate, but locally often seriously harmful. The good they do, aside from the often very welcome rain they bring, includes the invigorating drop in temperature, the awe-inspiring display of cloud, wind, and lightning, and the generally stimulating effect of the dramatic change of weather. There is sometimes hail as well as rain.

The harm thunderstorms do includes the squall wind, which usually comes just before the rain begins. This gust may be violent enough to do a great deal of damage. The average annual property loss from "windstorms not tornadoes" in the United States is about $200,000,000. Much of this loss is due to thunderstorm squalls. Often torrential rains fall, with the result that a large share of the water that falls upon sloping ground runs away; much of the soil erosion of our fields is due to thunderstorm rain. "Cloudbursts" not only cause erosion, but they sometimes produce locally harmful floods. The lightning incidental to thunderstorms affords a spectacular display of usually harmless fireworks, but it may do damage. In an average year more than 500 people are killed by lightning in the United States. The hazard to livestock is recognized on every farm. Fires started by lightning annually destroy many million dollars' worth of buildings and crops. It is the cause of many forest fires, with their tremendous losses. Lightning is most dangerous at the beginning of the storm, just after the squall wind, when lightning flashes frequently turn downward. Later, lightning commonly is between parts of the storm, or even upward from the ground.

The lightning of thunderstorms does good as well as harm. Each flash combines some atmospheric nitrogen and oxygen to form ammonia, which, when carried into the soil by rain, adds fertility. Ammonia is an important source of increased fertility in the regions where thunderstorms are common. At the few stations where prolonged studies of this subject have been made, it has been learned that 4 to 8 pounds per acre per year are added thus in England, 6 to 8 pounds in northeastern United States. Lightning flashes also create ozone, an unstable form of three oxygen atoms (O_3). Ozone has a pungent odor and once was credited with "purifying the air"; the claim even has been made that it has a stimulating influence upon mental activity!

Hail, which falls in many thunderstorms, is a convincing proof that the air is cold overhead even in hot weather. It also proves that the updrafts of air that make the imposing thunderhead are strong. Hail does much damage in various mid-latitude regions and on tropical plateaus. In the United States the annual property loss reaches large figures. Detailed Federal estimates are that hail normally reduces the wheat harvest by some 15 million bushels, the oats harvest as much, and the corn harvest by some 24 million bushels. A great deal of damage is also done to fruit, greenhouses, and poultry. Each year some people die in the United States as a consequence of hail bruises, and some hundreds die thus in Europe, Asia, and on the plateaus of Africa.

Thunderstorms are most numerous in the

doldrums or the wet tropics. Severe thunderstorms occur in Brazil, in the Congo basin of Africa, and in the East Indies. They are prevalent throughout the rainy season in parts of the tropics that have a wet season, such as the monsoon region of southeastern Asia and the Sudan of Africa. They are common during the summer months in the parts of the middle latitudes that have summer rainfall; for example, in eastern United States, in Russia, and in eastern Argentina. They occur occasionally in summer in subpolar regions and even in winter over the sea near Iceland. Map 1 shows that in the United States 60 to 80 thunderstorms occur in an average year in each locality in much of the Southeast; 20 to 40 occur in most of the Northeast; and 20 to 50 in most of the West, except along the Pacific coast where there is little summer rainfall and hence few thunderstorms. Thunderstorms occur, however, in spring and autumn in the California type of climate; for example, the vineyards of southern France suffer badly from hailstorms incidental to thunderstorms.

Thunderstorms are the most numerous type of storm, with a world total of about 40,000 per average day, according to C. E. P. Brooks. In the rainy tropics each locality has, as a rule, one a day throughout most of the year, and sometimes two. They are common in summer in most of the United States, where many localities have an average of one or two thunderstorms a week that pass at least within sight of the observer. In most subpolar regions, however, only a few occur, sometimes not one in a summer.

Typical thunderstorms are only a few dozen miles across, and, as they commonly do not live to travel far, they affect only a relatively small area, say, something over 1,000 square miles. In the United States, however, they may travel a few hundred miles and affect several thousand square miles. Their average local duration is only an hour or two. Generally, in the Westerly Wind Belt they travel in an easterly direction, whereas in the Trade Wind Belt they commonly move westward. Their rate of average progress increases with the latitude: it is slow near the equator and usually 10 to 30 miles an hour in mid-latitudes.

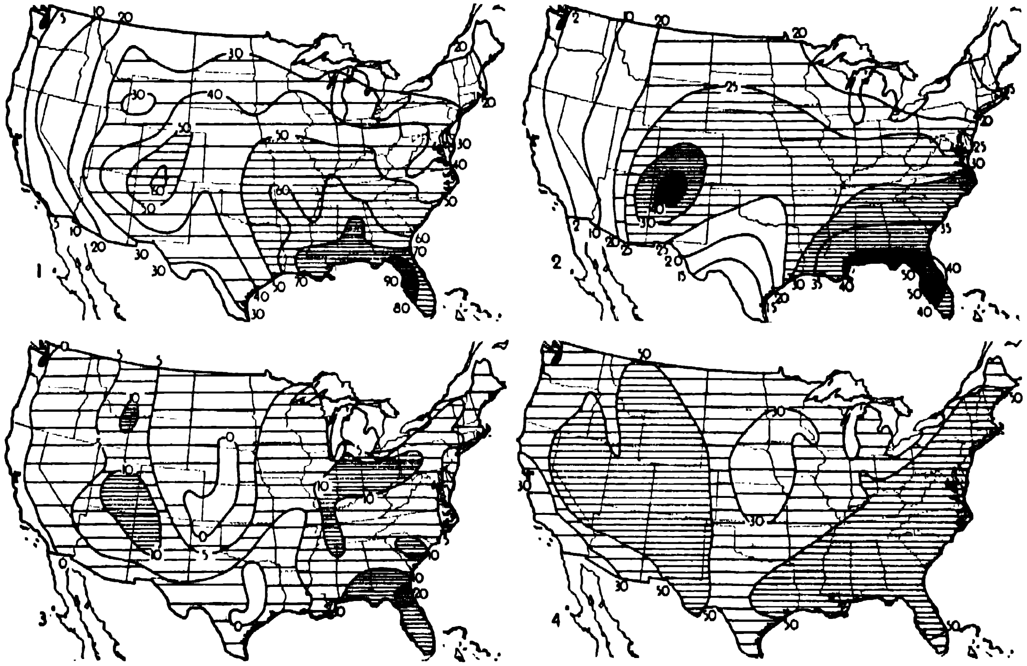
Although thunderstorms are predominantly convectional, detailed studies have proved that convection sufficiently intense to cause thunderstorms almost never occurs except where the air is disturbed by at least a feeble cyclonic disturbance, or an air-mass movement. Many of the more intense thunderstorms, especially of the tropics and subtropics, are parts of tropical cyclones.

The average summer thunderstorm in the eastern half of the United States yields somewhat more than half an inch of rain; many yield an inch, and frequently one yields more than two inches. An exceptionally intense thunderstorm may yield more than five inches, and a considerable number, especially in the South, have yielded more than ten inches.

The value of the thunderstorm rainfall depends, of course, on when it comes. If, as frequently happens, it comes when it is badly needed, a single thunderstorm may increase the value of the corn yield to the extent of several thousand dollars per square mile. In late August 1947, when this article was being completed, the drought in the Corn Belt was each day reducing the corn yield an estimated 15,000,000 bushels, when corn was selling for $2.40 per bushel. A "good" rain in the critical period of corn growth, following a prolonged dry, hot spell, increases the average yield at least several bushels per acre. Since in the Corn Belt each average square mile has about 200 acres of corn, and since the average American thunderstorm yields rain to a few thousand square miles, a single thunderstorm, under these special conditions, may well be worth millions of dollars. Whenever the Midwest lacks summer thunderstorms, it receives too little rainfall for corn to thrive.

THE foregoing summary suggests that it is appropriate that thunderstorms be seriously studied. Recently, a two-volume mimeographed report on thunderstorms was issued by the Federal government (*Hydrometeorological Report No. 5,* by A. L. Shands and five others), through cooperation between the Weather Bureau, Corps of Engineers of the War Department, and the Soil Conservation Service. Seven of the eight maps of this article are adaptations of maps in that report.

The present article calls attention to this highly technical, costly Federal report (331 pp., plus 155 illustrations) and presents some of its conclusions. That this article is written by a geographer rather than by a meteorologist reflects the fact that geographers desire to have environmental influences more widely understood. Meteorologists, on the other hand, tend to be chiefly interested in technical phases and write mainly for other meteorologists or for engineers.



FREQUENCY OF THUNDERSTORMS IN THE UNITED STATES

Map 1: AVERAGE ANNUAL NUMBER OF THUNDERSTORM DAYS. *Map 2*: AVERAGE NUMBER OF SUMMER THUNDERSTORM DAYS. *Map 3*: DAYS PER YEAR NORMALLY HAVING TWO THUNDERSTORMS. *Map 4*: PERCENTAGE OF THE SUMMER THUNDERSTORMS THAT OCCUR IN THE AFTERNOON.

Map 1 shows the average number of calendar days per year on which thunderstorms occur in each locality. It is based on the data for 40 years (1904–43) gathered at 217 first-order Weather Bureau stations. It reveals a regional contrast from less than 5 thunderstorms per year along the Pacific Coast to more than 90 a year in part of Florida. East of the Rockies, there is a general southward increase, which reflects the increases in total radiation received, and hence contrasts in the intensity of convection; significant, also, in explaining this general increase southward is the South's longer warm season—thunderstorms occur there even in winter. In the southern two-thirds of the country, there is a general eastward increase in thunderstorms, except that the southern Rockies have relatively many. The general eastward increase reflects the greater moisture content of the air associated with the greater availability of moisture from the Gulf of Mexico. West of the Rockies, where the summers are dry, there seldom is sufficient atmospheric moisture to induce by its condensation the rapid convection required to cause thunderstorms. This is important because thunderstorms feed upon the latent energy freed by condensation.

Map 2, showing the number of summer thunderstorms, indicates that a large share of the thunderstorms occur in the summer quarter. Near the Canadian border, except in the West, two-thirds or more occur in summer; midway between Canada and the Gulf of Mexico, roughly one-half occur in June, July, or August. Along the Gulf of Mexico, however, somewhat less than one-half occur in that quarter. Likewise in the Pacific States less than half the small annual total occur in summer. In the western mountains most of the thunderstorms occur in summer or near its beginning or end. Map 2 (original) was compiled from the data for each of the summer months.

It frequently happens that there is a thunderstorm in the afternoon and another at night. Map 3 reveals that there is considerable regional contrast in the prospects of 2 thunderstorms in a calendar day. There very seldom are 2 along the Pacific Coast, in the central Great Plains, or in eastern Texas. Conversely, on 10 or more days in an average year there are 2 thunderstorms in a great part of the eastern third of the country and in the Rocky Mountains. Comparison with Map 1 shows that in Ohio, for example, nearly a fourth of the days that have a thunderstorm have 2 of them. Thus, in the areas shaded darkest on Map 3, whenever conditions are favorable for an afternoon thunderstorm, there is considerable prospect that there will be another that evening. The reverse is true of the central Great Plains.

Maps 4 and 5 show the percentage of the summer thunderstorms that occur in the evening or afternoon. Map 4 shows that in more than half the country somewhat more than half the thunderstorms occur in the afternoon (noon till 6:00 P.M., local time). Near the center of the country, however, and in the Southwest, less than 30 percent of the thunderstorms occur in the afternoon.

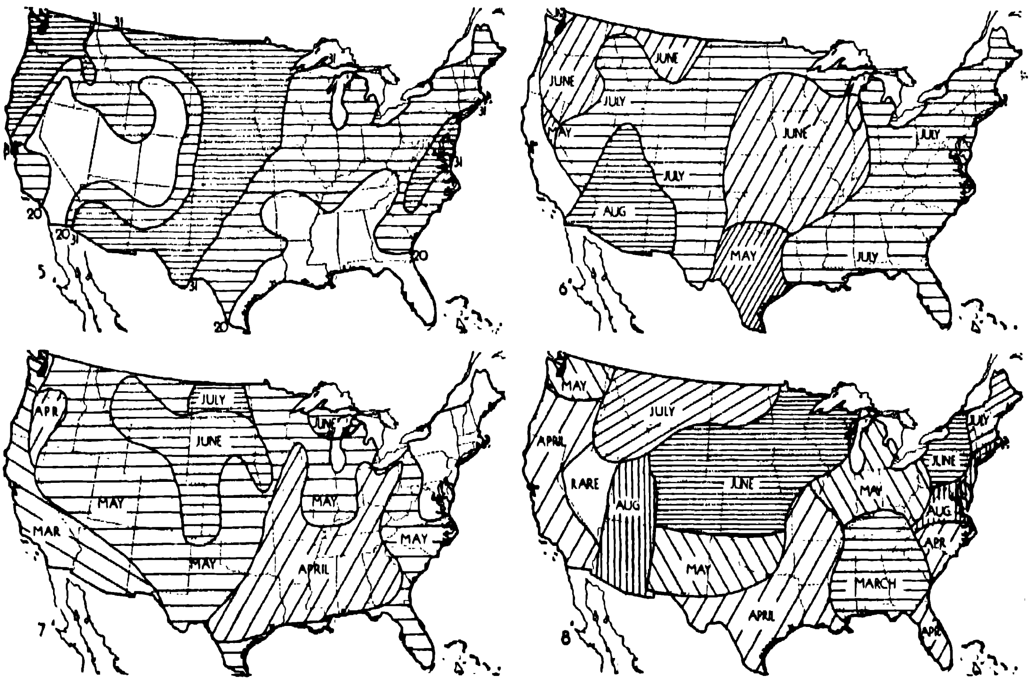
Map 5 shows that in no part of the country do half the thunderstorms occur in the evening quarter (6:00 P.M. to midnight, local time). In large regions in the West and Southeast, less than one-fifth of the thunderstorms occur in those 6 hours.

When the percentages of the annual total of thunderstorms that occur in the two periods dealt with by Maps 4 and 5 are added, it is seen that for most of the country only a small fraction—less than one-fifth—of the thunderstorms occur between midnight and noon. The chief exceptions are along the Gulf Coast and in southern Florida, where nearly one-half occur in that half of the 24 hours, and in Wisconsin, where about 40 percent occur then. Coastal California also has approximately one-half of its very few thunderstorms between midnight and noon. These exceptional areas approach somewhat the condition characteristic of the ocean, over which most thunderstorms occur during the night, nearly always shortly before dawn. At that time, as a result of nocturnal cooling of the air the vertical temperature contrast is greatest, since the water's temperature remains almost unchanged. Although eastern Wisconsin is far from the ocean, its temperatures are notably influenced by near-by lakes, especially in warm weather, the time when most thunderstorms occur in that general region.

Map 6 shows the month during which most thunderstorms occur normally. July has most in the eastern third of the country and also in most of the western half; June leads near the center of the country and in the Northwest; August leads in the Southwest; but May leads in Texas.

Hailstorms, although always occurring in thunderstorms, generally are most frequent earlier in the year than the month of most thunderstorms. In the lower Mississippi Valley, for instance, hailstorms are most numerous in April, thunderstorms in July; in Arizona the gap is even greater: March versus August. This gap reflects the greater melting of the downfalling hail in the later month. Observations made from airplanes and on mountains reveal that hail often forms during the warmest months, but generally melts before reaching the valley floors. The latitudinal shift in the month of most hail shown by Map 7 is interesting. (The section at the northeast is left blank on Map 7 because it is complicated, with the month of most hail in various areas in April, May, June, July, and October.)

The final map (8) shows the month of most tornadoes, which, fortunately, are a rather rare feature of thunderstorms, although practically all occur in thunderstorms, and hence are discussed in the comprehensive report from which this map was adapted. The general latitudinal shift of maximum frequency mentioned for hail occurs also for tornadoes, with interesting modifications. Tornadoes are relatively common earliest (March) from Florida to Kentucky. To the northwest of that area, they are most common in April, May, and June. July is the month of most tornadoes for most of New England and from North Dakota to Idaho. August is the leading month in Virginia, along the southern coast of New England, and in Arizona. Comparison of Map 8 with Map 6 shows that in general the month of most tornadoes comes the earlier. The chief exceptions are the areas where the same month leads for both, namely, New England, North Dakota, Arizona, and a central area. In general, the month of most hailstorms (Map 7) comes a month later than the month of most tornadoes, although hail occurs near almost all tornadoes. The same month leads for both hail and tornadoes in the Midwest. In Montana tornadoes are more numerous a month later than is hail, but tornadoes are so rare there that the evidence is not statistically strong. The causes for these regional differences in the month of most frequent occurrence of tornadoes are correlated with differences in the depth of warm air. Tornadoes occur only when the lower air is relatively warm and when a layer of cooler, drier air is not far above the warm surface layer.



THUNDERSTORMS, HAILSTORMS, AND TORNADOES

Map 5: PERCENTAGE OF THE SUMMER THUNDERSTORMS THAT OCCUR BETWEEN 6 P.M. AND MIDNIGHT. *Map 6*: MONTH NORMALLY HAVING THE MOST THUNDERSTORMS. *Map 7*: MONTH NORMALLY HAVING THE MOST HAILSTORMS. *Map 8*: MONTH NORMALLY HAVING THE MOST TORNADOES.

THE statement ascribed to Mark Twain that "everyone talks about the weather, but no one does anything about it" is no longer true. During recent months, numerous local rains have been induced by the dropping from airplanes to the top of rain clouds of a few dollars' worth of frozen carbon dioxide, or dry ice. Although numerous attempts have been made on other sorts of clouds, considerable amounts of rain have so far been induced only if the dry ice is dropped into well-developed thunderheads. Hence, thunderstorms have become significant in yet another respect, by contributing success to man's long-futile efforts to make rain. In thunderheads, the rising air currents that produce the head are almost making rain. But it frequently happens that the cooling caused by the rapidly rising air currents is not quite sufficient to produce enough rain to fall to the ground. Repeatedly, especially during a dry spell, a promising thunderstorm yields little rain. If a quantity of dry ice is dropped into the top of the thunderheads, the sharp cooling it causes produces enough condensation to intensify the convectional rise of air, thus starting a chain reaction that results in considerable rain. At a time when rain is badly needed, the financial returns may be of significant value to a stricken countryside.

Since in the United States thunderheads generally move roughly eastward at somewhat irregular speeds, the dry-ice-induced rain may not fall just where most desired. Its amount depends chiefly not upon the amount of dry ice dropped but upon how favorable local atmospheric conditions are for rain formation. Such induced rain does not deprive some other near-by area of rain. Instead, it is probable that areas subsequently passed over by the cloud, to the east of such a man-made shower, will get more rain than they would have received otherwise. This is partly because the atmosphere contains large supplies of moisture, usually far more than enough above each locality to yield more than an inch of rain. The difficulty is not at all the lack of moisture, but the fact that the air retains its moisture until forced by sufficient cooling to drop it.

The dropping of dry ice on a thunderhead produces rain, not hail. Indeed, it is scientifically highly probable that dry ice dropped into a thunderhead from which damaging hail is falling will cause a reduction of the hail. This is because the dry ice produces a more simple reaction, not the convectional turmoil characteristic of a hailstorm. This turmoil is evidenced by the several layers in most hailstones, indicating that they were carried repeatedly by violent rising currents to great heights.

Since thunderstorms are a major source of rainfall in the United States, and since they are harmful as well as useful, increased knowledge of their nature, occurrence, and possible control is highly desirable.

ON THE DEATH OF A WILLOW IN THE HURRICANE

JULIUS SUMNER MILLER

I knew her in her youth.
I saw her first a sapling,
Young, slender, graceful,
Full with the will to live.
The elements were kind;
The rain fell, the sun shone, the wind blew—gently.
She prospered in God's earth,
Her roots went down, and through them was drawn up
The nurture of the soil.
On this she fattened and her branches spread.
Ofttimes I lingered underneath her limbs,
And sheltered from the blazing sun I looked up
Through the latticework of her leaves,
Geometry against the sky.
On windy days she wrestled with the wind,
Each knowing the other's gentleness,
The wind did not with anger blow
Nor did the tree defy.
It was a sporting game they played.

Years passed. Her girth increased.
Once I could encircle her with hands alone,
And then her bark was smooth and green.
But now her trunk was huge and gnarled,
Her skin wrinkled with the years,
Only her tresses retained the flimsiness of youth,
In every leaf she breathed the joy of living.

And then came an evil day.
For reasons men do not know
God saw fit to bespoil his own handiwork,
To bring ruin to his own achievement and to man's.
The skies darkened, the wind blew, the rain fell;
The storm had come and with it terror in men's hearts.
First, courageously, with some defiance, the willow lashed her limbs;
This was not the playful game of other days;
With every beating she beat back.
She flailed her arms, her branches creaked,
Wrenched first this way, then that.
The torture was too great;
A thousand deaths she lived, and died but one.
From the shelter of the earth her roots were torn away;
Her glory gone, a prostrate form she lay, lifeless,
Her tresses stilled.

Today men came with saws and axes.
Today the wind is silent.
With solemn tribute, as if in prayer,
Nature is quiet in the face of Death.

SCIENCE ON THE MARCH

THE ROLE OF THE RUMEN IN THE NUTRITION OF THE RUMINANT

THE ruminant has a true stomach and in addition three compartments geared by nature to handle fibrous feeds such as pasture grass, hay, straw, and silage. The first stomach, or rumen, has a very large capacity, whereas the second stomach, or reticulum, is nothing more than a pouch of the rumen. It is sometimes referred to as the "hardware" stomach, since heavy objects such as wire, nails, stones, and even gold watches and diamond rings, sometimes swallowed by the large ruminants, are collected in this pocket. The third stomach, or manyplies, presses the feed, but its contribution to the animal is not clear. The rumen and reticulum make one large compartment, sometimes referred to as the fermentation vat, where feed is fermented by fauna and flora. The fermentation vats in animals other than ruminants are the caeca and large intestines, but these animals, such as the pig, horse, and chicken, are handicapped when it comes to handling tough fibrous feeds. The ruminant, on the other hand, is well suited for the job, because of the large size and the position of the rumen. The location of the fermentation vat at the head of the digestive tract gives the bacteria that do the fermenting priority for the dietary factors consumed by the animal. Then, too, the products synthesized during fermentation are efficiently digested and absorbed in the small intestine. The flora in the caecum and large intestine exist on constituents that fail to be digested and absorbed before reaching those portions of the digestive tract.

It is apparent that the efficient fermentation vats of cattle and sheep make possible the utilization of one-half to one billion acres of ranch land in the United States, which otherwise would yield little if any human food. An additional one-half billion acres of land are devoted to pasture, of which only one hundred million acres are plowable. Also, seventy five million acres are devoted to hay crops for livestock feed. These add up to an enormous amount of highly nutritious and palatable food for man. In utilizing these crops, the ruminant contributes to the conservation of one of our greatest national resources—soil fertility. Grasses aid in preventing soil loss. Pasture and hay crops used in rotation, when properly fed to ruminants, aid in building up the soil—thereby contributing to increased yields of grain.

The rumen, which does so much for man, is at last receiving the attention of a few scientists in several different countries. Many different species of bacteria and protozoa and pseudo yeasts have been found in rumen contents.

The feed and water consumed by the mature ruminant pass directly to the rumen. The rapid rate of digestion of a feed of hay is one of the wonders of nature. As soon as the hay reaches the rumen, bacteria begin to bring about disintegration of celluloses and so-called hemicelluloses, so that at the end of six hours most of the feed of hay has been digested. The presence of compounds such as lignin is believed to interfere with the digestion. Highly lignified feeds, at least, such as wheat straw, have very low coefficients of digestibility for cellulose, whereas the cellulose in young grass, beet pulp, and delignified wood pulp is highly digestible.

There are no digestive enzymes secreted by the ruminant, present in the saliva or in the rumen. A few digestive enzymes may come into the rumen with the feed, but these are believed to play an insignificant role in rumen digestion. Although several species of protozoa are present, their function if any is not known. Pseudo yeasts may play a significant role in rumen digestion in some species of ruminants but not in others. Bacteria do most of the work of rumen digestion.

Many species of bacteria have been found in the rumen. A group referred to as iodophiles, because they take the iodine stain, are the best known. The iodophiles may be small cocci, large cocci, small rods, or large rods. These bacteria penetrate the roughage particles and secrete enzymes that digest the feed for the bacteria. The bacteria absorb these materials and convert them into their own tissues. These bacteria are aided by the host, which, after a good meal, finds a place to rest and chews its cud. The regurgitation of the coarser pieces of roughage enables the ruminant to macerate it by the chewing process. This gives the roughage-loving bacteria a better opportunity to digest it.

The rumen is often looked upon as a storehouse for roughage or a place for digestion to take place. Actually, digestion and synthesis are taking place simultaneously. A few of the dietary factors synthesized in the rumen are: vitamin K, thiamine, riboflavin, niacin, pantothenic acid, biotin, fat, carbohydrate (starchlike compound), protein from nonprotein nitrogen, and unknown dietary factors. The reason that cow's milk is always a good source of most of the members of the vitamin-B complex, regardless of the ration fed, is that they are synthesized in the rumen.

Numerous investigations have shown that rumen bacteria are able to convert urea nitrogen into protein of fair quality. This explains why it is not necessary to consider the quality of protein in formulating rations for ruminants. It is of interest, however, that when the host is fed timothy hay alone, supplementation with urea was without value until starch was added. The replacement of the nitrogen in urea with an equal amount of nitrogen as fish meal increased nitrogen utilization. The addition of the amino acid methionine to the ration of lambs increased the value of the urea as a supplement to a low-protein ration.

It is apparent that the ruminant does not receive the greater part of its nutrients directly from the feed it eats, but from the digestion and absorption of a countless number of food-laden bacteria after they have handled the feed consumed by the host. There is an intriguing theory that postulates the consumption of the nutrient-filled bacteria by protozoa and that the protozoa digest the bacteria and store the nutrients in their own bodies. The host then digests the protozoa. The evidence supporting the theory is that as soon as the animal is fed, the bacterial population increases at an enormous rate, but the protozoal population does not increase until the bacterial population has reached its peak. It is believed that the protozoa wait around until the bacteria have converted most of the feed into their own bodies; then the protozoa fill their stomachs with bacteria. If this theory is ever substantiated, then the animal would be getting its nutrients at thirdhand.

There is some evidence that the vital role of cobalt in animal nutrition may depend on the need of certain bacteria in the rumen for this element. When cobalt is injected into the blood of cobalt-deficient ruminants, the response is very slow, if any. On the other hand, the oral administration of cobalt results in an increase in appetite on the third day, and the recovery is dramatic.

Sometimes cattle suffer from the disease poikilocytosis, characterized by lack of appetite associated with abnormally shaped red corpuscles. The administration of vitamin B_6 brings about recovery. Cows with large open rumen fistula also develop this disease. It is assumed that the rumen flora may under certain conditions lose the power to synthesize vitamin B_6. Occasionally rumen fistula cows lose their appetite and do not respond to cobalt or vitamin B_6 supplementation. In these cases rumen transfusions—the addition of about 50 pounds of normal rumen contents to the rumen of sick cows—have brought about recovery.

The by-products of rumen fermentation are organic acids—principally acetic, propionic, and butyric—and gases—carbon dioxide and methane. It has been estimated that about three-fourths of a pound of acid, calculated as acetic, is produced per day in the rumen of a cow. This amount of acid, most of which is produced during the first few hours after feeding, must be neutralized in order to maintain optimum pH for rumen fermentation. Two methods are used: A cow produces about 120 pounds of saliva daily, which has a pH of 8 and is capable of neutralizing acid. The saliva, however, has other functions: one is to aid in maintaining a fairly constant water content in the rumen—usually 75–85 percent; it also is a splendid wetting agent and contains an adsorbent responsible for the historical expression referred to in poetry and prose: "The sweet breath of the kine." It is of interest that some of the salivary glands of the ruminant work continuously. The other method of handling acids, and the only one available in the caecum and large intestine, is absorption into the portal circulation through the walls of these fermentation vats.

The gases, principally carbon dioxide and methane, produced during fermentation are eliminated by eructation through the esophagus and to a minor extent by absorption through the rumen wall. Occasionally, however, for some unknown reason, gas elimination stops and bloat takes place.

The advantage of fitting the feed to the needs of the desirable types of rumen bacteria is indicated by the results of several experiments. The feeding of sugar feeds, such as molasses to ruminants, results in a decreased digestibility of the fibrous part of the ration, which is believed to be due to the preference of cellulose-digesting bacteria for sugar. In another experiment, more riboflavin was synthesized by the rumen flora when finely ground corn supplemented a ration of hay than when whole or coarsely ground corn was fed. The possibility of stepping up the tempo of rumen fermentation of roughages is apparent. More knowledge of rumen bacteriology and physiology may revolutionize the nutrition of ruminants, thereby increasing the efficiency in the conversion of roughages to human food.

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ITINERARY[*]

FIRST ANNUAL INTERNATIONAL PHOTOGRAPHY-IN-SCIENCE SALON

CLEVELAND HEALTH MUSEUM Cleveland, Ohio	March 8–29
Science Illustrated New York City	April 6–16
BROWN UNIVERSITY and PROVIDENCE ENGINEERING SOCIETY Providence, Rhode Island	April 22–May 6
GAMMA SIGMA EPSILON University of Florida Gainesville	May 13–June 1
THE JOHN CRERAR LIBRARY Chicago, Illinois	June 8–June 21
AUBURN CAMERA CLUB Bureau of Animal Industry U.S.D.A. Auburn, Alabama	June 28–July 12
AMERICAN OSTEOPATHIC ASSOCIATION ANNUAL CONVENTION Boston, Massachusetts	July 19–23

[*] Reservations for showing the 1948 Salon pictures may be made now. The 1947 pictures will also be available after July 30.

BOOK REVIEWS

SCIENCE AND THE MILITARY

New Weapons for Air Warfare. Joseph C. Boyce, Ed. xvii + 292 pp. Illus. $4.00. Little, Brown. Boston. 1947.

THIS is one of a series of books giving the history of the Office of Scientific Research and Development. The series was introduced by a volume summarizing the activities of the whole organization under the title *Scientists Against Time,* by James Phinney Baxter, 3rd. The remaining seven volumes, of which *New Weapons for Air Warfare* is the first, are grouped under the broadly inclusive title, "Science in World War II." This book gives a running account of the activities of Divisions 4, 5, and 7 of N.D.R.C. and Section T of O.S.R.D.

In late 1940, when England and France were first experiencing the sting of the youthful Nazi Wehrmacht, and the war with Japan was still in its diplomatic phase, the scientific world was rich in knowledge born of research virtually ready for engineering into practical application. The principles of remote control by radio had been demonstrated on toy wagons and flying aircraft by the U. S. Navy. The measurement of distance by timing the flight of radio pulses to and back from the ionosphere was an established procedure, and the more precise location of much smaller objects in three coordinates by similar methods had been demonstrated first by the U. S. Navy, and later and almost concurrently, by the U. S. Army Signal Corps and the British Royal Air Force. Nuclear fission, long predicted by theoretical physics, had been demonstrated in the laboratories of several universities, and the principles of atomic combustion, the basis of the atomic bomb, were known. The necessity for pressing this knowledge into the immediate service of the country for war provided the impelling motive to drive research scientists out of their ivory towers and into the exacting slavery of engineering to specifications. Thus was born the National Defense Research Committee, and later, the child that was made its parent, the O.S.R.D.

New Weapons of Air Warfare contains within its pages much of the drama of personal adjustment incident to the admission of civilian scientists to the military councils of war, to the painful restriction of research personnel to the inexorable demands of engineering specifications, and to the equally painful imposition of the "scientific method" on military personnel conditioned to less scientific precedent. It shows what happens when a tremendous scientific potential, consolidated by patriotic motives, is suddenly released through removal of financial restrictions; how industry may thus be mobilized in a common effort; how duplication of effort may result from planned multiple attack on a problem as insurance against failure of any one attempt, as well as from lack of coordination due either to poor management or to the intellectual isolation of military secrecy; how costly projects are necessarily undertaken on a gamble, the breaks determining whether decision to go ahead was a "stroke of genius" meriting awards and high honors, or an "idiotic blunder" calling for congressional investigation. It gives an inside look at a small part of the fantastically deadly war potential that was about to be released upon the hapless residents of Nippon when the two atomic explosions presented to their war lords a face-saving occasion for surrender.

Multiple authorship is apparent more from variations in literary style and quantity of detail than in outline and organization. In particular, the superlative standards of the first few chapters are never approached in the rest of the book. Although it cannot be said that this does not detract from the main purpose of the book, it is at least partly compensated by the interesting nature of the information presented. If some of the writers in their zeal showed more tendency to repetition than to professional fraternization, it is to be remembered that the achievements reported truly are phenomenal, and that the book is intended to chronicle the effort of certain divisions of O.S.R.D., and not of the whole country. Most discordant of all, however, is the title itself, since the emphasis of the book is more on antiair than air warfare.

By far the most outstanding feature of the book is the editor's postscript. Here is a gem of purest wisdom, profoundly true and eloquently presented as an appeal to heed the vital lessons whose costly demonstration is so well documented in the pages of this book.

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LEGEND VS. FACT

Alsos. Samuel A. Goudsmit. xiv + 259 pp. Illus. $3.50. Schuman. New York. 1947.

"THE myth of Nazi military supremacy was smashed forever by the might of the Allied armies, but the legend of German scientific supremacy lingers on." Thus the author of the book under review introduces his Preface. He later gets evident satisfaction out of presenting the facts that "establish beyond any possible argument" that such German scientific supremacy is *only a legend*.

However, he is less concerned with the fact that "American science" in the field of nuclear physics is found to be superior to "German science" than he is desirous of assessing the factors back of this reversal of the scientific ranking of these two nations.

"Alsos" was the code name for a commission of scientists sent by General Groves to investigate scientific war research in the universities of southern Italy. That unit's success prompted the organization of a larger and more inclusive unit to cover all scientific war research in Germany. The unit was to move into France and Germany as fast as Allied armies occupied those territories. The commission was a joint army-civilian group, headed by Colonel Boris T. Pash for the Army and Dr. Samuel A. Goudsmit for the scientists. Although every aspect of science that related to the war was to be inventoried, actually the greatest concern was with regard to German progress toward the assembly of an atomic bomb.

Headquarters were first set up in Paris, but it soon became evident that effective prosecution of the search for atomic missiles could not be accomplished by such a "remote-control" plan of operations. Very soon operators were in Belgium, Holland, and southern France, running down leads that suggested activities having to do with atomic fission or chain reactions.

The first real "find" was at Strasborg, where they made their initial capture of scientific personnel working on the "uranium problem." At Strasborg the Germans were evidently trying to transform a French institution into a model German university.

The next point of interest and value to the commission was Heidelberg, with its university and Drs. Bothe and Kuhn. At Thüringen they found a "pile" laboratory; at Hechingen, Otto Hahn and Von Weizsacker were picked up, but not Heisenberg, the top-notch German physicist. After the fall of Munich, not only Heisenberg, but the remainder of Germany's key nuclear physicists were at the disposal of the commission.

The commission from the first had assumed that the Germans had a two-year lead in the race to implement the energy from atomic fission into an instrument of fabulously destructive power, for Otto Hahn had revealed the reality of atomic fission, and another German had suggested the probability of a chain reaction in 1938. This assumption received an agreeable negation when the key men had been interviewed and all documents and materials assessed. The facts were found to be:

> Months after our scientists had established . . . the feasibility of the atomic bomb, the Germans were still only talking about the "uranium problem" and the possibility of constructing a "uranium machine."
>
> They did not know how to produce a chain reaction in a uranium pile. . . . They did not know how to produce plutonium.

To make up for loss of face in their failure to do what had been successfully done by others, the author says "a theme song" was agreed upon by the interned German nuclear physicists. The refrain of that song was to be: "Germany worked upon the uranium problem for peaceful uses only; the Allies for purposes of destruction."

In the author's words, the major purpose of his book is to consider "why German science failed where the American and British succeeded . . . [or] that science under fascism was not . . . the equal of science in a democracy." The latter half of the book is devoted to that purpose. In brief, the conclusions are as follows:

> [The assumption] that totalitarianism gets things done where democracy only fumbles along . . . is [far] from the truth. . . . The failure of German nuclear physics can, in a large measure, be attributed to the totalitarian climate in which it lived. . . .
>
> Complacency was one of the worst enemies the German scientists had. Confident of the superiority of German science, they assumed no nation could equal them. . . .
>
> The interference of politicians in the affairs of science . . . the appointment of party hacks to important administrative posts . . . [were] grave errors. [They knew not the wisdom of] leave science to the scientists. . . .
>
> The stubborn blindness of dogma and the free inquiring spirit of science do not mix.

By Gestapo "witch-hunting," which drove "Jewish science" of theoretical physics from the universities, they reduced potential man (brain)-power available for effective work upon the uranium problem.

A "Heil Heisenberg" attitude upon the part of other German physicists restricted maximum utilization of creative production from younger colleagues. "Science is not authoritarian, nor can scientific thought be dominated by a boss."

These factors as contributors to Germany's ill-success are particularized for American science in a final chapter, captioned It Can't Happen Here. The irony of this title is quite apparent before the reader has scanned the first page of the chapter.

It should not be inferred that the book lumps all German scientific participation in the war effort into the same category with nuclear physics. Credit is given for the work done on aeronautics, on submarine techniques near the war's end, and on the V-2 bombs by the army.

For the benefit of the nontechnical reader, an Appendix, An Outline of the Uranium Problem, interprets the vocabulary of nuclear physics.

If, in the course of reading this book, any person senses, upon the part of the author, a note of subjective glee over the failures of the German Nazis, that reader should recall that in the course of his sleuthing for Alsos, Dr. Goudsmit learned "the precise date my father and blind mother were put to death in the gas chamber [in a concentration camp]. It was my father's seventieth birthday."

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CIVIL ENGINEERING

Surveying Instruments. Edmond R. Kiely. xii + 411 pp. Illus. $3.00. Bur. of Pub., Teachers Coll., Columbia Univ. New York. 1947.

SELDOM does a book such as *Surveying Instruments* find its way into the ranks of engineering and mathematics texts. The scholarly excellence of its presentation and documentation alone would have warranted its choice as the yearbook of the National Council of Teachers of Mathematics. Such perfection of presentation makes the history of surveying and its instruments doubly absorbing, even though the history of man's advances in the art of civil engineering as told by the author fascinates the reader in its own right.

Awareness of a code of ethics and adherence to it are counted the distinguishing marks of a professional man. Consciousness of such a status is gained chiefly through the justifiable pride of being a co-partner in a service to mankind that spans the centuries with its scholarly and applied wisdom carefully passed from hand to hand, with each participant striving to add to it. Lack of high ideals and moral stature in those who practice professions lower them to the level of devices used for personal gain. This book provides the spark that will build the fire of professional pride in those who study it.

The value of the book as a text is only heightened by the progressive development of surveying instruments such as the leveling rod, quadrant, theodolite, and the transit from their earliest beginnings in Egypt, China, and Babylonia down through the Greek, Roman, Medieval, and the Renaissance periods. One achieves a new viewpoint toward what otherwise become commonplace "tools of the trade."

A practical exposition on geometry in schools, followed by a series of well-chosen and well-described exercises in the applications of geometry and trigonometry to simple surveying, completes the text.

In his thirty years of engineering, teaching, and professional experience, the reviewer has never encountered a book that he could recommend so wholeheartedly as "must" reading for all beginning engineering students, teachers of mathematics, and the engineering profession at large.

Earle S. Hannaford

Long Lines Department
American Telephone and Telegraph Company
New York, New York

MODERN CHEMISTRY

Small Wonder: The Story of Colloids. Gessner G. Hawley. ix + 220 + x pp. Illus. $3.50. Knopf. New York. 1947.

THIS book fills a genuine need in the field of popular science by describing the scientific basis for the behavior of many of the colloidal systems that one meets everywhere in the home, in the factory, and in the hospital. It seeks to inform rather than to baffle and amaze, as is so often the case with "scientific" writing for the general public; but at the same time it is not in the least dull or stodgy, since it furnishes explanations of a sort for so many of the wonders of modern science.

After a fine group of photographs indicative of the scope of the subject, there is a remarkably sound and well-balanced presentation of the fundamentals of colloid science in Chapters 1 through 5 and 11, although in Chapter 5, dealing with the origin and influence of electrical charges, the author all too frequently steps across the narrow boundary between simplification and error. Six additional chapters describe the behavior of representative colloids: milk, rubber, blood, genes, and surface-active agents. These chapters are, in general, well written, although there are a somewhat larger number of technical errors than might have been anticipated, such, for example, as the statements that the most effective industrial catalysts are metal powders, that Conant and Kettering are among the foremost students of the process of formation of proteins in plants, and that solid particles repel water and soft sticky ones attract it. There is, nevertheless, a fine perspective here of our developing knowledge of these and similar systems. Although it would be impossible in a book of this size to devote a full chapter to each of the important colloidal systems, it seems a pity that cellulose, clays, and lubricants could not also have been included.

The chapter on Colloid Chemistry and Life does an admirable job of showing just how the definition of life in terms of capacity to reproduce fails to distinguish adequately between animate and inanimate systems. The terminal chapter on Design for Science might better have been omitted. Who can foresee future advances in any science? It is easy to detail outstanding problems whose existence is recognized—and not all of them concern the origin of life—but it is characteristic that science in its advance discovers *new problems* and then solves them for the ultimate benefit of all mankind.

This book can be heartily recommended to the intelligent general reader, who will be able to derive much satisfaction and enjoyment from it in an increased capacity to interpret his own observations of common colloidal phenomena.

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FAR FROM THE IVORY TOWER

Combat Scientists. Lincoln R. Thiesmeyer and John E. Burchard. Alan T. Waterman, Ed. xii + 412 pp. Illus. $5.00. Little, Brown. Boston. 1947.

SOME of the most brilliant scientific (and a few of the most competent military) brains in this country realized by the end of World War II that the utilization of scientists in staff functions such as operational research and analysis and scientific intelligence is essential to the formulation of well-rounded strategy and tactics. Furthermore, they believe any future emergency will call for a much greater use of military-scientist teamwork in planning and in the field in order to achieve maximum benefits from the ever more complicated weapons under development. On the other hand, many of our military leaders are still far from convinced of the soundness of this reasoning. It is thus particularly unfortunate that *Combat Scientists* does little to present properly, or strengthen, the case for these vital functions of the scientists.

The book is one of the series on the history of the Office of Scientific Research and Development in World War II. Part I of the book, written by Mr. Thiesmeyer, is entitled History of the Office of Field Service. It is dull reading. The detailed account of administrative matters and the short-circuiting of red tape will interest few. The reader is struck by a certain inferiority complex of the Office of Field Service, as brought out in the book by the thrilled use of the words "high level" whenever dealings with anyone above the rank of colonel are mentioned. It does not seem to occur to the author that this was one of the faults of the whole organization. Moreover, the reader is left with no real understanding of what operational research is all about, since not one complete example is given. Not one table or diagram is used. Beautiful examples of the power of operational research do exist. Some way could certainly have been found, within the security regulations, to give the public and rising young military leaders a grasp of this important subject.

Part I belies its title in that it is not truly a history. It gives no consideration to the military side of the picture. Consequently, it furnishes the military student no means of evaluating the thinking behind the many struggles that ensued as the armed forces wrestled with the question of the wisdom of admitting scientists to staff work in Washington and staff and field work in the theatres of war. No mention is made of the career risks assumed by the handful of farsighted officers who pioneered in the introduction of civilians into war planning. The failure of the book to give the rising generation of officers the background history of the valiant action of these men within the services, which led up to such disarmingly simple statements as "at the request of Admiral Blank such and such was initiated," leaves a tremendous void. This makes necessary a whole new military educational process that will have to be undertaken along such lines by the Research and Development Board.

A particularly bad paragraph is to be found in the Lessons for Tomorrow chapter. It recommends the establishment of an organization such that in the event of a national emergency any research scientist could be drafted from his work and sent to theatres of war for field assignment at the mere request of the Field Service office, which request would

> carry the force of directives from the top. . . . Progress of research and development, even on what were considered crash programs this time, was far too slow to warrant keeping our best brains at work in the laboratory on things that might not be ready within the few weeks or months the next war lasts.

If there is any lesson we should have learned from the Germans in this war, it is that taking scientists out of research laboratories just because it is believed the war "will be a short war" is the best way to lose our homeland. Consider the atom bomb, microwave radar, and biological warfare from this standpoint.

Part II consists of a live account by Mr. Burchard of two endeavors undertaken by the Office of Field Service scientists. The problem of furnishing navigational aid to amphibious landing operations is shown to be the type of job on which scientists could be eminently successful despite heartbreaking delays. Many lives were saved through their unsparing efforts. By way of contrast, the second example chosen is that of a complete failure. The task in this instance was to develop a method of clearing enemy beaches of obstacles to landing operations, such as concrete piling, rails, etc. Mr. Burchard weaves skillfully the cobweb of difficulties and lack of ideas that enmeshed this undertaking. In the end, men had to swim in to enemy beaches and sacrifice lives in placing charges to clear the way for their buddies in landing barges.

It is recommended this book be read widely. It is hoped that out of such study there will arise a more scholarly analysis of the challenging and still-unsettled problem of the optimum method of utilization of scientists in military staff planning and in theatre-of-war assignments.

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IN ENDLESS FILE SHALL LOVING SCHOLARS COME

Louis Agassiz, Scientist and Teacher. James David Teller. xi + 145 pp. (paper). Graduate School Studies, Educational Ser., No. 2, Ohio State Univ. Columbus. 1947.

AN AURA of intermingled fact and fable always hovers over our national heroes, great or small. Louis Agassiz was no exception, but there is no fiction to his greatness, either as a scientist or as a teacher; and Dr. Teller, in this scholarly yet readable study, has performed a service in summarizing and re-emphasizing Agassiz' contributions to science and education. Its publication nearly coincides with the centennial of Agassiz' coming to America and with the diamond jubilee of the establishment of his famous marine biological laboratory on Penikese Island near Woods Hole, Massachusetts.

Agassiz' students were like stars in the sky—almost as numerous and some of them nearly as bright. In the last century, especially from the sixties through the eighties, they studded the faculties of American schools and colleges the country over, adding luster to their master's name and keeping alive his methods of teaching.

One of Agassiz' students, the later illustrious William James, said of him:

> He was so commanding a presence, so curious and inquiring, so responsive and expansive, and so generous of himself and of his own, that everyone said of him: "Here is no musty savant, but a man, a great man, a man on the heroic scale, not to serve whom is avarice and sin!"

This indeed is a strong encomium, but we must believe it, for the record shows that every one of Agassiz' students felt the same way. Though his pedagogic method to us may seem a little hard—("There is probably no public school teacher now in New England," wrote James, "who will not tell you how Agassiz used to lock a student up in a room full of turtle shells, or lobster shells, or oyster shells, without a book or a word to help him out till he had discovered all the truths which the objects contained")—Agassiz won every student to him by his irresistible personality and his ultimate rectitude. One of them still survives—Edward A. Birge, president emeritus of the University of Wisconsin, in his ninety-seventh year.

Agassiz left his mark on many phases of education, and Dr. Teller elucidates them all —on the development of museums, on the summer-school idea, on the study of natural history, on the nature-study movement, on laboratory instruction, on the lecture method, on the university in general. Everyone interested either in the history of science in America or in the history of education will want a copy of this booklet. He will learn a great deal from it, and marvel that even yet the incredible spell of Louis Agassiz has not passed away.

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INTERPRETATION OF SCIENCE

The Scientists Speak. Warren Weaver, Ed. xiii + 369 pp. $3.75. Boni & Gaer. New York. 1947.

IF FOR no other reason, this book would merit reading on the basis of having so distinguished an editor and list of contributing authors—including eight Nobel prize winners, seven directors of prominent American research institutions, three university presidents, and many other leaders in science and education. And the content of the book lives up to what one might expect from men of this stamp.

The volume is a collection of the radio talks on science that formed the intermission program of the New York Philharmonic-Symphony broadcasts sponsored by the United States Rubber Company. The Preface to the volume, written by the Advisory Committee for this program, stresses the fact that the talks were built around men rather than around subjects. It was the intention of both sponsors and Committee that outstanding leaders in science should be asked to describe as intelligibly as possible their own fields of work and tell what they felt was significant for the layman. Without exception, they have succeeded in a most gratifying way. The talks, changed but little in their transition to the printed form, are of necessity brief—two to four printed pages each. It is a delight to see how great a vista of a given field of research can be presented in such small space; yet one never gets the impression of being hurried through a topic. Those who heard some of the talks will probably concur with this view.

The Committee has maintained a fine balance between the various scientific fields, and as a result the work is bound to interest everyone who desires an acquaintance with the problems of greatest present interest in science. It is remarkable, too, how almost every author unobtrusively weaves in the background needed to appreciate the problems of the hour. One of the most valuable features of the collection is the introductory chapter by Dr. Weaver—a skillful appraisal of the status of science and the tasks before it today. Equally helpful are his brief introductory pieces heading the main divisions of the book.

No undertaking of this magnitude can be completely free of blemishes, but very few were found, and none of these seemed serious. A chemical engineer and a physicist appear to contradict each other on the virtues of synthetic rubber (pp. 67, 85), and a medical researcher makes the statement that "the spectroscope operates on the principle that the color of an object depends upon its absorption or reflection of light."

The simple clarity of exposition in these articles bears out a conviction long held by this reviewer: that the best interpreters of science, today as in the past, are the men who lead the research itself—if only we could persuade them to do more of this type of writing. Our country seems to be one of the few where the bulk of interpretative science writing is left to journalists. Several of our science writers do a uniformly good job, but their number is pitifully small. In the European newspapers, for many decades, the practice has been to ask top-flight scientific workers to write expository articles for use in the *Feuilleton* or *Unter dem Strich* portion of the paper (which, by the way, was on page 1). There must be many men of the caliber of Eddington, Jeans, Gamow, and Hecht who are equally skilled in uncovering truth and talking about it simply and interestingly. Some of the contributors to the present volume may well be included in such a list.

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EARLY AMERICANS

The Indians of the Americas. John Collier. xi + 326 pp. Illus. $3.75. Norton. New York. 1947.

RARELY does one find the combination of careful scientific work and gifted writing that one meets in John Collier's book, *The Indians of the Americas.*

Beginning with the earliest evidences of man's culture in the New World (even pushing back into its Asiatic antecedents) and following through to the ultimate future of prediction, Collier traces the American Indian, his way of life, and his philosophy. He gives the reader a sense of continuity often lacking in the view of the Indian. He interrelates the various Indian cultures. Too often in the past these cultures have been treated as separates, with their historical continuity obscured.

The book is divided into four parts. Part One presents the Indians of prehistoric times; Part Two presents the Incas, Aztecs, Spanish conquest and rule, and the Indians of the Republics; Part Three presents the Indians of the United States; Part Four deals with the future.

The book is based on solid fact. Its author has had long experience as a research worker and a practical administrator. One recognizes this experience combined with a deep sincerity and intensity of purpose.

In this book the Indian emerges as a man. His achievements appear as a part of mankind. If no other end had been attained than this one of placing man in the New World in the perspective of man in the total world, the book would have been valuable. But it does more than that. It gives a clear, integrated picture of the Indians of the Americas. Time and space are used to clarify, not to bound or limit. The solution to modern world problems is the main goal.

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NEO-FREUDIAN

Conceptions of Modern Psychiatry. Harry Stack Sullivan. vii + 147 pp. $2.00. William Alanson White Psychiatric Foundation. Washington. 1947.

IN THE past few years, several psychoanalytic writers have offered major modifications in the theoretical structure of traditional Freudian psychoanalysis. In this series of lectures, Dr. Sullivan, a leading psychoanalyst, has done more than this. As a result of his experience in teaching, treatment, and research, he has evolved a set of principles concerning the nature and development of personality which, although they would probably never have been developed had psychoanalytic thought not been available, differ very radically from the tenets of traditional analysis. The importance of childhood events and the transfer of their effects to later life, as well as the notion of unconscious processes (here called "dissociation"), remain in Dr. Sullivan's formulations, and these points portray, of course, some of the debt he owes to Freud.

Some of the writing in the present volume is difficult and technical, and in general Dr. Sullivan's style does not make for easy reading. The relatively uninformed reader will find Mr. Mullahy's summary of the major theoretical points very useful as background for reading the lectures.

Sullivan's basic conception involves the self concept, which consists of the "reflected appraisals" of an individual communicated by others to him. It is largely through the approval and disapproval of an individual's actions by others, as he pursues the goals of satisfying biological cravings and attaining psychological security, that the self is formed. Likewise, it is through this reward-punishment mechanism (reminiscent of the law of effect) that the activities, interests, and directions of the self are limited: "selective inattention" and "dissociation" largely based on anxiety are the processes described here. The infantile and childish tendency to accept events as "given" because they are incapable of much rational reflection or logical thought may carry over in greater or lesser degree to later life and play an important role in one's personality and adjustment.

The familiar emphasis on sex, Oedipus complex, etc., which characterizes many psychoanalytic publications, is lacking here. When Dr. Sullivan refers to psychiatry as the study of interpersonal relations, he means that human behavior may best be understood in terms of the kinds of interpersonal situations the individual has experienced. What these experiences were will determine the individual's handling of future such relationships, his mental health and adjustment, his happiness, satisfactions, and security. This is indeed a stimulating conception and merits serious study by all who wish to acquire understanding of human motivation.

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STEPPINGSTONES TO ASIA

Bridge to Russia: Those Amazing Aleutians. Murray C. Morgan. 222 pp. $3.00. Dutton. New York. 1947.

IMPRESSED by the paucity of information on the Aleutians, where he spent a year in military service, Mr. Morgan was one GI who decided to do something about it. What he did was write this book, actually the first general, popular description of these remote and barren islands which he, unlike most of his companions, found interesting rather than dull or repelling. It is unfortunate that such a book was not available during the war. If it had been, or if the Army had even had the foresight to provide its men with such regional literature as did exist, morale would probably have been higher and the Aleutians might not have been such a dreary experience for many of the GIs.

The book is divided into five parts: The Land, The People, The Sea, The War, The Future. In the first three sections, roughly two-thirds of the book, the author presents a factual but consistently interesting and well-written account of the islands and their history, from the period of Russian discovery to the beginning of the war. A brisk, informal, and often humorous style masks the large amount of careful research that went into the preparation of these chapters. The chapter on the war provides a vivid description of military operations and of Army life in the Aleutians. A topically arranged bibliography and a short appendix on place names, natural resources, and settlement possibilities conclude a book that should be of interest to the general reader, the student, and the many service men once stationed in the Aleutians.

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THE PLANT KINGDOM

Our Flowering World. Rutherford Platt. vii + 278 pp. Illus. $6.00. Dodd, Mead. New York. 1947.

THERE is a great gap between Mrs. Lincoln's botany of a century ago and modern plant science, which is becoming ever more precise and more deeply rooted in chemistry and other exact sciences. The opening of this gap was a prerequisite to development of the science, but with its opening came a distinct loss. Lost was most of the intimate feeling the professional and lay naturalists had for the living world that surrounded them. This feeling was in part a direct response to nature's beauty, to the perfection of nature's products, a realization of the vastness of the natural stage, and, after Darwin, an awareness of the ages-continued evolution that has produced the world of plants and animals. Some of what has been lost may be found in *Our Flowering World.*

This book is concerned mainly with plants and time, time in terms of geologic ages and time measured in sequences of dormant buds and their opening flowers, and fruits. Its story is the tale of plant evolution controlled by, and reflecting, geological evolution. It begins with a brief description of modern plants. The reader is next taken back hundreds of millions of years and then returned to the present, stopping here and there along the way for a look at geological formations and their characteristic plants. Once more in the present, much is made of several seed-dispersal mechanisms and their physical operation.

Rutherford Platt is first and foremost an illustrator. His photographs are, as in his earlier works, superb. Several of his word pictures are hardly less so. His technique gives faithful representation of what is mostly northeastern vegetation. Further, it has all the selective lure of good portraiture. No one can read this book without a distinct feeling of aesthetic appreciation.

The author has worked into the book an important continuum of scientific data, theory, and hypothesis. There are some errors, a few misjudgments, and several omissions of fact. Were this book intended as a scientific presentation, they should be pointed out and their effects evaluated. It is perhaps better to accept Mr. Platt's Dresden-like panorama, with its romantic overglaze, as nearer to poetry and just enjoy it.

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MAN'S ENDLESS SEARCH

English Naturalists from Neckam to Ray: A Study of the Making of Modern Man. Charles E. Raven. x + 379 pp. $6.50. Cambridge, at the University Press. Macmillan. New York. 1947.

ALTHOUGH we often think of Aristotle as the father of biology, we do not as often remember that biology—or natural history—had many stepfathers. These are the men who brought the study of natural history out of the traditional belief in strange monsters and the improbable habits of imaginary beasts which was the "natural history" of the medieval mind, to the beginnings of modern natural history with its reliance on observation and verifiable facts. This book tells the story of natural history's return to nature in England. This story of the transition from medieval bestiary to science might be repeated for Europe as a whole, for England was not alone in this development. The author well understands this, and, as his subtitle indicates, his book is more than a chapter in the history of science, it is a study in the enlargement of man's mental horizon, a phase of his endless search for a key to the harmony of the universe, which both literal theology and narrowly applied science deny. The synthesis is yet to be achieved, but its beginnings are in the Religio Medici of Sir Thomas Browne. This, the author declares in his concluding sentence, may well be the starting point for the quest for the *religio medici* of our own age.

Together with Dr. Raven's previous book on John Ray, this is a notable contribution to the history of natural history, and it is to be hoped that he will continue to follow the course of natural history down to our own time.

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PISCICULTURE

Fish Ponds for the Farm. Frank C. Edminster. xii + 114 pp. Illus. $3.50. Scribners. 1947.

FISH culture in natural or artificial ponds is as old as history. Two thousand years ago the Chinese practiced fertilization of ponds to increase the growth of the vegetable organisms upon which fish indirectly depend. Their methods of stocking ponds with fish roe or young fish were quite efficient, as was also their plan of supplying additional food.

Centuries old, too, are the first published records of the practices followed in fish reproduction within the pond, the prevention of overpopulation by using predaceous fish to hold in check the multiplication of forage fish. In the early days of fish culture a much-used system consisted in taking as an annual crop all the fish in the pond by draining or seining and then restocking with eggs or young fish.

The author of the present volume reviews the evidence for a knowledge among the ancients of all the basic principles of fish culture, and proceeds to show how in modern times important refinements in the detail of methods have come into use, more especially in the United States since 1930.

Perhaps new methods of fertilization should head the list of these improvements. Chemical fertilizers (nitrogen, potash, and phosphoric acid) have replaced the use of organic materials. They are more sanitary and promote the growth of the minute plant organisms that constitute the natural food of animalcules and other small aquatic creatures upon which the forage fish (carp, breams, and the like) feed. This renders artificial feeding unnecessary. The author shows how this system furnishes a natural self-regulating device for the control of the fish population. There is a definite limit to the crop of fish to be expected in a pond of a given size. If it is too densely populated, the fish will be smaller and less desirable and will weigh no more in the aggregate than the smaller number of larger fish that will thrive in a limited population.

To this end the combination of largemouth bass and bluegill sunfish, or bream, in the ratio of one to ten has proved most successful. In that proportion the bass feed upon the bream and prevent them from multiplying unduly, and, of course, the bass is a desirable, well-flavored game fish.

Mr. Edminster devotes a large part of his book to practical directions for selecting a site for the pond, the many engineering features of its construction and maintenance, and the variety of uses the pond may serve. Properly located, these ponds may supply water to domestic animals on the farm, in the open or by piping to the barn. If situated near the house, the water might be pumped through hose in case of fire. The pond may be looked to for swimming in summer, skating in winter or as a source of ice in the absence of other means of refrigeration, or for duck hunting in the fall.

Finally, the pond may become one of the

beauty spots of the farm, for family gatherings, evening siestas, or picnics, not forgetting its fish population, from which under proper care about 150 pounds of good eating may be harvested yearly per acre of pond area.

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BEYOND THE RANGE OF SOUND

Hearing and Deafness. Hallowell Davis, Ed. xv + 496 pp. Illus. $5.00. Murray Hill. New York. 1947.

THE objective of this volume—to answer the thousand and one questions of the layman about how we hear, the nature of deafness, and what can be done for it, as well as to provide an authoritative guide to all those actively concerned with the problems of the deafened—seems to have been successfully achieved.

The need for such a book has been accentuated with the increased consciousness of the problem that has accompanied new techniques for the prevention of deafness, the development of electric hearing aids, new operations and further advances in the field of education and rehabilitation during the war years.

In particular, the articles in the lay press in recent years have awakened new interest by their portrayal of the dramatic side of the new surgical operation suitable for one type of deafness. They have created a need for broader information to be made available to the laity, which this book supplies in nontechnical terms.

The chapters dealing with the physics and psychology of hearing, the structure and physiology of the sense organ, the explanation of the cause for various types of deafness, and the medical and surgical aspects of prevention and treatment, including the place of the fenestration operation, provide general basic information that can supplement the more specific advice of the otologist. They supply the answers to a host of questions that must be settled before the full acceptance and adoption of any electrical aid or a long-term program of education.

The sections on the various tests of hearing, the mechanical details of hearing aids and their selection, use, and care provide information of both a general and a specific nature that has hitherto been available only to a limited few. These sections should be a valuable guide both to those concerned with giving advice and to the prospective buyer of a hearing aid. The facts should do much to prepare him to get the full value out of his instrument.

One chapter is devoted to the organization of a rehabilitation center during the war; included in the discussion are medical and surgical care, auditory testing, fitting of hearing aids, auditory training, speech (lip) reading, and conservation of speech. In this, as in all chapters dealing with education and rehabilitation, the psychological aspect of the problem is given major consideration.

The history of progress in education of the deafened and the development of a more enlightened social viewpoint are reviewed. The training of the deaf child in the home and at school is discussed with a view to creating an understanding of the psychologic problems and providing a guide to various steps in education.

The various organizations for the aurally handicapped and for their vocational guidance are outlined.

This volume provides a reliable and much-needed source of information for the deafened, as well as a guide to all those concerned with the alleviation of the disability.

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COMMENTS AND CRITICISMS

SAN JOAQUIN VALLEY HOG WALLOWS

About twenty-five years ago a member of the staff of the Citrus Experiment Station, Riverside, California, who was investigating soil problems in Tulare County, wrote an article for the *California Cultivator* on the origin of hog wallows. In this he stated that the finest specimens of hog wallows in the state were in the Lindcove district, close to the foothills and about 13 miles east of Visalia. His idea was that the mounds were formed by clumps of vegetation holding wind-blown soil.

Because I was then, as now, living upon and working hog-wallow land and also writing occasionally for the *Cultivator,* I was much interested in this. A plant pathologist of the county, with whom I discussed the matter, did not agree with the above theory and thought that the mounds were formed by clumps of water plants catching and holding sediment washed down from the hills.

After this second theory was published in the *Cultivator,* a flood of letters and suggestions on the origin of the mounds came in, including all that were given in the article on Mima Mounds in THE SCIENTIFIC MONTHLY (October 1947) except that of their building by pocket gophers. If the mounds in the San Joaquin Valley were built by any mammals, it was evidently by ground squirrels, but even that seems more than improbable.

Neither the gophers nor the squirrels needed to construct mounds in hog-wallow land in order to find a place to live. Although there is much hardpan in hog-wallow land, it varies in depth, thickness, and quality, and there is always enough space above the hardpan for the animals to burrow in, so that the construction of mounds was unnecessary. Also, in former days, before systematic poisoning of ground squirrels was done, they gave no sign of building large mounds like these on flat or gently rolling land. Mounds where they do work lose their smoothness, instead of remaining round and smooth. The influence of pocket gophers on the shape of the mounds seems insignificant.

In fact, all theories for the origin of these mounds seem to have more faults than merits. None give satisfaction to more than a few individuals. The most satisfactory way around here to explain them seems to be that they are composed of material left over when Paul Bunyan built the Sierras long ago. That theory is more pleasing than accurate.

Nevertheless, there is a good deal of data by which we can approach the problem of the origin of these mounds. The local term, "hog wallows," is of course very misleading. In thinking of wallows one visualizes circular depressions in flat or gently rolling country, such as might be formed by animals wallowing in mudholes and causing them to increase in area and depth. Hog-wallow land is the reverse of this: it is composed of mounds, not depressions, sprinkled over fairly flat or gently rolling areas.

These hog-wallow lands have certain marked characteristics that should be helpful in studying the manner of their formation. The material composing them is reddish, indicating a fairly high iron content. This is true not only of the San Joaquin Valley hog wallows, but of hog-wallow land in other parts of California.

Next, and this is a very significant feature of the case, the soil neither expands on getting wet nor shrinks and cracks in drying, but retains its size and shape at all times. When dry in its wild state, it cements together like a brick. Incidentally, this causes the growth of vegetation on the mounds to be scanty, though grass may be abundant in the flat areas around the mounds. Raw hog-wallow land during the dry, hot summers thus appears barren and infertile. Actually, it is rich in mineral plant food and makes some of the finest fruit and grape country in the state when it is leveled, subsoiled, and irrigated. After a few years of irrigation and tillage, it often tends to become dark rather than red, and a stranger would not dream that it had once been covered with hog wallows.

The fact that the soil in these mounds does not shrink and crack on drying is one of the reasons that the mounds retain their shape. If they cracked on drying, the slopes would evidently gradually flatten and the mounds would disappear. Also, the fact that the material sticks together when wet and cements together when dry indicates that it was not brought into place by wind.

Incidentally, in much of this hog-wallow land, there are depressions here and there where ponds of considerable size form in wet seasons and endure, perhaps, for several months; the soil on their beds is a kind of adobe that cracks as it dries.

These depressions and the fact that hog-wallow land is often quite rolling near the hills suggest that the hog wallows did not form on a flattened lake bed, for then there would be no appreciable depressions in the land and it would not be rolling.

It is also worth noting that usually the more rolling the land, the larger the mounds. As the surface tends to flatten, the mounds tend to become smaller and lower. The fact that many hog wallows are on rolling land therefore suggests that it was not made by normal sedimentation, but that the material forming it was laid down in a hurry. Incidentally, too, water could never stand between mounds on rolling land as it is seen to stand between the mounds in San Diego County (p. 291, October SM). (The lower picture on page 287 shows how hog wallows often exist on rolling land where water drains off quickly.)

Another significant feature is the composition of the material of hog-wallow lands. In typical hog-wallow land here in Tulare County and the San Joaquin Valley generally, the soil proper is composed of very fine material in which there are considerable sand, fine gravel, and even pebbles up to a good throwing size.

These pebbles seem to have had a varied origin in both composition and in movement from their original position. Some are well rounded by water, and some are uneven—friction has done little more than smooth the edges and corners.

Although the higher hills in this locality are mainly granite, many of the hills at the edge of the valley are of gabbro and related femic material, and the hog-wallow land seems to have been derived more from them than from the granite. The iron in it indicates this, also the high calcium and magnesium content. Many of the rough pebbles in the hog-wallow land seem to be made of magnesite, which has been mined in places in these hills. The gravel and pebbles occur both in the mounds and the land underlying the mounds. In the nature of the case, if the mounds had been made by wind-borne material, the pebbles would not be there.

The rounded and partly rounded pebbles, the sand, and fine gravel indicate deposit by water in some way or another; but the fact that these are well mixed with fine clay indicates that the material was brought into place as a whole, evidently at about the time that the land surfaces in these places took form. Patches of hog wallow on rolling land (p. 291, October SM) surely could hardly have been laid down gradually, on lake bottom.

The failure of every other theory for their origin, with the above-mentioned conditions, has therefore suggested to me the possibility that the mounds were formed by the uneven settling of considerable thicknesses of soft, unconsolidated material that was laid down hurriedly, but this provides no explanation of why mounds should form, or why they should take such well-rounded shapes if they did form in such settling. To the extent that this hypothesis has merit, it does little more than provide another unsatisfactory hypothesis for their origin.

As to their being built by pocket gophers, as has been suggested for the Mima Mounds of the Puget Sound country, this seems out of the question, for the hog-wallow mounds at least. Gophers live in such land, but give no sign of helping to make the mounds.

Mounds seem to exist only on certain kinds of land laid down under unusual conditions. From the pictures of the Mima Mounds in Washington, it seems unbelievable that gophers ever built mounds containing such large stones, or that they could do so if they tried. It seems probable that the origin of the mounds will always remain a mystery, though the cobbles of the Mima Mounds of Washington and of the hog-wallow lands of California indicate that rapidly moving water had much to do with the origin of both. The mounds of California retain their shape because the soil does not crack when dry, but cements firmly together. The soil of the Washington mounds may or may not cement together, but the size and number of the stones in them would nevertheless help to retain their shape.

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Exeter, California

TO AJAX

Dr. Carlson's article, "Science, Education, and the Future of Man" (SM, December 1947), dealing with a subject and a philosophy so important to all human beings, is couched in language that should be understandable by anyone who has progressed through the common schools of this American democracy. How can such a subject, presented in such convincing terms, fail to be of extreme interest to everyone? The great tragedy of it all is that the material presented by The Scientific Monthly trickles through only a very small percentage of our population. I find much in the journal of recent months that could well be made part of the general education of everyone if there were only some way of reproducing it a hundred thousandfold. If the A.A.A.S. could sponsor the distribution of such material directly to those owners and editors of American newspapers who are, or could be persuaded to be, sympathetic to popularizing the work of scientists, I feel a great mission would be launched.

Such scholars as Carlson, and correspondent Laurens C. Bolles (SM, December 1947, p. 526), who discusses "Back into Balance with Nature," are doing their part in furnishing information regarding the philosophy of scientists, a philosophy which shows that science can be really humane. But the distraught worker cannot, singlehanded, be his own Thomas Huxley to a whole world. Why should not the Rockefeller Foundation, or a similar organization, be interested in advancing civilization in some such manner as well as by the techniques it already employs?

C. Allan Lyford

East Aurora, New York

MORE ON MAN, THE MYTH-MAKER

C. A. Muses in his criticism (SM, December 1947) of Read Bain's article on "Man, the Myth-Maker" (SM, July 1947) gives a rather novel definition of democracy as the "brotherhood of man itself." Apparently, this definition arises from the study of theology and not of political sciences, and its vagueness helps strengthen R. Bain's contentions.

The majority of the terms used in the liberal arts lack strict definitions. For instance, where are the dividing lines between aristocracy, democracy, and ochlocracy? How many persons, including some well-known historians, are able to differentiate between culture, civilization, and manners? Where is the border line between despotism and freedom or freedom and anarchy?

All types of governments are backed by elaborately worked out ideologies, but ideals should not be confounded with grim realities. Every nation has its own beliefs which, moreover, undergo continuous changes, and a person, or even a nation, is often persecuted because it is out of step with the stronger opponents. Unfortunately, the present age of narrow specialization witnesses the mass production of experts at the expense of persons with encyclopedic education. This tends to increase the intolerance, as witnessed by the brutalities committed by the civilized but uncultured barbarians of the present age, who are decimating their opponents, in the name of freedom, humanity, and justice, at a much greater rate than has ever been done before.

C. A. Muses' uncompromising statement "that the overwhelming majority of scientists worthy of the name do not share the polemic and uncreditable aspects of the views to be found in Bain's article," may be regarded as an attempt to kill the development of free thought among scientists without resorting to any convincing argument. Imposition of such types of unasked censorship merely shows that quite a few persons are trying to force their beliefs on others by methods that can hardly be recognized as scientific.

V. A. Kalichevsky

Beaumont, Texas

VITAL STATISTICS

May I direct your attention to an error, not too important, of course, but definite, that appeared in the December 1947 issue of The Scientific Monthly?

On page 447 the statement is made that the "Chicago Medical Society . . . is the largest county medical society in the country, with a membership of approximately 6,300. . . ." I should like to state that the Medical Society of the County of New York has a membership of over 6,500.

We have every honor and respect for the Medical Society of Cook County, but it is good to have the record straight.

I want to take this opportunity of thanking you for the enjoyment and education that I have had from reading The Scientific Monthly. I think that that magazine and *Science* are the most outstandingly enjoyable things that I get to read.

Harold B. Davidson

Medical Society of the County
of New York

VIVIPARITY

The paper by C. L. Turner, "Viviparity in Teleost Fishes," in the December issue of The Scientific Monthly was very interesting. However, I feel that I should register my objection to the definition Dr. Turner assigned to the word "viviparity." He used it "for all cases in which eggs are fertilized before leaving the body and are retained even for a short time."

According to this definition, a chicken is viviparous.

If a special name is felt necessary for the described condition in fish, it would be better to manufacture a new word rather than to give a new and contradictory meaning to an existing one whose definition is widely accepted.

Humpty Dumpty said to Alice, "When I use a word, it means just what *I* choose it to mean—neither more nor less," but for everyone to follow such a practice would lead only to confusion.

Norman D. Levine

College of Veterinary Medicine
University of Illinois
Urbana

RE RHINE

I am discouraged! After over fifty years of experimental research, it appears from your reviewer's article on Dr. Rhine's book (SM, February 1948) that I must be ignorant of the first principles of scientific research; and perhaps my own work on solar radiation may be worthless.

I submit there never was a study of phenomena so elaborately hedged about with checks devised to shut out errors as that which Dr. Rhine describes.

When I first heard of the results, about twelve years ago, I firmly disbelieved them possible. To confirm my negative view I procured a set of the twenty-five ESP cards with record sheets. I determined to test clairvoyance by myself alone, so that I could not be deceived. After about two months' work, to my surprise, the chance was about 100,000 against one that I could have accidentally attained so great a success in my calls.

My experiments ran true to the forms that since have been many times reported. Success was greatest when the mind was fresh and happy. The record sheets showed greatest success in the upper left-hand quarter, and least in the lower right-hand quarter. At times of headache, tired condition, or depression, with neutral success for calls, there was strong positive result as between the call and the adjacent card. This displacement effect is now well known from results reported by the English experimenter Dr. Soal.

About ten years after the experiment on dice throwing at Duke University, the record sheets were received to tabulate the results separately for the four quarters of the record sheets (*J. for Parapsych.* 8, 1, pp. 18–60, March 1944.) Such a test was not contemplated when the successful experiments were performed and their results computed. It was found *universally true* that positive results had been most accentuated for the throws recorded on the upper left quarter; next, and nearly equal, but less, for the upper right and lower left quarters; and least of all for the lower right quarter of the record sheets. This test eliminates all defects of the dice and all predisposition on the part of the experimenter.

The experimental basis referred to in Dr. Rhine's book now rests on the work of many years by hundreds of observers in scores of institutions, in many lands. I do not think this work can be brushed aside as it is done by your reviewer.

Charles G. Abbot

Smithsonian Institution
Washington, D. C.

TECHNOLOGICAL NOTES

Largest technical college. Purdue University, like other institutions of learning, has had a great postwar growth. The enrollment of 14,000 is about double the former figure. The College of Engineering, with 8,000 students, is the largest technological school in the land.

Trying to go along as usual when conditions are unusual puts a good deal of strain on an institution and the personalities that make it up. Nevertheless, the methods developed by Dean A. A. Potter and Associate Dean W. A. Knapp to personalize education at Purdue appear to have enough flexibility to make the extraordinary expansion.

As first teacher in the college, Dean Potter keeps up on his pedagogics by personally teaching a senior class in power-plant design. Also, besides the administrative work with faculty members and university officials, he finds time to meet every senior personally several times during the year, and every student at least once a year.

Another course Dean Potter teaches seems to be unique in engineering education. It is a course for young instructors in the college. He believes that they should have some actual instruction in engineering education. The course is open to ten faculty youngsters who are working for advanced degrees. It covers the history of engineering education, methods, and curriculums in the United States and foreign countries, and the connection between engineering education and industry. The students do research on assigned topics, meeting in the dean's office to present their findings orally, and standing ready to defend their positions in discussion.

Teaching engineering instructors how to teach seems like such a good idea that you wonder why similar courses aren't given everywhere. Dean Knapp reports that the students like the course; it gives them a chance to express their views without being abashed by their elders, as would probably be the case in faculty meetings. The dean enjoys the course, too, although keeping ahead of such eager minds imposes a strain, particularly since it is not always easy to answer questions about traditional educational practices.

Subtle advertising. Releases from advertising agencies, no matter how technical the subject, usually make a pretty definite point of the advantages of the thing advertised. One recently received is different, to say the least. It is a sort of article on pipe, from ancient lead tubes—there's a reminder that the Latin word *plumbum*, "lead," gives us the word "plumbing"—to the modern underground transportation systems for water and fuels.

There is mention of Syrian baked clay pipe and of the copper tubes used by the Egyptians in 3800 B.C. Then come Greek and Roman aqueducts, and the hollow logs used up to one hundred years ago for water mains in England and America. (But there's no hint of the fact that holes chopped in the wooden mains and then plugged gave us "fireplug" to designate the hydrant for fire fighting.) Pipe fittings of malleable iron were first made in 1854 by Stanley G. Flagg, of Philadelphia—this mention of fittings and their importance in the business is about the only suggestion that the real purpose of the piece is advertising.

Dehydrofreezing. Dried fruits and vegetables are not as popular as frozen foods. A combination of drying and freezing may have advantages over either method of preservation, suggests the Western Regional Laboratory of the U.S. Department of Agriculture. First, a part of the moisture is removed, saving weight and bulk. The remaining water is frozen, breaking up the tissues less than if the full amount were present. The combination is called by the monstrous word "dehydrofreezing."

Engineering and geography. What engineers did in 1947 to improve transportation in various parts of the world is the subject of a National Geographic Society release. The report covers projects on the seas and in the air, and on and in the earth. Riga, the Latvian city on the Baltic, is being rebuilt as a year-round port for Russian commerce. The Corinth ship canal in Greece and the Albert Canal in Belgium were opened for business. Despite civil war, China is at work on an 1,800-mile railroad line, and the Russians are building 2,500 miles of trunk lines across southern Siberia. Big airports are under construction; yet some of the planes are too big for them. Arabian desert pipe lines are as big as the "big-inchers" in the United States; the pipe is 30 inches in diameter to carry 300,000–400,000 barrels of oil a day.

Viking "Wineland." The Smithsonian Institution reports that study of the Viking sagas and comparison of the descriptions they contain with the shape of the earth as determined by modern methods indicates that the "Wineland" of the Norse adventurers was probably the territory near the mouth of the Merrimack River north of Boston.

Lightning center. What the General Electric Company describes as the world's largest laboratory for study of the behavior of lightning is scheduled for completion this spring at Pittsfield, Massachusetts. Two 5-million-volt impulse generators will supply discharges of 10 million volts—the pressure of natural strokes on transmission lines. These generators and other equipment to produce artificial lightning are expected to yield much information on the effect of Jove's bolts on electrical systems and how to achieve protection against them.

Cloud resources. When the General Electric team of Irving Langmuir and Vincent Schaefer announced the possibilities of triggering snow from cold clouds by dropping dry ice from airplanes, there was general interest and acclaim. Lowering clouds might be opened up near an airport. Clouds moving toward an urban center might be induced to drop their snow before they got to the congested district. Clouds that were about to escape the mountain ranges might be caught and their precious contents saved for irrigation. Rains might be induced—and some rains do, indeed, appear to have been touched off in Australia, our Western states, and elsewhere.

An interesting sequence to this scientific development is its legal complications. In the West a water claim has been filed on the clouds overhead. There is possible liability for causing unwanted rain or snow to fall, and also danger of accusations that land has been robbed of precipitation it would have received if the clouds had not been tampered with.

Threats of the law may prevent private experimenters from undertaking some weather projects. One of G.E.'s vice-presidents suggests that the development has great possibilities—as great, perhaps, as nuclear power—and it deserves study on an adequate scale, free from danger of legal entanglements. Mr. Schaefer has described the possibility of weather control as development of a natural resource.

THE BROWNSTONE TOWER

AT THE recent Chicago meeting 4,940 people registered with the A.A.A.S., the largest number yet recorded. The total attendance is unknown because many others registered only with affiliated societies. The meeting was probably bigger than any previously held by the A.A.A.S., and, though it is gratifying to those concerned that it proceeded as smoothly as it did, it is alarming to contemplate still larger meetings. The possibility of holding satisfying scientific meetings declines when the numbers of those attending exceed the optimum. Meetings like that in Chicago have probably passed the optimum and, indeed, can be held in only a few cities of this country. Many large hotels are required, and if the visitor wishes to meet individuals in different specialties and to attend meetings of different groups, he must spend much of his time walking or riding from one hotel to another and making his way through crowds. At the end he emerges somewhat exhausted and frustrated, having failed to see people he wanted to meet and to hear addresses that would have interested him.

When scientific meetings become numerically overwhelming, some action must be taken to reduce the attendance. The American Chemical Society, for example, will try a marathon meeting next summer. There will be an Eastern Session in Washington, D. C., August 30–September 3; a Midwest Session in St. Louis, Missouri, September 6–10; and a Western Session in Portland, Oregon, September 13–17. Thus, no one city will be overloaded with chemists, but those who are able and willing to attend all three sessions can do so.

The meetings of the A.A.A.S. have so far been held to practicable limits of attendance by the voluntary decision of more and more affiliated societies to meet separately. In my opinion, this trend is desirable both for the affiliated societies and for the A.A.A.S. When a scientist goes to a meeting of his professional society, he should devote his entire attention to it without being distracted by other events heretofore characteristic of A.A.A.S. meetings. On the other hand, he also needs the broader view and contacts to be obtained at A.A.A.S. meetings. The answer to the needs of the broad-minded specialist is obvious. Let him concentrate on his professional meeting at one time and expand his general knowledge at an A.A.A.S. meeting at another time. It remains for the A.A.A.S. to provide attractive programs for the inquiring mind. The conventional type of A.A.A.S. meeting, overweighted and unbalanced by concurrent professional biological meetings, does not provide the proper milieu for considering science as a whole. The A.A.A.S. should hold meetings of its own where current scientific thought and achievements may be synthesized, where broad scientific problems cutting across the recognized branches of science may be considered and brought to a focus. This, surely, should be a function of the American Association for the Advancement of Science. Perhaps the new Institute of Biological Science will replace the A.A.A.S. as the agency for arranging meetings of the biological societies that wish to meet together, leaving the A.A.A.S. free to develop unique programs of its own.

The Policy Committee that is planning the Centennial Celebration of the A.A.A.S. has boldly taken the step advocated above. The Committee has decided that neither sections nor affiliated societies will meet with the A.A.A.S. in Washington, D. C., September 13–17. So, for this meeting at least, the A.A.A.S. will stand alone while celebrating its Centenary. True, some of the biological societies will meet in Washington just prior to the A.A.A.S. meeting, but that is an advantage to those who may be able to stay over for the Centennial.

Previous meetings of the A.A.A.S. have included various symposia, but the Centennial meeting will be the first to consist exclusively of carefully chosen technical symposia and semipopular lectures. All participants will be as carefully selected as the subjects and will serve by invitation only. At this writing the program is tentative, but it is expected that the planned meetings will be held only on three mornings and four evenings, the afternoons being left open for visiting places of scientific interest in the Washington area and for making personal contacts. At last the pressure of an A.A.A.S. meeting will be relieved, and visitors will have time actually to visit, to think, and even to rest.

The first and largest meeting will probably be held in Constitution Hall on the evening of September 13. Here, let us hope, the President of the United States will give an address of welcome. The address of the retiring president of the A.A.A.S., Harlow Shapley, will follow, and the evening will be concluded by a reception.

On each morning thereafter five symposia will probably be held concurrently in as many different auditoriums. Here the visitor will get scientific stimulation, for each technical symposium will cut across three or more recognized branches of science. For example, on a given morning the visitor, in accordance with his interests, may attend one of the following symposia: World Natural Resources, World Health Problems, Nucleonics, High Polymers, or the Upper Atmosphere. He will hear three speakers, each active and prominent in research on some aspects of a general subject and able to present and evaluate current facts and theories—able, in other words, to erect a significant structure out of the building stones of research. The speaker, not being limited to ten minutes, but allowed almost half an hour, should be able to present effectively the results of his mature thinking on his subject. Other equally able men will be invited to discuss each paper, thereby emphasizing or throwing a different light on the conclusions of the speaker. The visitor himself may participate by submitting written questions to the speaker, who may take them up at the end of the session. Thus, quality will be substituted for quantity, and sound knowledge and understanding for the overwhelming details of previous meetings.

If the visitor should be fagged by the uncompromising scientific atmosphere of the morning symposia, he may relax at the evening lectures, where competent scientists will give interesting talks on subjects that would be suitable for the SM; for example, on bird migration, weather control, or mechanical brains. On each of these evenings he may choose among four lectures. At the end of the meetings he will go home scientifically and culturally refreshed.

The Centennial Celebration, we feel sure, will be well attended by out-of-town visitors. The importance of the occasion, the novelty of the program, and the location of the meeting in the national capital will combine to attract all the visitors that our hotels can hold. Furthermore, the meeting comes at the right time for most university scientists, just before the opening of the fall quarter or first semester, and one can drive to the meeting without facing the hazards of snow and ice so prevalent in the North during many of our Christmas meetings. What an opportunity for a pleasant and profitable vacation! So many people, I think, will be of like mind about it, that it would be well to make plans and reservations as early as possible.

We, of course, have made our plans for the Centennial (September) Issue of the SM. Instead of presenting a survey of science in Washington as we did for Chicago, we shall devote the Centennial Issue to contributions from distinguished scientists who have won the A.A.A.S. $1,000 Award. Thus, we shall have a notable scientific issue that will not only reflect one important aspect of the Association's advancement of science, but will be a tribute to the anonymous donor of the Award.

We are enthusiastic about all prospects here described and look forward to the Centennial Celebration with keen anticipation.

F. L. Campbell

THE
SCIENTIFIC MONTHLY

MAY 1948

THE NOBEL PRIZES*

HERMAN ERIKSSON

A native of Upsala, Sweden, Mr. Eriksson received his B.A. in government and administration at the University of Stockholm in 1919. In 1932, he was made Permanent Undersecretary in the Swedish Ministry of Finance and remained in that capacity until 1936, when he became President of the Board of Trade. He was Minister without Portfolio in 1938; Minister of Supply, 1939–41; and Minister of Commerce, 1941–44. He became Swedish Minister to the United States in 1945 and Ambassador in 1947.

TO SWEDEN the provisions of the last will and testament of Alfred Bernhard Nobel—the native-born scientist, inventor, and industrialist—came as a complete surprise. Never before had such a fortune been amassed by an individual Swede, nor had one ever been devoted to such idealistic and wholly international purposes. That Sweden had been chosen to administer the fund, which had been built up almost entirely outside the country, was naturally considered an exceptional honor, especially as it came from a citizen who had spent almost his whole life abroad. Obviously, the honor also involved many obligations and heavy responsibilities. Each year the award of the Nobel prizes would center the attention of the world on Stockholm.

On December 10, 1896, at the age of sixty-three, the frail-bodied but mentally alert inventor of dynamite and other high explosives died in his winter home at San Remo in northern Italy, and on December 31 of the same year the contents of his will were announced. It had been signed in Paris on November 27, 1895, and had been written out in Nobel's own hand. In form it was so unprofessional that manifestly no lawyer had been engaged in the composition, and yet its purpose was perfectly clear. The witnesses were employees of the testator in France, and the two executors named were Swedish engineers also employed by him. The younger of these, Ragnar Sohlman, then but twenty-six years old, is still living today. To him and a colleague was entrusted the settlement of perhaps the largest private estate in Europe up to that time. After debts and personal legacies had been paid, it amounted to about 33,125,000 kronor, or roughly $9,000,000 at present rate of exchange. As the property was located in many different countries and the money had been invested in various kinds of securities, it took some time to settle the estate. The will was contested by some of the Nobel relatives in Sweden, but a compromise was soon reached. The final sum available for investment in "safe securities," as directed by the will, was about 31,125,000 kronor, or nearly $8,000,000.

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* All photographs by courtesy of the American-Swedish News Exchange, Inc., New York, N. Y.

As only the income from this residuary fund was to be distributed each year in the form of five prizes, the amount of the main fund has been kept fairly constant. In 1936, for example, it was 32,600,000 kronor; the special funds set up for testing laboratories, libraries, etc., brought the capital sum up to 48,860,000 kronor. The cost of special buildings was about 2,330,000 kronor, which raised the total assets to nearly 50,000,000 kronor, or roughly $14,000,000.

Since the prize amounts depend on the income received from the securities in which the main fund is invested, the monetary value of the prizes varies from year to year, usually being between $30,000 and $40,000. Since the depression of the early 1930s the figure has usually been around the lower amount. In 1947, however, the income from the fund was declared for the first time to be tax-exempt, and consequently the value of the prizes for that year became nearly $40,000 each. In most countries it is an accepted fact that the civic and professional distinction of a Nobel prize is worth more than the money involved, though the two are necessarily incommensurable. No other prizes in the world have brought such fame. Throughout his life, whenever a winner appears in public or has his name printed in a newspaper or program anywhere in the world, the title "Nobel Prize Winner" is always attached.

WHO, then, was Alfred Bernhard Nobel? How did he make his fortune, and why did he devote it to such idealistic purposes?

In ancestry he was wholly Swedish. Surmises that, because of his name, he must have been of at least partly English descent are unfounded. The origin of the name is Latin and not English. His earliest identifiable ancestor was a farm youth from the province of Skåne in southern Sweden, who, because of his intellectual gifts, was provided with an academic education. The name of this ancestor was originally "Peter Olofsson," but because such traditional patronymics caused confusion outside Sweden, the youth was renamed "Petrus Olai Nobelius" when, in 1655 or thereabouts, he entered the University of Upsala, then the only one in the country. The word "Nobelius" had been derived from the name of the young man's birthplace, the parish of East Nöbellöf. This procedure was customary and corresponds to the latinization of personal names on modern American college or university diplomas. A grandson of the first Nobelius, Immanuel, changed the name to "Nobel," with accent on the last syllable. For a while he had to serve in the Swedish army as a noncommissioned officer, where only surnames of not more than two syllables were permitted for men below officer's rank. Ultimately, Immanuel Nobel became a barber-surgeon. (His father had been an instructor in drawing.) Normally, the first Nobelius, being a farm youth, would have studied theology and become a clergyman. Instead he chose the law and at an early age became a judge. He also had a gift for music. In his career he was aided by the fact that he had married the daughter of Olof Rudbeck, perhaps the most learned and most influential professor at the University. All contemporary knowledge seemed to be his field, and the Swedish biographers of Alfred Nobel like to believe that some of the latter's brilliance may have been inherited from this famous ancestor.

His own father, also called Immanuel Nobel, and born in 1801, was the son of the barber-surgeon, who in his early years had gone to sea. There was a definite spirit of adventure in the family, and Immanuel Nobel had inherited his share of this spirit. He was, besides, a man of imagination, an inventor of sorts, an engineer, and a builder in Stockholm, who overextended himself and suffered bankruptcy while his children were still young. From him Alfred Nobel inherited the ferment of genius, the imagination and restless spirit of a dreamer and inventor; from his mother he derived the common sense, the stability, and the practical turn of mind that is a more usual Swedish characteristic. Her three oldest sons became famous as engineers, promoters, and industrialists. While Alfred Nobel became a pioneer in the manufacture of high explosives, from which he derived his fortune, his brothers built up the oil refineries of southern Russia, and until the revolution of 1917 they were rated among the wealthiest men in Europe. In the oil business Alfred Nobel participated only as an investor and financial adviser.

When Immanuel Nobel became bankrupt in 1842, he first moved to Finland to repair his fortunes, only to fail again. He next went to Russia, where he found greater opportunities to apply his engineering ideas. It is interesting to speculate upon what would have happened had he come instead to the United States, whither his countrymen were then beginning to emigrate. Among the first ideas he launched in Russia was that of submarine mines for coastal defense, and when the Crimean War broke out he received large Russian government orders. He was thus able to build a foundry and a machine shop of fairly large size. In his war work he became associated with N. J. Putiloff, one of the leading Russian industrialists of the day. Nobel had the ideas and the "know-how," and the Russian had the capital and the contacts. Out of this association grew the famous Putiloff munitions works near St. Petersburg, or Leningrad, as the former capital is now called. Having become acquainted through his war work and inventions with the Russian Emperor, Nicholas I, himself and believing he had received his promise and that of his ministers that in the future, too, the Russian government would continue to buy his products, Immanuel Nobel built more and more factories—on borrowed capital. When the war was over and the emperor had died, he discovered that he had been mistaken. The new emperor and his advisers, like new Pharaohs, did not know Joseph or what he had done for Egypt. Once more Immanuel Nobel became a bankrupt and, brokenhearted, he returned with his wife to Stockholm, leaving Alfred and his two older brothers in Russia. Among the new ideas he then worked on was that of combining thin slices of wood, or even shavings, placed crosswise, so as to make firm boards. Out of this conception, modern plywood was ultimately evolved, but he derived no benefit from his scheme.

ALFRED NOBEL, 1833–96

THE DONOR OF THE NOBEL PRIZES.

Soon after settling in Russia early in the 1840s, Immanuel brought over his family and thus interrupted the schooling of his sons. At the age of eight, Alfred had attended with his brothers a private school in Stockholm (there were then hardly any public ones), but was able to remain for only two terms before being taken to St. Petersburg. This was to be the only formal schooling he ever received. In the Russian capital there were not even private schools that suited the recently arrived young Swedes, so a private tutor was imported for them from Stockholm. In time he was succeeded by a Russian. Both must have done their work well, for all the sons of Immanuel Nobel became in time highly educated men. Alfred Nobel not only was an eminent scientist and inventor; he read and spoke and wrote with almost equal ease Swedish and Russian, German, French, English, and Italian. In all these languages he carried on a regular correspondence, writing, on the average, about thirty letters a day, all in his own hand. Many of them are still preserved in the Nobel Institute in Stockholm. He never had a stenographer or personal secretary. Otherwise, his policy was to engage experts to do everything that he did not have to do himself, especially if they could do it better.

As early as 1850, when he was only seventeen years old, Nobel was sent alone by his father on a foreign tour. He covered not only Germany, France, and England, but also the United States, a country few Europeans then considered worth visiting. Unfortunately, he kept no diary or any systematic record of his travels. From a letter to a relative, it has been learned that in New York he met Captain John Ericsson, of Civil War *Monitor* fame. Ericsson had arrived from London in 1839, and, thanks to his various inventions, he was by 1850 the most famous Swede in America—except, of course, Jenny Lind. Alfred Nobel does not seem to have known Jenny Lind. Music was not among his hobbies; for composers and singers he established no awards.

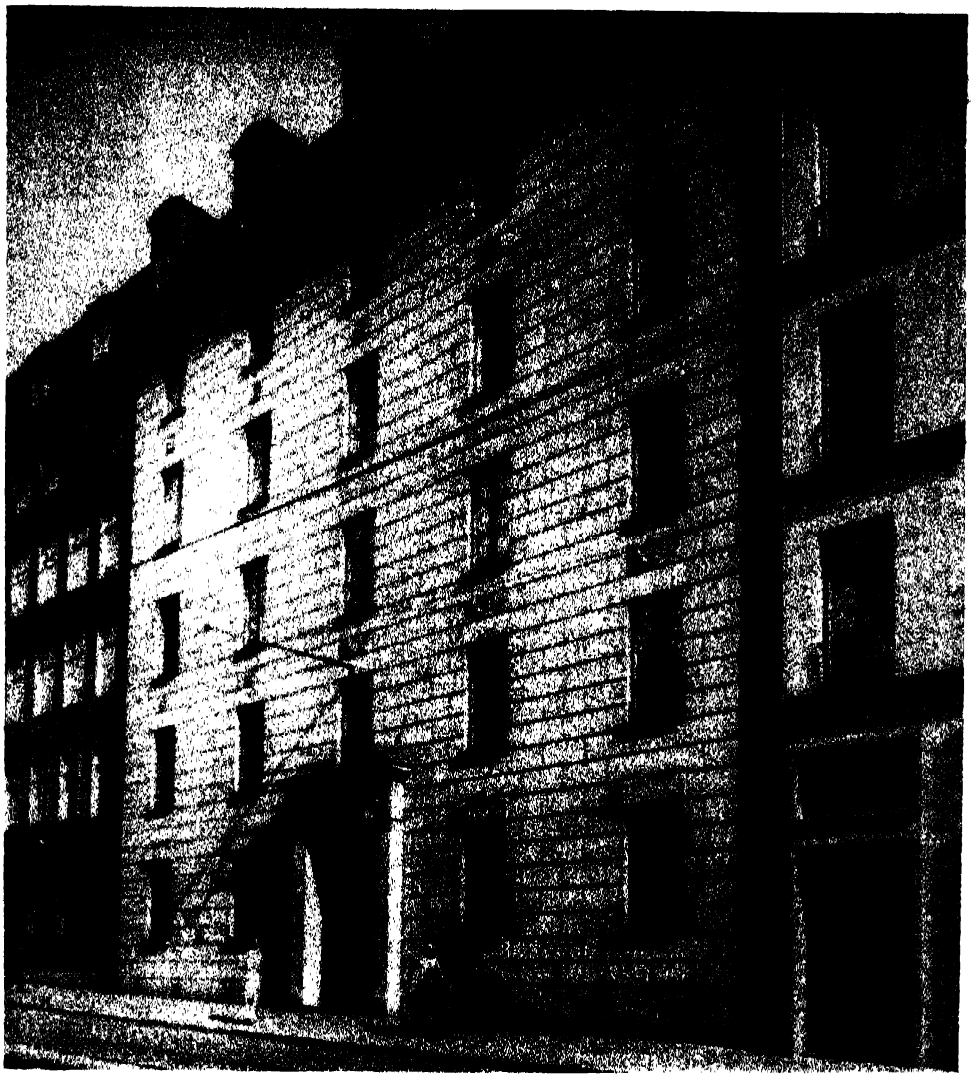
IN THE course of his early studies, Nobel had become interested in explosives, particularly the newly discovered nitroglycerin, or "blasting oil," which an Italian professor, Ascanio Sobrero, had discovered in 1847 in his laboratories at the University of Turin. What to do with it, no one knew. At high temperatures it caused violent explosions, but no one had any idea how to ignite it at the right moment, even when it was cold. It could not be safely transported, and all over the world it began to cause catastrophes, sinking ships and killing men. To ignite the violent liquid at any temperature, Nobel first devised the mercury percussion cap. He had found that not only a spark, but also a shock, was required. Once, in New York, a can of nitroglycerin had been left by a careless salesman in a hotel lobby. When it exploded, some weeks later, not only the hotel but most of the block went up. The whole of lower Manhattan was shaken. From Panama, San Francisco, and many other scattered points came reports of similar disasters. Nitroglycerin was sometimes used by the anarchists of the day in their bombs, and in general it caused much the same anxiety as the atomic bomb has done in recent years.

When Nobel came to New York again in 1865 to form a company for the exploitation of his patents, he found it difficult to get lodgings. By the public he was looked on with a terror comparable to that which would be evoked today by the arrival from Europe of a bewhiskered stranger carrying in his satchel or brief case an atomic bomb or two. For the benefit of the press and a few invited guests, who remained at a discreet distance, Nobel himself demonstrated his new percussion cap by setting off a few blasts in an abandoned quarry in the Yorkville section of upper Manhattan.

During his early experiments in Russia, in the summer of 1862, he had shown his two older brothers that his new cap would cause nitroglycerin to explode even under water. In 1863 he returned to Stockholm and with his father's aid continued his experiments, even beginning to manufacture the new explosive on a commercial scale. Among his first customers were Swedish mine owners and the Swedish State Railways, then under construction. The capital stock of the first Nitro-Glycerine Company was only 25,000 kronor, or about $6,000.

In the autumn of 1864 the new plant blew up, killing five employees, including Nobel's youngest brother, Emil, who was then a university student working during his vacation. This tragedy caused the father to suffer a shock from which he never completely recovered. What error had been committed could not be determined, as only a few splinters were left. But Alfred Nobel did not despair, even though he was forced to set up his new plant outside the city limits and, provisionally, on a barge that could be towed off to a safe distance. Among his financial backers was J. W. Smitt, a wealthy Swede who had become known as the "Swedish Brandy King." The first foreign patent was sold to Norway outright for 20,000 kronor in cash. In all other cases, however, Nobel retained a part interest in foreign companies.

In 1865 he founded his first enterprise abroad, Nobel & Company, in Hamburg, Germany. There he built a factory and moved to Germany himself to supervise his new business. The same year he obtained



THE BUILDING OF THE NOBEL FOUNDATION IN STOCKHOLM

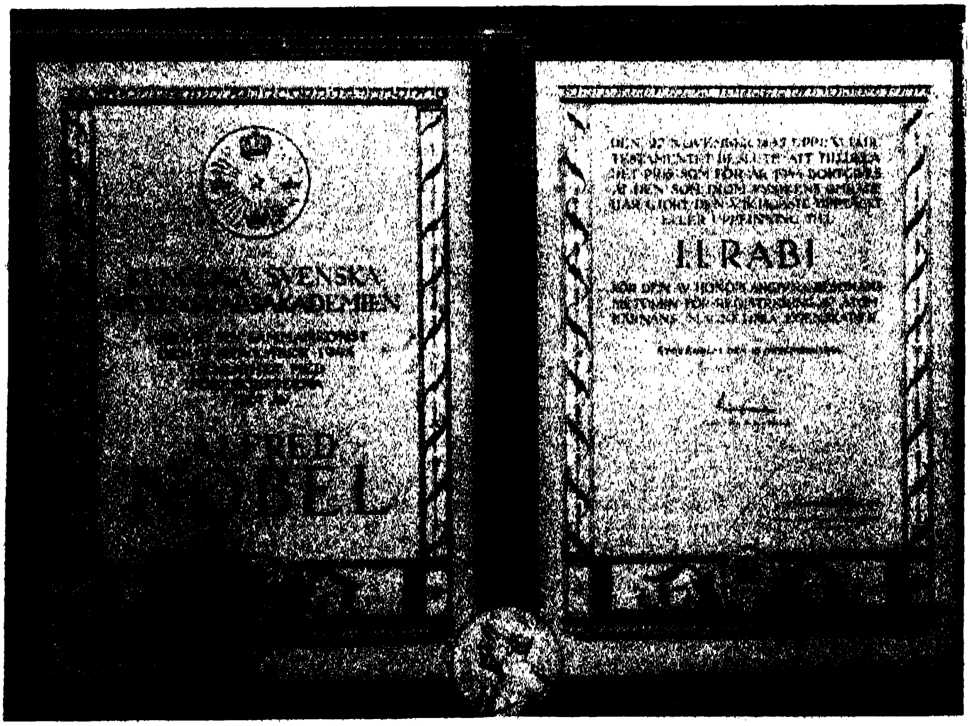
his first American patent and was immediately sued by an American financial adventurer named Shaffner, who had visited him in Stockholm to try to find out how nitroglycerin could be used. Without too much difficulty, Nobel won the suit and then formed The United States Blasting Oil Company to handle his American business. On many other occasions he had to use legal means to protect his rights, since attempts at infringements were frequent, particularly after he had discovered in 1866–67 a way to make the new explosive safe by mixing it with kieselguhr, an infusorial silicate, which he found in Germany. Legend has it that he hit on this idea by observing how the dangerous liquid had solidified upon being absorbed in the sawdust in which a leaky container had been packed, but this cannot be proved.

In the technique of blasting, the discovery of dynamite caused a world-wide change. In 1871 Nobel formed a company in England and soon afterwards, one in France. In both countries dynamite factories were set up and soon began to enjoy a tremendous business. A plant built at Ardeer, in Scotland, was capable of supplying one-tenth of the world's needs. At Pressburg, in Austria, still another factory was built.

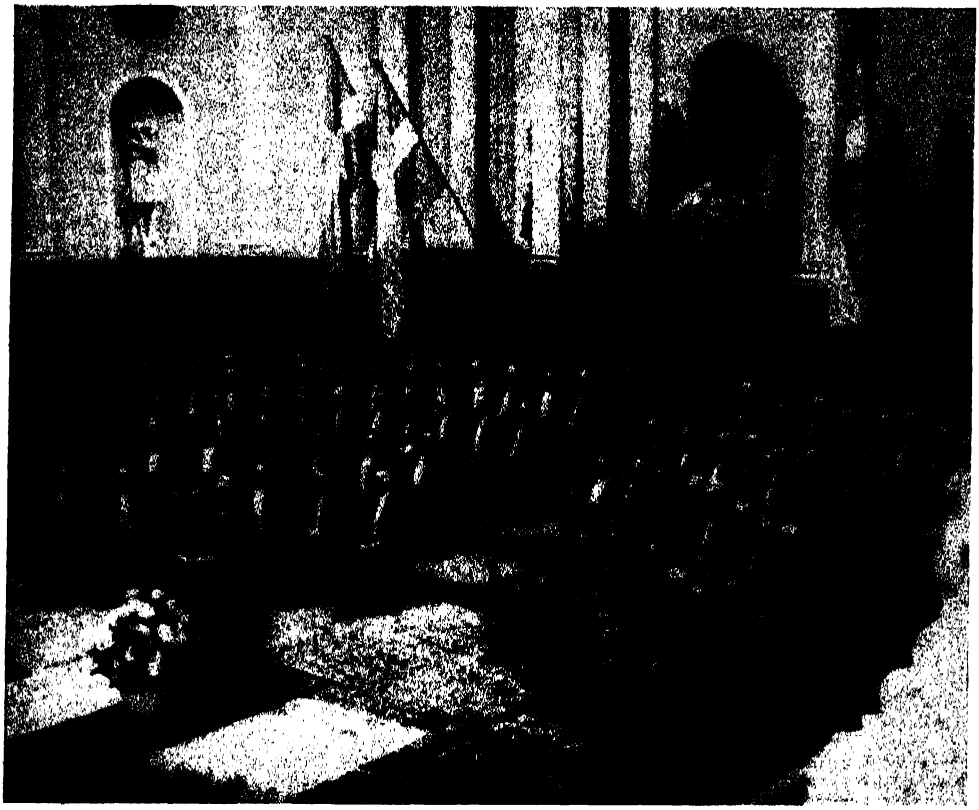
After the early 1870s, Nobel had his main residence in Paris, where he acquired a private house near the Arc de Triomphe. In Sevran-Livry, a Paris suburb, he had a private laboratory and there he evolved one improvement in explosives after another. In 1875 he launched his blasting gelatin, a colloidal solution of nitrocellulose, or guncotton, in nitroglycerin. The use of kieselguhr had made the blasting oil safe to handle, but it had also reduced its explosive power. Another of the Nobel legends relates that he thought of the new mixture after applying collodion to a cut in a finger. As he lay awake in the night suffering from the pain in his finger, it occurred to him that collodion, which was a nitrocellulose with a lower nitration, might absorb the nitroglycerin better than ordinary guncotton. At four o'clock in the morning he went to his laboratory to try out the idea, and when his assistants arrived after breakfast he was able to show them the first sample of the new blasting gelatin. This was another step forward in the production of high explosives. His next move was to develop a smokeless powder, which obviously was of great military significance. This new mixture he called "ballistite"; it was patented in 1887–88. It contained approximately equal parts of nitroglycerin and nitrocellulose, with a 10 percent addition of camphor.

The first government to buy the new powder was the Italian. This caused so much resentment in France that Nobel's private laboratory was closed by the authorities. He then moved to San Remo on the Italian Riviera, where he had a winter residence.

In England two scientists, members of an official Explosives Commission to which Nobel had submitted samples of his smokeless powder, with specifications, evolved another mixture that was only slightly different,



AN EXAMPLE OF A NOBEL PRIZE DIPLOMA AND MEDAL



CEREMONY AT THE STOCKHOLM CONCERT HOUSE

THE NOBEL PRIZES FOR 1947 ARE BEING AWARDED, DECEMBER 10, 1947.

which they called "cordite," the patents being assigned to the British government. To settle the question of patent rights, the English Nobel Company brought a friendly court action which, on slight technical grounds, was decided in 1894 in favor of the British. "I wish I had been able to reach a conclusion by which it would have been impossible to deprive Mr. Nobel of the value of a most important patent," wrote Lord Justice Kay, a member of the Court of Appeals.

Although the financial loss was not fatal to Nobel, he resented bitterly what he regarded as a rank injustice. To relieve his feelings he composed in English a play called "The Patent Bacillus," in which he satirized British court procedure and legal logic. It might have served as the libretto for a new Gilbert and Sullivan comic opera, though less soothing than some of them to British self-esteem. Of his achievements as an inventor Nobel was always proud, cynical though he often pretended to be in other respects.

The same year, 1894, he bought a controlling interest in a Swedish concern, the Bofors-Gullspång Company, which he started to expand for the manufacture not only of his new smokeless powder, but also of other munitions and various industrial products for which he had ambitious ideas. He even had an old manor house renovated and partly rebuilt as a summer residence, and a new laboratory prepared for experiments on a large, industrial scale. By that time he had no less than 355 patents in different countries, and from his records it is clear that he had begun to experiment with rocket projectiles and aerial torpedoes. He also worked with an artificial rubber (finally developed in the United States during World War II), and

THE AMERICAN PRIZE WINNERS FOR 1947

DR. CARL F. CORI AND HIS WIFE, DR. GERTY T. CORI, AT THE BANQUET IN STOCKHOLM CITY HALL FOLLOWING THE PRIZE AWARDS. THEY SHARED THE PRIZE IN MEDICINE WITH DR. B. A. HOUSSAY, OF BUENOS AIRES, ARGENTINA. THE CORIS WERE HONORED FOR DETERMINING THE PROCESS BY WHICH THE BODY STORES SUGAR IN THE LIVER AS GLYCOGEN AND THEN RECONVERTS THE GLYCOGEN TO SUGAR AS IT IS NEEDED.

with an artificial silk that was later perfected by others as rayon. His death cut short all these projects. As late as December 7, 1896, he had written in his own hand a letter to Mr. Sohlman, who was then in charge at Bofors, in which he commented on some samples of powder he had just received. "Unfortunately, I am again so poorly that I find it difficult to write these few lines," he scribbled, "but as soon as I can I shall return to the matter that interests us. Your Sincere Friend, A. Nobel." That was his last letter. In a few hours he suffered a stroke and three days later he died.

From his writings and the recollections of his friends and associates, it clearly appears that Nobel's was a complex and unusual personality. In a sense he epitomized the whole nineteenth century. Though he had had but little formal education (he had never graduated from any school or academy), he was a remarkable linguist and an omnivorous reader. Most of his mature years he spent in travel, usually to attend to the interests of his various industrial enterprises. He also had a very wide circle of acquaintances among the leading personalities of his day. Yet he himself was shy, avoiding publicity or public appearances as something improper. Once, when he was asked for his picture to be used in a magazine, he replied: "In these times of pompous and shameless advertising, only those persons who are specially qualified for it should allow their photographs to appear in print." He felt the same way about decorations. "One cannot refuse to accept them," he once wrote, "without being considered eccentric. But they make one feel embarrassed and are therefore undesirable." "I owe my Swedish Order of the North Star to my cook, whose skill won the approval of an eminent stomach," he wrote at another time. "As for medals," he went on, "I possess a gold medal presented by the Swedish Academy of Science. I am also a member of this academy, and set more store by this distinction than by the various Orders." In 1893 he accepted, however, an honorary doctorate from the ancient Swedish University of Upsala, which he had never attended.

> It would almost be a pity [he then wrote to one of his assistants] if I were to "shake off this mortal coil" now, because I have some particularly interesting things in hand. But since these fellows have made me a Doctor of Philosophy, I have become almost more of a philosopher than before. . . . My natural inclination [he wrote on another occasion] is less to honor the dead, who feel nothing, and who must be indifferent to marble monuments, than to help the living.

Superficially, Nobel may be considered to have been a man of contradictions. His profession of atheism was, however, but a cover for a deep-rooted idealism. "Our religious views differ possibly more in form than in substance," he wrote to the pastor of the Swedish church in Paris, to whom he had just sent a contribution twice as large as the sum requested, "for we are both agreed that we should treat our neighbor as we would be treated by him."

Being endowed with an unusual intellect, and feeling in need of human affection, he suffered from a loneliness that sometimes bordered on melancholia. He was deeply devoted to his mother, who lived until 1889. He nearly always managed to spend Christmas with her in Stockholm. He also knew, and carried on correspondence with, many women, usually brilliant ones, but he remained a consistent bachelor. Through an advertisement for a housekeeper, "to be the hostess of his Paris home," he became acquainted with Bertha von Suttner, a young Austrian noblewoman, who had been thwarted in love. In the advertisement he had described himself as "an elderly well-to-do person"; actually, he was just over forty. She came to Paris, but before assuming her new duties returned to Austria—and matrimony. Toward the end of his life he resumed correspondence with her, and she may have inspired his endowment of the annual Peace Prize.

KING GUSTAF AND MRS. CORI

Not only was Nobel personally generous with his money during his lifetime, he also provided his mother with enough funds to enable her to exercise an extensive charity. After his death the letters he had received were found tied up in bundles labeled "letters from men," "letters from women," and "letters from beggars."

> Not a day passes [he once wrote] without the post bringing me at least two dozen such requests for help, averaging about twenty thousand crowns; enough to demoralize even J. Gould, Vanderbilt, and Rothschild. . . . Although my income is considerable, I have been forced in each of the two last years, to supplement it with a million francs taken from my capital. One can go on like this for a time, but not forever.

His frail health had often forced him to spend much time at various watering places

AWARD FROM THE KING

KING GUSTAF DELIVERS A 1947 NOBEL PRIZE TO SIR EDWARD V. APPLETON, WINNER IN PHYSICS.

and other health resorts. His extensive travels and his irregular life did not help matters. When he was absorbed in testing a new idea he often forgot his meals. His interest in medicine and physiological problems, which caused him to endow the prize for new discoveries in those fields, was lifelong. At one time he planned to finance a series of medical investigations, "directed toward ascertaining by means of experiments the nature of physical processes connected with disease, and, above all, toward discovering by the experimental method means for the curing of diseases," to quote one biographer. He often stated that he was considering the founding of an institute for experimental medical research, but he never found time to carry out the plan. Sometimes he hinted that medical theories might be a hindrance and that a person free from them might be able to get at the heart of a problem all the more readily.

That was the pragmatic way he worked in physics and chemistry, and often he hit on practical ideas that became revolutionary. At the same time, he appreciated the facts discovered by professional scientists, and his endowment of prizes to encourage them was not surprising, nor that he selected the Swedish Academy of Science, his membership in which he so highly treasured, to nominate the laureates. On the whole, the selections so far have been endorsed by an informed world opinion.

In Nobel's time the so-called social sciences were poorly developed. He left no prizes to economists, sociologists, or political scientists. His interest in human relations, however, was often manifested. He believed in education and that science could ultimately make men not only wise but happy.

> To disseminate enlightenment is to disseminate prosperity [he wrote]. I mean general prosperity and not individual wealth—and with the attainment of prosperity disappears the greater part of the evil which is our heritage from darker times. The triumphs of scientific research and the ever increasing field which is coming under its sway give us reason to believe that the microbes of the soul as well as those of the body will soon be exterminated, and that the only kind of war which humanity will wage in the future will be war against these microbes.

When he saw how his discoveries, which he had hoped would be used to promote civilization and reduce human drudgery, were being turned more and more toward military purposes and pure destruction, he naturally brooded over the problems of peace and war. To Bertha von Suttner he said at Zurich in 1892, "My factories may end war sooner than your [peace] congresses. The day when two army corps will be able to destroy each other in one second, all civilized nations will recoil from war in horror and disband their armies." He did not foresee how few nations were to prove themselves "civilized" when external danger threatened; nor the growing strength of nationalism, the spirit of which has been capitalized by warmakers.

> I am beginning to believe [he wrote in October 1892 to a friend in Belgium] that the only true solution would be a convention under which all governments would bind themselves to defend collectively any country that was attacked. Such a treaty might lead gradually to partial disarmament, which is indeed the only kind of disarmament possible, for it is necessary that there should be an armed force to maintain order. In former days, governments used

> to be more narrow-minded and clamorous than their subjects; but nowadays it seems as though the governments were endeavoring to tranquilize the idiotic passions of a public that is aroused by pernicious newspapers.

About the methods by which "pernicious" newspapers can be utilized by governments to serve their purposes much has been learned since Nobel's day. The convention he favored was roughly in the Covenant of the League of Nations, but that, too, failed. Now we are trying the idea once more in the United Nations. The annual Nobel Peace Prize, for which the candidates are selected by a Norwegian Parliamentary Committee, serves as a stimulus to serious thinking about a problem that half a century or more ago was close to the heart of Nobel. That he established the Peace Prize as a salve to his conscience for having invented high explosives cannot be demonstrated. Improved weapons have never been the cause of war; on the contrary, the atomic bomb may achieve the very results that Nobel hoped his dynamite, explosive gelatin, and ballistite would bring about.

> I should like [he wrote Bertha von Suttner in

NOBEL PRIZE WINNERS SIR EDWARD APPLETON AND HAROLD C. UREY

SIR EDWARD APPLETON (*right*) IS SECRETARY OF THE DEPARTMENT OF SCIENTIFIC AND INDUSTRIAL RESEARCH, LONDON. PROFESSOR UREY, NOW AT THE UNIVERSITY OF CHICAGO, WAS THE 1934 WINNER IN CHEMISTRY.

DR. B. A. HOUSSAY

HE SHARED THE 1947 NOBEL PRIZE IN MEDICINE AND PHYSIOLOGY WITH DR. AND MRS. CORI.

> January 1893] to make use of part of my fortune for the establishment of a prize fund, to be distributed in every period of five years (we may say six times, for if we have failed at the end of thirty years in reforming the present system, we shall inevitably revert to barbarism). This prize would be awarded to the man or woman who had done the most to advance the general idea of peace in Europe. I do not refer in this sense to disarmament, which can be achieved only by slow degrees. I do not necessarily refer even to compulsory arbitration between the nations; but we should soon achieve the result (and it can be done) that all states bind themselves absolutely to take action against the first aggressor. Wars will then become impossible, and we should succeed in compelling even the most quarrelsome state, either to have recourse to a tribunal or to remain quiet. If the Triple Alliance [Germany, Austria, and Italy] instead of comprising three states were to secure the adherence of all, peace would be ensured to the world for centuries.

Such was his dream two generations ago. That even shreds of civilization could survive two world wars would probably have surprised and, in a way, encouraged him.

Nobel's interest in literature, primarily poetry and drama, though he also both read and tried to write fiction, dated from his early youth. Much of his political radicalism, or at least his aversion to the personal autocracy and dictatorship represented by the Russian czars, he must have absorbed from his early reading of some of the great Russian novelists. His favorite author and the one who influenced his mind most, both politically and aesthetically, was the English poet Percy Bysshe Shelley. The verse he wrote himself (and later burned) was usually in English. He made it clear by various remarks that, just as he intended to encourage young scientists to continue their research, he wanted to help young writers who had something to say for the benefit of mankind. In his will he expressly stated that the Literary Prize should be given for "the most distinguished work of an idealistic tendency." Clearly, he did not favor either satire or hardboiled fiction.

WHEN he sat down in Paris in the fall of 1895 to formulate in his Last Will and Testament the ideas he had entertained for years, he wrote as follows:

> I the undersigned Alfred Bernhard Nobel hereby declare after mature consideration that my last Will with regard to the property which I may leave on my death is as follows:
>
> With the residue of my convertible estate I hereby direct my Executors to proceed as follows: They shall convert my said residue of property into money, which they shall then invest in safe securities; the capital thus secured shall constitute a fund, the interest accruing from which shall be annually awarded in prizes to those persons who shall have contributed most materially to the benefit of mankind during the year immediately preceding. The said interest shall be divided into five equal amounts, to be apportioned as follows: One share to the person who shall have made the most important discovery or invention in the domain of Physics; one share to the person who shall have made the most important Chemical discovery or improvement; one share to the person who shall have made the most important discovery in the domain of Physiology or Medicine; one share to the person who shall have produced in the field of Literature the most distinguished work of an idealistic tendency; and, finally, one share to the person who shall have done most to promote the Fraternity of Nations and the Abolition or Diminution of Standing Armies and the Formation and Increase of Peace Congresses. The prizes for Physics and Chemistry shall be awarded by the Swedish Academy of Science [Svenska Vetenskapsakademien] in

Stockholm; that for Physiology or Medicine by the Carolinian Medico-Surgical Institute [Karolinska Institutet] in Stockholm; the prize for Literature by the Academy in Stockholm [Svenska Akademien] and that for Peace by a Committee of five persons to be elected by the Norwegian Storting. I declare it to be my express desire that, in the awarding of prizes, no consideration whatever be paid to the nationality of the candidates, that is to say, that the most deserving be awarded the prize, whether of Scandinavian origin or not.

This is my only valid Will and cancels any previous testamentary dispositions that may come to light after my death.

Paris, the 27th November, 1895. ALFRED BERNHARD NOBEL.

Although from a legal point of view the will he wrote himself to dispose of one of the largest fortunes of his time was obviously defective and might very well have been refused probate on purely technical grounds, its meaning was clear enough. Its worst fault was its failure to state who should administer the residuary fund on a permanent basis. The executors were only to settle the estate and then convert the remainder into cash, which was then to be invested in "safe" securities. Who should take charge next?

This defect was cured by an act of the Swedish government providing for a permanent board of trustees representing the government as well as the institutions designated to select the prize winners. Thus, the annual procedure in regard to the management as well as the distribution of the prizes is regulated.

Nominations with proper motivation can be made by former winners and also by recognized institutions of learning or official academies in any country. Such nominations must be made before January 31 of the year in which the prize in question is to be awarded. The faculty of the Carolinian Institute, which decides on the prize in medicine and physiology, usually makes the announcement before October 31; the Academy of Science, in regard to the prizes in physics and chemistry, before the middle of November. The Swedish Academy, which selects the winners of the Literary Prize, usually is ready with its decision about the same date; but the Norwegian Parliamentary Committee often withholds the name of its candidate until the eve of the distribution in Oslo on the anniversary of Nobel's death.

SIR ROBERT ROBINSON

PROFESSOR AT OXFORD UNIVERSITY, HE WAS THE WINNER OF THE 1947 NOBEL PRIZE IN CHEMISTRY.

On that date, December 10, the new prize winners are invited to be in Stockholm. Usually they also bring their wives or husbands. At five o'clock in the afternoon there is a full-dress ceremony in the Stockholm Concert House, at which the King and members of the royal family are present, as well as court officials, members of the diplomatic corps, representatives of the Nobel Foundation and of the prize-awarding academies, government ministers and high officials, and prominent private citizens. It is apt to be a solemn occasion, with classical music and bugle signals at the various stages of the ceremony. The chairman of the appropriate Nobel Committee introduces each winner by a speech in which the reasons for the selection are stated in considerable detail. The winner is greeted by the King and then presented, amidst applause from the audience, with an especially designed diploma, a gold medal, and a certificate to the effect that the appropriate sum of money is at his disposal in a specified bank. The amount is always indicated in Swedish currency, its value in other media depending on the current rate of exchange.

After this rather stiff, traditional ceremony is over, the whole audience, and sometimes additional guests, repair to the Stockholm City Hall, in the Golden Room of which the Nobel Foundation gives a formal banquet with members of the royal family (though not the King) present. Toasts are offered to the King and to the sovereigns of the various states from which the winners hail. Customarily, a representative of the Nobel Foundation speaks. For years Mr. Ragnar Sohlman has managed to recall, each year, a new anecdote or a fresh quotation from Nobel's writings. The winners are also expected to make a few remarks in their own languages, of which translations appear in the next morning's newspapers. After the banquet the guests assemble informally around small tables in the Blue Room or central reception hall of the building. There the Stockholm university students' chorus sings, and their leader may make an address to the new prize winners in the native language of at least one of them. Afterwards there is a good deal of visiting back and forth among the tables, and as the evening progresses the atmosphere becomes more and more sociable. The next evening the King gives a state dinner in the royal palace, at which there are no speeches. Before leaving Stockholm, each prize winner is expected to make an address or deliver a lecture on a subject connected with his work before an assembly of his Swedish colleagues. All other functions to which the laureates are invited are informal.

TELESCOPE ON MOUNT PALOMAR

That earth was level, mankind once conceived;
Only his eyes could trace its true dimension,
Till Galileo, with a strange invention,
Destroyed the myth each man and child believed.
Then with new eyes to probe beyond his sight,
Man found a universe where he could grope
Among the stars: a minute telescope
That hurtled him through centuries of light.

Yet still so much he could not see, or guess:
Is space a curve? The universe expanding?
What is infinity? And nothingness?
Now, trembling near the brink of understanding,
Man, through the vast new lens at Palomar,
May bridge the ultimate void from star to star!

MAE WINKLER GOODMAN

WHAT IS SCIENCE TALENT?

ZACHARIAH SUBARSKY

Mr. Subarsky, after taking his M.S. at Columbia, taught biology in several New York City high schools. Formerly head of the Science Department at the Benjamin Franklin High School, he is now chairman of the Department of Biology and Introductory Science at the Bronx (New York) High School of Science, from which several winners in the Science Talent Search have come.

IN HIS report *Science and Public Policy,* Dr. John R. Steelman, Chairman of the President's Scientific Research Board, recommends that in the next decade total expenditures for research and development should be doubled, that expenditures for health and medical research be tripled, and those for basic research be quadrupled. The report points out that by 1957 we should be devoting at least 1 percent of our national income to research and development in universities, industry, and in the government. This would call for an annual expenditure of some two billion dollars, much of this being public funds.

The effective utilization of so vast a source of revenue presupposes the existence of an adequate number of mature scientific investigators. Unfortunately, the supply of trained scientific personnel needed even today is reported as woefully inadequate (*Steelman Committee Report, Vol. IV*). A vast and carefully thought-out plan for the detection and cultivation of science talent would, therefore, become strategic in any such national program. It was probably in anticipation of this bottleneck that the Bush Committee Report, *Science, The Endless Frontier,* called for the establishment of six thousand scholarships for "science-talented" high-school graduates, each scholarship to provide full tuition and living expenses at any recognized college or university.

Fortunately, a search for science talent is already under way among high-school students. Annually, forty high-school seniors, winners in the national Science Talent Search, are invited to Washington, D. C., to compete for science scholarships in the five-day Science Talent Institute, conducted by the Westinghouse Educational Foundation in cooperation with Science Service and its Science Clubs of America. The Science Talent Search represents an intriguing educational venture entered into by a private corporation. The project is admittedly experimental. Nevertheless, its influence has already proved to be nation-wide, and its implications are far-reaching. Educators have long been pondering over scientific method, scientific attitudes, and scientific thinking. The Bush Committee recommendations and the Science Talent Search bring into focus a new facet in science education, namely, the endowments and predilections that are essential in the development of a scientist. What are these endowments and predilections? In short, what *is* science talent?

A fundamental ingredient of science talent is a high degree of innate curiosity. This generally manifests itself early in the life of a child. What makes Sam incessantly and persistently tinker with electric currents? What moves Harry to absent himself from school during the height of the bird migratory season to go out and observe the waves of warblers? What drives Alice to the marshes to collect still more different kinds of protozoa, and what keeps her bent over the microscope for hours at a stretch? True, in many cases such "passions" gradually peter out. In some cases, however, curiosity ripens into enduring interest, which leads eventually to a lifetime of endless adventure in research.

Another manifestation of science talent is the ability to spot or detect an incongruity or inconsistency (commonly called a "problem") when confronted by facts. How many bacteriologists from Pasteur to Fleming have had bacterial plates contaminated with molds! But it took a Fleming to spot the zone of inhibited bacterial growth and to pursue the investigations that led to the discovery of penicillin. Many horticulturists and animal husbandmen had hybridized living organisms before Gregor Mendel. But it took a Mendel to become so challenged by the very fortuitousness of the results of his predecessors, Kolreuter, Gartner, Herbert, Lecoq, Wichura, and others, that he was impelled to set up the experiments now regarded as classical. The mind of Joseph Black was arrested by the fact that the temperature of a mixture of ice and water does not rise even while a fire is under it, and that the temperature of boiling water in a kettle does not rise even as it sits over an open flame. Black was thus led to develop the concepts of latent heat and specific heat, concepts of great practical importance in engineering and meteorology. Similarly, Roemer was startled by discrepancies in the times of the eclipses of Jupiter's satellites, an observation that led him to the fundamental discovery of the speed of light.

Photo by Jerome Metzner

STUDENTS IN THE FIELD

When children are placed in a suitable environment, this element of science talent, namely, the ability to spot a problem, may soon manifest itself. A group of high-school students was assigned the job of maintaining several cultures of bacteria. Among these bacteria was the organism *Serratia marcescens.* Only one boy in the group seemed to be bothered by the observation that colonies of this organism are sometimes red, sometimes pink, and sometimes even colorless. Why did these different colors occur? He was interested to the extent of setting up an elaborate series of experiments to get the answer.

In the same laboratory dozens of students had observed that a large number of frogs kept in the frog tank were infected with the disease called "red-leg," but only one student was puzzled by the fact that the proportion of infected frogs in the laboratory was greater than the proportion of infected frogs he observed in nature. He set up a number of experiments to determine the reason for the difference. As a result of his findings, the number of infections in the frog tanks was cut down from about 60 percent to only 2 percent!

Just as important as the keenness to spot incongruities and inconsistencies is the power to develop by intuition, as it were, tentative explanations that can be tested by experimentation or by wider observation. Kepler's laws of planetary motion, Newton's theory of the propagation of light, Avogadro's hypothesis that equal volumes of gases have the same number of molecules, Mendelyeev's prediction of the properties of elements that had not yet even been discovered, Maxwell's theory of the propagation of electromagnetic waves, the electrolytic theories of Van't Hoff and Arrhenius, Kekule's formula for the benzene ring, the theory of organic evolution advanced independently by Darwin and Wallace, Morgan's theory of the gene as the vehicle of biological heredity, the planetesimal hypothesis of Chamberlin and Moulton, Einstein's theory of relativity—such products of creative imagination are only a few of the milestones in the history of science. Indeed, it can be said that modern science is the product of human imagination checked and double-checked against experience. To few mortals is it given to make such breathtaking syntheses as those listed above. Nevertheless, a degree of creative imagination is called into play in the daily life of the scientist, who, confronted by a problem, must formulate hypotheses and plan experiments to test them.

Photo by Martin Weiss

ANNEALING A THERMOMETER

THIS INSTRUMENT WAS MADE BY THE STUDENT.

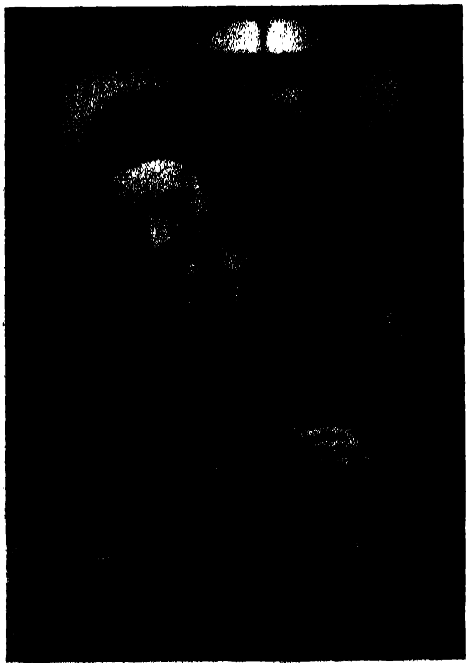


Photo by Martin Weiss

MARCIA'S MICROSCOPE

MICROSCOPY TAKES ALL HER SPARE TIME. WHY?

Thus, the boy who worked on red-leg infections in frogs suspected that the low incidence of red-leg among wild frogs might be due to the fact that wild frogs tend to burrow into the muddy bottom of pools, a condition that might be deleterious to the infective agent. He was guided by this hypothesis in setting up his experiments.

Another student's curiosity was aroused by an incidental observation in an experiment. Bernard became interested in the comparative effects of carcinogens in differentiated tissue (tail of a tadpole) and undifferentiated tissue (limb bud of the same tadpole). In his control tadpoles he injected the normal solvent (sesame oil) of the carcinogen employed. He observed necrosis in all cases at the site of the injection of both the control and experimental animals. This fact he did not permit to pass as a mere *observation.* He made it the subject of a further experiment in which sesame oil *alone* was injected into the experimental tadpoles and physiological saline was injected into the controls. He found to his surprise that, for tadpoles at least, sesame oil, an oil commonly used as a solvent for biologically active substances such as hormones, was far from biologically inert itself.

A third ingredient of science talent is the ability and predilection to think in quantitative terms. Such talent was shown by Faraday when he discovered that the quantities of different substances deposited by an electric current of given intensity were proportional to their atomic weight divided by their chemical valence; and by Joule who found that a definite amount of heat appears when a given amount of mechanical energy is converted into heat and that, conversely, a definite amount of mechanical energy appears when a given amount of heat is converted into mechanical energy. In fact, the evolution of a given science is measured by the extent to which qualitative observations are translated into quantitative terms by instrumentation, by the application of mathematics, or by other ingenious devices.

Recently, scientists have devised international units of vitamins, hormones, antibiotics, and pesticides. Experimental work with these substances becomes ever more quantitative. For example, in describing some recent researches on new fungicides, Dr. K. Starr Chester writes (SM, 1948, 66, 157):

> When the dosage of the chemical [fungicide] is varied, there is corresponding variation in response of the fungus spores. If dosage is plotted against toxic effect on a probability-logarithmic grid, there results the "dosage-response curve," a beautiful tool revealing much about the dynamics of fungicidal action.

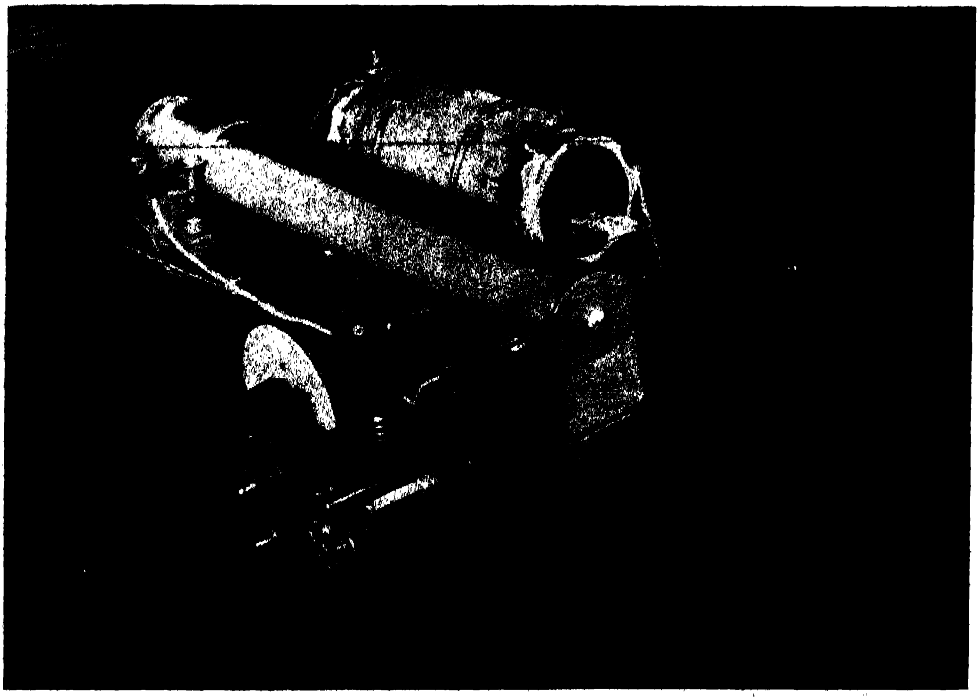


Photo by Martin Weiss

A MICROINCINERATOR CONSTRUCTED BY A HIGH-SCHOOL STUDENT

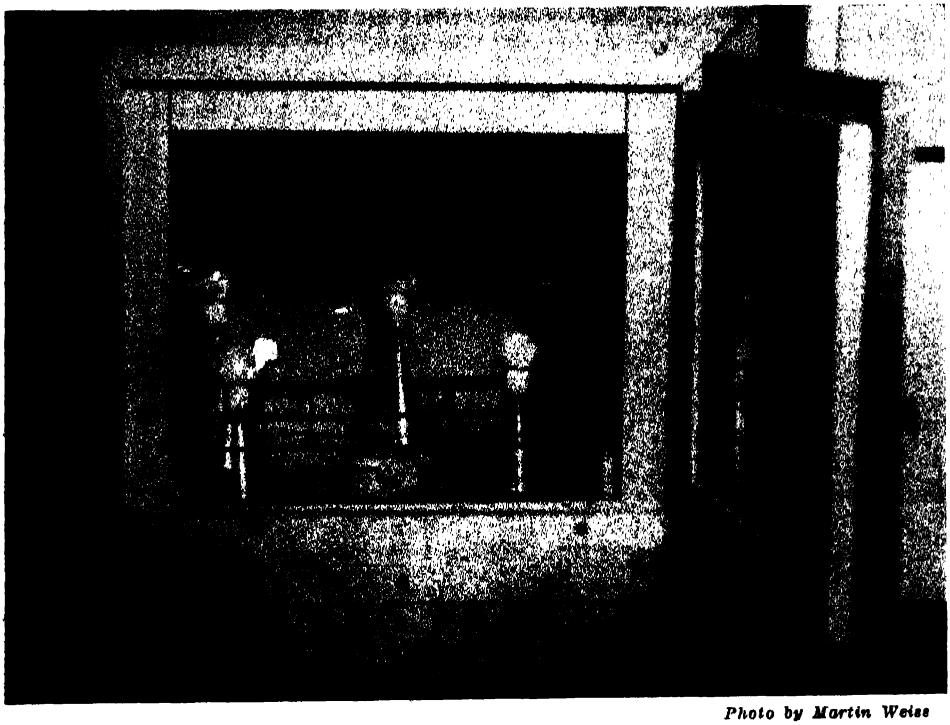


Photo by Martin Weiss

AN INCUBATOR DESIGNED AND MADE BY TWO HIGH-SCHOOL STUDENTS

Herbert, a high-school student, became interested in the fact that when bacteria are transplanted into a test tube there is a lag in their rate of reproduction despite the apparent optimum of conditions in the tube. Here, in his own words, is the way he went about trying to find out the reason for this lag:

> A culture of *Escherichia coli* in the logarithmic phase is treated in such a way as to stop its growth, by altering such conditions as food, temperature, pH, oxygen, carbon dioxide, etc. After treatment, the organisms are subcultured in fresh media, and the growth plotted for from three to four hours. Controls must be used in all cases.

This boy is manifesting a high degree of talent not only in the matter of formulating hypotheses but also in thinking quantitatively.

Robert, another high-school student, intensely interested in bird life, became curious about bird tracks. He proceeded to make plaster casts of tracks of birds' feet, but he was not satisfied with merely making and collecting the casts. He took measurements (Table 1).

TABLE 1

SUMMARY OF 45 CASTS

	MID-TOE		NUMBER OF CASTS	FREQUENCY OF PALMATION
	Length in mm.	Width in mm.		
Piping plover	18–20	1–5.2	2	....
Semipalmated sandpiper ..	19–21	1–1.5	5	2
Sanderling ...	20–24	2–3.5	6	....
Semipalmated plover	23–27	2–3.0	7	....
Spotted sandpiper ..	24–27	1–2.5	5	....
Killdeer	28	3.5	2	....
Ruddy turnstone ..	26–31	2–4.0	7	....
Dowitcher ...	31–40	2–5.3	5	....
Black-bellied plover	36–49	4–6.0	3	1
Red-backed sandpiper ..	25–27	2–3.0	3	....

Here, in his own words, is how this boy used this information:

> I had practically haunted the marshes around Eastchester and Pelham Bays and had never seen or heard a Clapper Rail. After discovering their tracks and noting the spots they frequented, I was able to find several of these secretive birds. This illustrated one use of the method. It may also be used to distinguish between the Eastern and Long-billed Dowitchers, and the Eastern and Western Willets. This is a difficult task with binoculars, and one for experienced observers only.

Here is a case of a high degree of specialized curiosity coupled with quantitative thinking.

A fourth ingredient of science talent is manual dexterity, manipulative ability, mechanical-mindedness, or whatever other name such ability may be called. Between 1924 and 1927 Van Slyke described an apparatus he had devised "for the combined determination of all gases in a sample of blood; methods for micro-analysis of fractions of a c.c. of blood; for determination of gases in liquids saturated at high tensions; for determination of dissolved gases in water; and for air and general gas analysis." This gasometric apparatus was nothing more than an ingeniously assembled collection of vessels, tubes, and stopcocks; yet this apparatus, with the many modifications that have since been made in it, constitutes a most important contribution to physiology and engineering. In this category belong also the many types of colorimeters (which compare color intensities of solutions), nephelometers (which compare opacity of suspensions), pH meters (which measure the concentration of hydrogen ions), the Geiger-Müller counter (which measures nuclear radiations), the many types of centrifuges (which separate suspended substances of different masses). One might go back to such "ancient" instruments as the thermometer, microscope, and telescope to get an appreciation of what these "mechanical" devices have meant in the development of science.

Even high-school students, under certain conditions, display mechanical-mindedness and manipulative ability. A group of students were growing microscopic animals. There were requests for some of these animals from distant places, to which they could be mailed. It occurred to one boy that these animals might be sealed in a glass ampule and thus be shipped without danger of spilling or drying. He worked it out successfully. Another student who had demonstrated a beautifully made Tesla coil, when asked where he got a certain part of it replied, "I cut it off my mother's andirons."

Stuart and Morton were about to embark upon a project involving surface growth of a mold. The school incubator was too small to accommodate the liter flasks. The boys would not let this difficulty stand in their way. They set about constructing a large enough incubator. Without very much previous experience for the task, they successfully solved the problems of insulation, thermostatic control, and interior design, and they came forth from a cellar workshop with a very creditable piece of laboratory equipment.

Bert, a member of the Cancer Club, came across a paper describing some work involving the microincineration of tissue. He examined the figures carefully, made his own modifications, procured the parts, and constructed the apparatus he is now using in his own microincineration work.

The cases of these high-school students have been cited to exemplify the elements of science talent: specialized and persistent curiosity, alertness in detecting inconsistencies, creative imagination in projecting possible explanations for such inconsistencies, and a high degree of mechanical-mindedness. How can such talent be detected and cultivated? The importance of this problem will become accentuated when and if Congress passes the bill establishing a national Science Foundation. Fortunately, we shall be able to lean on the experience of the national Science Talent Search for the Westinghouse scholarships, now in its eighth consecutive year. However, this is a search for talent that is flourishing. To enable it to flourish, we must set up school conditions that will bring out the best science talent in American youth.

AMERICA GROWS CORN

HOWARD ZAHNISER

When spring comes to America
The farmers of forty-eight United States
Get up from their breakfasts
And plant a hundred million acres of Indian corn.

Out of the south into Texas
The corn-planting time moves into February,
Drifts northward thirteen miles a day,
Sweeps northward across a nation,
Comes to March, to April, to May,
Comes to forty-eight sovereign and united states.

And in every State of the Union
The farmers get up and plant their Indian corn.

(Only man grows corn.)

In Iowa where the tall corn grows,
In Ohio and Illinois,
In Indiana, Kansas, and Nebraska,
In Minnesota and Dakota,
In Georgia, where more acres grow corn than grow cotton,
In Maine and California,
In Michigan, Kentucky, Tennessee, Alabama, and Louisiana—
In every State of the Union—
The farmers of America plant corn.

(Only men and their women grow corn.)

Seed corn on a hundred million acres,
Planted with care and with pride
To rise into the rank and file
Of the greatest crop that man has yet mobilized.

Roasting ears, corn-on-the-cob, succotash,
Canned corn, dried corn,
Corn-meal mush,
Johnnycake,
Corn flakes,
Corn syrup.

Seed corn rising to feed
The cows, the pigs, the poultry,
The livestock of forty-eight States.

Seed corn rising to feed
Men and women and boys and girls.

Seed corn rising to feed them
With as good food as the good earth grows.

(Only men and their women plant corn in the earth.)

Say "corn" to the English and they think about wheat.
Say "corn" to a Scotchman,
And he kens you're talking of oats.
Say "corn" to any man,
And he thinks of the greatest grain of his native land.

Say "corn" to the American, and he knows you mean CORN.

Be English and call it maize.
Be scientific with *Zea mays*.
Call it whatever you will,
It grows on a hundred million American acres,
Grows and yields right well—
Yields corn enough
To give two dozen bushels to every man, woman, and child
In the United States of America.

Clear the living rooms of America!
Make way for Indian corn!

For every man, woman, and child in the United States of America,
Two dozen bushels of Indian corn.

Picture the baby
On the living room floor
And twice a dozen baskets full of corn.

Picture the preacher
In the pulpit
And twenty-four bushels of Indian corn.

Picture every man, woman, and child
In the United States of America
With twice a dozen bushels of corn.

Make it into succotash, corn meal, corn flakes, and syrup.

Feed it to the cows, the hogs, the chickens.

Feed ninety percent of America's corn
(And we do. We usually do.)
Feed ninety percent of America's corn
To the cows, the hogs, the turkeys, the chickens—
And taste the beef,
The pork,
The milk, the cream, the cheese, the eggs,
And smell the bacon in a thousand kitchens.

Courtesy of Associated American Artists Galleries, New York, N. Y.

"THE CORNFIELD," BY JOHN STEUART CURRY, 1935

And who grows the corn?

Only men and their women grow corn from the earth.

Columbus discovered America in 1492.
Columbus discovered CORN.
Cortez marched on the Aztecs,
And he ate corn.
Pizarro pillaged Peru,
In 1532,
And the Incas were harvesting corn.

But no man knew
Where the wild corn grew,
And no man knows
Where wild corn grows.

(Only man grows corn from the earth.)

The scientists of America study corn—
Yet they don't know
Where the wild plants grow—
The scientists of America study corn,
And dreaming and scheming,
And plotting and planning,
With tall talk
Of chromosomes, genes, and megaspores,
The plant breeders of America
Do a new thing under the sun
With their old friend *Zea mays.*

And Indian corn is hybrid corn!

Sing the praises of hybrid maize.
Sing the praises of man.
Men with their scheming have managed to raise
More bountiful corn than Nature can.

Take if you will an inbred strain.
Make your selections again and again.

Do it some more
And choose out four.
Call one A, and call one B.
Call one C, and call one D.

Call them in rhyme.
(It's corn-planting time.)
It's corn-planting time
For A with his B
And C with his D.

Watch the summer winds sway
The tassels on A,
The pollen-rich tassels of A,
And at harvest time see
The kernels on B—
The full-ripened seed of detasseled B,
Predestined to mate with the seed of CD.

For C with his D
Has a way
You can say
Is like A's with his B.

(But the grain they both yield
In the seed-grower's field
Makes no johnnycake, corn meal, or succotash,
Makes no chicken corn, cow feed, or hog mash.
Its fate
Is to mate;
Its deed is to breed.)

So call them in rhyme again
It's corn-planting time again.
And AB
With CD
Will cross and sublime again.
And the double-crossed grain that their harvest will yield
Is a new kind of seed for the corn farmer's field.

Thus Science discloses
Controlled heterosis,
And hybrid vigor
Makes corn yields bigger.

And Indian corn is HYBRID *corn.*

And the farmers of America
In the Year of Our Lord Nineteen Hundred and Thirty-three
Rise up from their breakfasts
And plant a hundred thousand acres of hybrid corn.

And when harvesttime comes
They like it.
And ten years later
They rise up from their breakfasts
And plant fifty MILLION acres.

Sing the praises of hybrid corn
To the tune of twenty percent.
Hybrid vigor in *Zea mays*
Boosts the yield by twenty percent.

Yes, SIR!
The corn growers of America
Get Six Hundred and Fifty Million bushels more
For their time and their trouble
Than they could have
Without the magic of hybrid corn.

Sing the praises of hybrid corn
To the tune of twenty percent.
Hybrid vigor in Indian corn
Boosts the yield by twenty percent:

Feed it to the hogs of America.
Feed Six Hundred and Fifty Million bushels of Indian corn
(And we feed them more than that. We certainly do.)
Feed Six Hundred and Fifty Million bushels of Indian corn
To the greedy, grunting, squealing, pork-growing pigs of America,
And how much pork do you get?

BROTHER, you get pork enough
To give three pounds
To every man, woman, and child on the face of the Earth.

Sing the praises of hybrid corn
To the tune of pork at your meals,
For seven BILLION pounds of pork
Is hybrid vigor in *Zea mays.*

But no man knows
Where the wild corn grows.

(Growing corn is a human thing.)

When spring comes to America
The farmers of forty-eight United States
Get up from their breakfasts—
They and their sons and their daughters too—
In every State of the Union
The farmers of America
Rise up from their breakfasts
And plant a hundred million acres of Indian corn.

And before
And behind them
Across the cornfields of America
A
nation
moves
with
them.

AMERICA GROWS CORN!

GROUP MEDICAL PRACTICE

BENJAMIN F. MILLER

Dr. Miller (M.D., Harvard, 1933) interned for a year at New York Hospital and then spent two years as a National Research Fellow. In 1937 he went to the University of Chicago as assistant professor of medicine. Last year he took on a double assignment in Washington, D. C., with the National Research Council and the George Washington Medical School. The following article is from Chapter 5 of Dr. Miller's book You and Your Doctor, *which won honorable mention in a contest sponsored by Whittlesey House and* Science Illustrated.*

THE practice of medicine by groups of physicians has already proved its value in a number of places throughout the country. Groups of various sizes have been set up and are flourishing. They range from combinations of a minimum of five to ten physicians to the large group clinics, such as the Ross-Loos in Los Angeles, which cares for approximately 60,000 individuals, the successful Permanente Foundation set up by Henry Kaiser on the west coast, the Mayo Clinic, the University of Chicago Clinics, the group clinics set up by large trade unions, and many others.

In these groups, doctors band together in a logical combination to improve their standards of practice. Physicians possessing different skills work together, sharing not only their experience and specialized training, but also all their equipment. As a result, the patient has ready access to all types of medical treatment and to the most modern facilities. In many instances the groups are organized on a prepayment basis, which guarantees continuous medical care to the subscribers.

Group medical practice has certainly not been welcomed by all medical societies, and was actually fought bitterly. Its successful survival is the proof that the public needs and welcomes this kind of medical service. In recent years the opposition has virtually given up the fight. It continues to carry on a strategic retreat—a losing battle—for it is forced to recognize that the progress of group medicine cannot be checked.

Doctors and medical students alike look with favor on some fundamental change in the structure of medical practice. In a recent survey conducted by the American Medical Association, 63 percent of all doctors polled indicated a preference for some form of practice other than the traditional form of solo practice on a fee-for-service basis. Fifty-three percent wanted to enter some form of group practice.

Many of the physicians who served in the armed forces during the war are forming group clinics for civilian practice. These men worked in close cooperation with their colleagues in service hospitals and units; they had daily opportunities to appraise the expediency of these methods, and have retained the desire to incorporate the best features into their own practice. It seems unlikely that the impetus that the war gave to cooperative medicine will come to nothing. The concept of group practice has been growing for a long time, and not even the most fervent propaganda against it has succeeded in blocking it. Opposition has undoubtedly slowed its development, but it is comforting to realize that throughout history no entrenched group has ever permanently halted any movement that is on the side of true progress. Group medical practice has already proved itself to be a step in the right direction for the patient, for the doctor, and for nurses and technicians. It is a good, square deal for everyone concerned.

Let us assume that you are a patient going through a typical group clinic. When you arrive, you need not waste time or anxiety over your financial arrangements if they are on prepayment basis. If it is your first appearance at the clinic, you will receive a very thorough examination. First, there will be a laboratory checkup. Your blood will be tested to determine whether your red and white corpuscles add up to the correct total, so that anemia and other illnesses can be ruled out. The urine is examined for albumin, sugar, etc., to eliminate the possibility of nephritis, diabetes, and other maladies. A Wassermann test for syphilis is done routinely on all patients. If you protest this as a personal affront, you will be assured that anyone in the world, regardless of his style of life, philosophy, annual income, or tastes in entertainment, can contract syphilis. There are even cases on record with no venereal contact at all. The failure of the general practitioner to perform Wassermann tests routinely on every patient is a strong indictment against him, and has proved to be the source of completely avoidable but nonetheless tragic illness. In group medical practice, the Wassermann test becomes so routine a part of the general examination that you won't give it a second thought.

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* *You and Your Doctor* is scheduled for publication April 23. This extract is printed by special permission of the McGraw-Hill Book Company.

You are next sent into the X-ray or fluoroscopy room, where your chest is X-rayed or fluoroscoped. This takes only a few minutes, and with the new techniques you don't even have to undress. This examination can reveal the presence of tuberculosis in the lungs and also provides valuable information about the heart and other structures inside the chest. Judging from the number of cases discovered in recent surveys, there are over half a million people in the United States with unsuspected active tuberculosis of the lung. Undoubtedly, many of these individuals have consulted their physicians countless times, but the tuberculous infection was not detected because, in the early stages, it can be found only by X-ray or fluoroscopy techniques. In a group clinic where such techniques are part of the routine examination, many cases of incipient tuberculosis have been discovered at a time when they can be treated and cured. This is preventive medicine that actually saves lives.

You are now ready for the examining room. Here a young physician will take a complete medical history, which includes details of the present and all past illnesses, and he will examine you carefully. An older, senior physician is then called in. In a group clinic connected with a medical school, a fourth-year student makes the initial examination, and the older physician is generally an instructor or professor in the medical school. In either case, the more experienced physician will sit down with the student, intern, or young physician, and listen to an account of the illness and the assistant's findings. He will ask him to analyze the case and suggest a diagnosis, and together they will decide what further tests are required to verify the diagnosis. This constitutes important training for the younger doctor, since all his decisions are checked by an older and more experienced man.

The young physician or student who examined you now returns with the senior doctor, who checks the findings, and perhaps clarifies some aspects of the illness that the younger doctor may not have grasped. This concludes the teaching process. The senior physician then explains the findings to you, recommends any additional tests or X-rays that may be necessary, and tells you what treatment should be started. Before you leave, arrangements are made for a return visit, usually within a week or two.

In this manner, a thorough and careful examination is carried out with considerable saving of time to the senior physician. Because he has confidence in most of the routine preliminary checkup by the younger doctor, he can focus his attention on the abnormal findings. If your examination reveals high blood pressure, the doctor will be particularly painstaking when he checks the heart, the kidneys, and other organs that are apt to be affected by high blood pressure. If you have bronchial asthma, he can concentrate on a very thorough examination of your lungs and heart. He has not had to spend time on details that a less experienced doctor or student can be responsible for. Simultaneously, the younger physician has been given an opportunity to practice medicine and improve his knowledge. And you as the patient have had the benefit of a double check by two doctors.

Before your return session, the senior physician studies your laboratory reports and the notes on any X-ray examination. By the end of your second visit, he will have gained a reasonably accurate picture of the illness and the treatment required. If you need specialized treatment that he is not qualified to provide, he will refer you to the correct specialist in the clinic. But he will continue to check on your progress, and to receive your reports. He is your doctor, and he is the man you will always see for any new illness.

A SMOOTHLY functioning group offers obvious advantages for everyone connected with it. New patients in a group have frequently been heard to comment that this was the first "real examination" they ever had, the first time the doctor had sat down and taken the time to find out everything that might be wrong. The patient is not always aware that this method is also beneficial to the doctor, who, thanks to the students and assistants who completed the preliminary examination, has been left free to be painstaking in dealing with each new patient. Furthermore, the student or younger physician has been given the inestimable advantage of making independent examinations and diagnoses under the supervision of older, more expert physicians. This is a very important contribution of good group practice. The young physician who goes into practice after a one-year internship remains isolated, and seldom has anyone to consult when he is uncertain about a case. He accepts a tremendous responsibility for which he is not yet prepared. In the group, the younger doctor is under the supervision of a doctor with infinitely greater experience. The younger physician is trained and given responsibility at a graduated rate until he can be independent, and is never kept in the prolonged state of dependence that is so frequently the fate of the assistant to the private practitioner. Meanwhile, the senior physician, knowing that his routine work is in good hands, can allocate more hours to his hospital patients, to teaching, and to research.

In a group, specialists are accustomed to working closely with one another and with the diagnosticians, and find no obstacles to obtaining information in other fields. Such interchange of knowledge is surprisingly difficult to arrange in private practice and constitutes an unfortunate limitation for both the doctor and the patient. I saw a patient some time ago who illustrated this very dramatically. She came to our clinic with a perfectly obvious case of high blood pressure. The diagnosis was readily established and treatment started. During the preliminary examination, the younger doctor noticed a small ulcer, about a half inch in diameter, on the palm of her hand. When she was asked about it, she answered casually that she had had it for at least a year and that all the doctors who had seen it had assured her that it was a mild type of ulcer. It had never been treated, although it had opened and closed repeatedly, and she had become quite accustomed to the ugly moist area on her hand. Since we were not convinced that it was a harmless sore, we urged the patient to have it examined by a dermatologist. She was very reluctant and insisted that she had grown used to it, that it didn't bother her, and that she did not want to spend any money on it. I interpreted her last reason as the primary one, and assured her that it wouldn't cost anything to have Dr. X, our skin specialist, look at the sore. I walked down the corridor with her, and asked Dr. X to examine her hand. His experienced eye immediately observed the signs of a possible mild form of skin cancer. When cancer was mentioned, the patient's hesitation vanished. Dr. X removed a bit of skin and examined it under the microscope, confirming the diagnosis of cancer of a mild degree of malignancy. X-ray therapy was instituted, and the ugly sore disappeared rapidly. The patient was delighted to have a normal hand, and all of us learned a lesson from the case. Everything was easily arranged and proved inexpensive for the patient because of the organization of this group medical center.

A physician in private practice would also, undoubtedly, have noticed the lesion. But getting something done about it would have been considerably more difficult for him. The patient could much more readily have persuaded him not to do anything about it because "she had become accustomed to it, and it didn't bother her." He would hesitate to antagonize her by insisting that she consult a skin specialist. In addition to the high fee, there would be the problem of the time and distance involved in getting to the specialist. And, even if the specialist were located in the same building, the practitioner would be conscious of a certain hesitation, derived from our system of private practice and private fees, of not wanting to sell the patient more than she came to buy. This patient might well have continued to take her lesion home with her and to have tolerated it until it really began to bother her. By then it might have been too late. A doctors' building, no matter how large or how modern, is still nothing more than the headquarters for an assortment of doctors. It cannot generate the type of informality and cooperation which exists in group practice, among physicians who work together day after day.

It is sometimes said that a group may operate more efficiently than the general practitioner, but cannot provide a good, intimate patient-doctor relationship. I can refute this argument from personal experience, and the experience of many colleagues in group practice. A smoothly functioning group offers a better rapport between the doctor and the patient. A double emotional relationship is established toward the patient's doctor and also with the group as a whole. I consider this double attachment very important. The patient comes to rely on his own physician and on the skills of the associated specialists. Confidence is inspired by the careful and thorough examination. He knows that if he is ill, the illness will be correctly diagnosed and treated, and that if he is told he is in good condition, there is every likelihood that this is true. The patient does not have a vaguely dissatisfied feeling because the doctor was too busy to hear all his symptoms, or because the examination was not thorough. He knows he has access to any specialist he needs without great expense. He can go to the group for treatment of relatively mild illness. Financial considerations will not force him to wait until obviously serious symptoms are manifested. Even through the dry dullness of a statistical chart, we can read the tragedy of the many who die or are permanently disabled every year because they did not receive medical care soon enough. Nor can we minimize the fact that the reason most of them did not obtain medical care was that they could not pay for it, or had to travel too far to get it.

A family attending a group clinic over a period of years becomes attached to the group as a whole. Their babies have been delivered by its obstetricians; they have consulted its surgeons and X-ray technicians; they have brought their children to its pediatricians; they have attended a variety of clinics over a period of years.

In a group, the physicians rarely discuss money with the patients. Instead, the fees are determined by experts in medical economics. To me, this is one of the finest elements in group medicine, and represents a genuine improvement in the physician-patient relationship. I agree with Dr. Sigerist that it is high time "the shopkeeper element" was removed from medical practice.

Another important consideration in establishing group clinics is their effective utilization of modern medical equipment. Rarely can the general practitioner afford the variety of expensive apparatus required for accurate diagnosis and treatment. Today, one doctor may own an expensive fluoroscope; another has his own electrocardiograph machine; another supports his own laboratory. These doctors do not share their patients with one another, and they are no more likely to share their equipment. The patient whose family doctor boasts a fine modern electrocardiograph machine is lucky if he needs an electrocardiogram, but if his condition calls for fluoroscopy, he is not very fortunate. In the group clinic the essential equipment is on the premises, and its cost is shared by the unit as a whole. An active group can support good X-ray equipment, a complete laboratory, and even good physiotherapy equipment.

The ideal group works in close proximity to the hospital to which it refers its patients, and they can be attended there by the physicians who made the diagnoses. As anyone who has ever been hospitalized knows, this is a great comfort to the patient and spares him a good deal of emotional disturbance. It is also a good arrangement for the doctor because of the obvious educational advantage in following an illness from its initial diagnosis, through its entire course, to the final convalescence.

THE opportunities for the doctor to continue his education under group practice are not to be overlooked. Almost every doctor plans to attend conventions and conferences and to take an occasional refresher course, in order to keep abreast of medical developments. But by the time a man has built a large and devoted general practice, he finds himself anchored to it. He cannot go away for a month or, often, even for a week. His patients will resent it. Often there is no one who can take care of his practice while he is away. Young doctors, who especially need additional training, find it even more difficult to leave the practice they are establishing. An operation is scheduled, if possible, at a season of the year when conditions will be most favorable. A maternity case cannot be suspended to enable a doctor to go to a scientific meeting. The average physician in a small town finds himself completely cut off from medical progress, except for what information he can obtain from reading in his very limited amount of free time. Yet, refresher courses for the doctor are not a means for self-indulgence, but are vitally necessary to the care of his patients.

In group practice such efforts are perfectly feasible and are encouraged. The doctor is not inextricably tied down to his routine. His patients have faith in the group as well as in him. They know that the doctors work together, that they discuss diagnosis and treatment with one another, that the group attempts to maintain a uniformly good staff. If the doctor leaves the clinic for a brief period for a scientific meeting or a refresher course, they know that he will return an abler physician, and that the dividends of his period of study will accrue to them. The records of a group are complete and carefully kept, so that if Dr. A arranges with Dr. B to take care of his patients for a while, the transition can be made easily and the patient will experience no anxiety. The patient may already know Dr. B from occasional contacts in the clinic. When a private practitioner leaves his practice for any reason, he creates a void. In a small community, the patients frequently have no other doctor to go to. When a member of a group takes time off, the group continues to function, and the patients continue to receive good care.

Sufficient leisure to follow the progress of medicine is by now taken for granted in all groups attached to large hospital centers and medical schools. But this principle can and should apply to every group. Even the smallest group unit in the most isolated community can arrange for occasional leaves of absence for members of its staff without materially lowering the quality of its service. In the sparsely settled and remote counties of the nation, the small units consisting of six to ten doctors could easily serve the average medical needs of one or more counties by means of improved transportation arrangements. The educational activity of the group could be predicated on some plan of rotation. The experienced physicians would always have younger doctors in training who would assume responsibility whenever their absence was necessary. This would be equally possible for other members of the staff whose specialties coincided at certain points. For example, although there would probably be only one general surgeon in a small group, he could, for brief periods, leave his patients in the care of the group's obstetrician and gynecologist who also has had a good training in general surgery. This interlocking of skills in even the smallest group clinics frees the doctor and permits him to keep abreast of advances in his profession. It represents an incalculable improvement over the lot of the general practitioner who is continuously harnessed to his practice. Well-organized and well-located groups in rural areas are not merely desirable; they are indispensable unless we are ready to admit that we are totally indifferent to the health of our huge rural population.

We are a sufficiently rich and ingenious nation to work out a satisfactory method for the payment of doctors, groups, and hospitals, once the advantages of this type of practice are generally recognized. Groups already in existence employ various methods of payment. Some, like the Ross-Loos Clinic in Los Angeles, the Chicago Civic Medical Center, the Group Health Association of Washington, D.C., and the Farmers' Union Cooperative Association, of Elk City, Oklahoma, operate on a prepayment basis. The Mayo Clinic, the Cleveland Clinic, the University of Chicago Clinics, and others charge for each clinic visit, but their fees are adjusted to the ability of the patient to pay.

The average citizen is not aware that several of the group clinics now in operation are providing some of the best medical service in the United States; their medical staffs include a number of the most distinguished physicians in the profession; their patients prefer this type of medical care to any other. Fortunately, despite opposition from the American Medical Association, these groups have grown and have developed into some of the best medical centers in the world. Not only have they raised the standards of medical service generally, but they have also, in some instances, conducted important research projects. They show the efficacy of group practice.

Certain groups are so successful and so completely accepted that they are now unassailable; hence the attack is directed against smaller groups and the concept of prepayment. The dread adjective "socialized" has been transferred from institutions like the Mayo or Lahey clinics to the smaller medical groups and the prepayment plan. We are now supposed to be impressed by the sanctity and holiness of the private fee. It is "socialization" to put the doctor on a salary. Yet the doctors at the Mayo Clinic, the University of Chicago Clinics, and the Cleveland Clinic have worked on a salaried basis for years. I am sure that a referendum among doctors and medical students would indicate that they prefer a fair salary to the first ten or fifteen years of economic struggle in private practice, so that they could practice medicine instead of bookkeeping. The sanctity of the individual physician's fee may be an obsession with some of the more reactionary representatives of the medical profession, but it definitely does not represent the most pressing problem for either the medical profession or for the nation. The problem is to provide the nation with adequate medical service, and the medical profession with an efficient and logical method of practice.

SEND THE SM TO VETERANS' HOSPITALS

Having reason to believe that patients in Veterans' hospitals want to read scientific as well as popular magazines, the Engineers' Society of Milwaukee has appealed for contributions of technical journals to the local hospital. As the response was gratifying, Samuel R. Snead, of that Society, suggests that the appeal be made on a national basis. Specifically, we suggest that unneeded copies of the SM be sent to the nearest Veterans' hospital. The SM has the desired meat in it, without being terrifyingly technical.—Ed.

THE FAITH OF SCIENCE

ARTHUR WARD LINDSEY

Head of the Department of Biological Sciences at Denison University, Dr. Lindsey (Ph.D., Iowa, 1919) has spent most of his academic life at Granville. In the article below he succeeds so well in his attempt to state concisely the relation of faith to intelligence and the place of faith in scientific thought that the editors felt obliged to reconsider their decision to publish no more articles on this subject.

WE MAY hope that the day of violent controversies between the proponents of the religious point of view and those of the sciences has gone forever, but there is still a yawning gulf between them. The religious man is concerned with the mysteries of life and the hereafter for which he has no direct evidence of the senses. The scientist requires that evidence as a necessary foundation of belief. Religion, therefore, is free to accept on a basis of faith the concepts that are most comforting, whereas science demands the suppression of individual feeling in the relentless search for truth.

Scientists themselves, since they are human, have the same capacity for faith and feeling and belief that other men may have. They may reach sound conclusions without following the stony path of detailed research and may feel strong convictions that are not amenable to scientific investigation, but no scientist can be content to rest his thoughts on such foundations. Knowing the fallibility of human observation, he must test his hypotheses by reference to the natural world. He must safeguard his observations and experiments by the most exacting checks and must anticipate all alternative interpretations as fully as possible before he can be convinced that what he believes is not merely a product of his own mind, but also an interpretation of the actual phenomena of nature.

This procedure is rewarding to intellectual men, but it is also costly. It is far easier for human beings to yield to the emotional processes of their minds than to subject their behavior always to rational analysis, for emotional behavior is far more deeply rooted in the remote animal ancestry of man than the elaborate intelligence that he alone possesses. The use of his more recently acquired powers of thought is exhausting. If he belongs to the peculiar breed of scientists he cannot be contented with any less than the best that his mind can do, and that ideal is likely to color his life or, better, to rob it of color, even where his emotions should rule.

As a result, scientists are liable to criticism as a cold and heartless lot. No doubt they deserve the indictment in part of their lives. Possibly some of them are too completely scientific in all their living, and possibly all of them are too precise at times in the exactions of their thinking. If that is their loss, they have a reward to compensate it in the beauty of precision that satisfies their type of mind.

That there is beauty in precision of thought may not be an appealing idea to many men. Certainly it cannot appeal to those who do not have the capacity or are unwilling to drive their minds to the last degree of accuracy that may be the boundary between right and wrong. But once a man has known the satisfaction of following a problem through to the final correction of his own misconceptions and to final proof through the rigid methods of the laboratory, he can never again be satisfied with loose thinking. He has found the reward that President Wishart of the College of Wooster once characterized as the austere beauty of precise thinking.

It is a beautiful phrase, and apt. Austerity must be the mantle of scientific procedure and the cross of the scientist when his habit of mind carries over into contacts with other men. He can only echo Luther's cry: *Ich kann nicht anders.* His reward must come from the satisfaction that he knows so well in his work, even though it may deprive him of some of the warmth of human life.

Science does not claim absolute understanding. Too much has been discovered within the twentieth century to permit any scientist to feel that what he knows today is all that can be known tomorrow, or that what seems accurate now may not be completely overthrown by the discoveries of another year. His own experience may very well have included the reversal of conclusions that he once held dear, and if he has lived a reasonable span of life the things that he was once taught have certainly been subject to revision. The atom forty years ago was the indivisible unit of matter.

The keen appreciation of the fallibility and incompleteness of human knowledge that must result from such understanding can have only one effect. We seek ultimate comprehension of ourselves and of the universe in which we live, and from the dawn of human intelligence have been formulating explanations according to the stage of our experience. Our one source of information has been our special senses. What we have done with that information in our own minds has varied widely, but it has always included concepts of the supernatural beyond reach of the senses, and these concepts have been potent in shaping and directing the course of human affairs. Scientists, like other men, have fallen back upon such methods of thought when they have reached the limits of scientific procedure, but with their scientific habit of mind they have often not required abstract concepts where other men might require them. They fully know that the incompleteness of knowledge has been lessened by degrees. If today they face an unknown, it can be faced in agnosticism with no craving for an explanation when accurate explanation is obviously beyond reach of our present knowledge.

WHILE their agnosticism touches no cherished creeds it cannot offend. They can disagree with the tenets of their friends in economics and philosophy and education at the risk of nothing more than scorn. Among themselves they may disagree heartily on these subjects and out of exasperation they sometimes express their feelings more openly, where they can be argued down by the opposition. But as a rule they keep such thoughts to themselves, knowing too well the futility of argument where facts are not available. The opposition is in the majority. It rises easily to defend its own position, secure in the knowledge that the poor scientist is handicapped by the clipped wings of his imagination. So the scientist cannot arouse deep feeling here.

But the matter of religion is very different. It is the crutch of the weak, the bulwark of the strong, and the self-declared salvation of our species. He who questions it is indicted, tried, and condemned for heresy in the minds of the faithful, for he strikes at the roots of their being. Yet scientists are human. They, like other men, face the awe-inspiring mystery of the universe and as thinking men must have some concept to adjust their own existence to the great unknown. What can they believe?

No part of science has brought this conflict so sharply into the open as organic evolution, which has flared into violent controversy even in the twentieth century. Since it seems to the literal-minded a direct contradiction of cherished biblical teaching, it has been bitterly opposed at times, so bitterly indeed as to bring into question the evolutionist's fitness to present his views to the young. Why should not such a field offer good testimony to the attitude of science on the ultimate power of the universe? We find this testimony in the beautiful passage from Erasmus Darwin's *Zoonomia,* antedating the work of his famous grandson, Charles, which caused the most serious discussions:

> The world has been evolved, not created; it has arisen little by little from a small beginning and has increased through the activity of the elemental forces embodied in itself, and so has rather grown than suddenly come into being at an almighty word. What a sublime idea of the infinite might of the great Architect! the Cause of all causes, the Father of all fathers, the *Ens entium!* For if we could compare the Infinite, it would surely require a greater Infinite to cause the causes of effects than to produce the effects themselves.

> **All that happens in the world depends on the forces that prevail in it, and results according to law; but where these forces and their substratum, matter, came from, we know not, and here we have room for faith.**

When the knowledge of the astronomer reduces the heavens to ascensions and light-years and astrophysics, when the atom yields its secrets so fully that man can produce elements that do not exist in nature, when the geneticist traces the wonders of heredity to microscopic bands in the chromosomes, can the basis of faith be less? The simple world of our ancestors was wonderful enough to demand some explanation, but the world of modern science is infinitely more wonderful as a tribute to the Power that lies behind it. There is still room for faith, and if faith without knowledge is beautiful, then surely faith with knowledge is transcendent.

But in what shall the scientist have faith? Shall it be Michelangelo's God of the Sistine Chapel, or the great Byzantine Christ of Monreale? Shall it be a Lord willing to suspend the laws of nature to meet a despairing cry for personal advantage? Shall it be the promise of personal immortality, of heaven and hell? Shall it be the sinfulness of man and the need for expiation before a stern but forgiving Judge? Or shall it be faith in a nebulous Unknown, secure in the conviction that whatever that Unknown may be, he has given man the power to see for himself and to work out his own destiny through the courage and intelligence of his own being?

That intelligence has already transformed him from a solitary nomad, exposed to the elements and taking his food like any animal wherever he could find it, into a civilized being. It has given him control over his environment that would have seemed a thousand years ago like the power of a god. It has lengthened his span of life almost to the threescore years and ten of the Scriptures. Why should he question or try to estimate its capacity to extend his knowledge and control to unpredictable ends?

He must be concerned now as never before with the use of his knowledge for the destruction of his kind, but this danger only adds to his obligation to solve the problem of living in brotherhood. Here the tangible expression of the Christian religion gives him a system of ethics which, properly applied to human behavior, would solve all problems. Biology supports that system and brings science into close accord with religion in the affairs of men, not on an emotional basis but through purely intelligent understanding. Here the scientist is a supporter of the most practical aspects of Christianity. Can he go further?

For himself it is a personal problem. Men believe what they can believe, accept what they must accept of Christian principles for their own peace of mind. For others the scientist's attitude can be tolerant. He knows the variability of men. He knows that the deeply rooted emotions are present in all men, whatever their intelligence quotients, and that far more men can therefore be reached by emotional appeal than by pure reason. Since, in his own life, serenity can be attained only through that residuum of faith which lies beyond the limits of comprehension, he must not deny to others a similar right; and if their need is greater, even to the extremes of emotional worship, it is not his privilege to condemn. He can have faith in their religion for its value to them even if he cannot share their faith for its value to himself.

Few scientists are militant iconoclasts. That is a tribute to their intelligence. The few who are meet disapproval from their fellows scarcely less than from the proponents of religion. Since this is true, it seems that tolerant scientists should enjoy equivalent tolerance for their views from men of the Church, but this again is a matter of intelligence. Throughout the more emotional sects, both laity and clergy have been scornful of the scientific attitude to religion. They may condemn the agnostic even more bitterly than the infidel and may publicly denounce his beliefs. But the scientist also has abundant reason to appreciate the fine tolerance of many religious men for his own views and so to realize that the varied balance is only another expression of the variability of men.

Faith is for all men the recourse of the mind when it reaches the limits of true comprehension. It is bound to vary as the capacity and habits of the mind vary, and so for the inquiring mind the domain of faith must be far more restricted than for the mind of a child. But its role is always the same. For every man it is the normal complement of reason.

If, for the scientist, the extension of his knowledge of natural forces into realms not even imagined a century ago brings a contraction of the realm of faith, there still remains a great Unknown that his intelligence cannot encompass. If his solution of mysteries once insoluble gives him confidence in the capacity of the human mind to solve the mysteries of the present, the realization can only add to the wonder of that Unknown. His faith must be greater, rather than less. His demands for a symbolic expression may be small or nonexistent, but faith must persist: faith in the majesty of the Unknown, faith in the manifestation of that Unknown in his own human powers, faith in the nobility of man's struggle toward perfection in spite of the discouragement of failures along the way.

Science must have faith. It cannot share the comfort of faith in a loving God who will ease the burdens of life on earth for the faithful and receive them into a gilded afterlife, for it knows too well the inexorable laws of nature. It cannot share a God created by man in the human image, for it knows too well the frailties of men. But it must have faith in the Infinite Power that lies behind the magnificent attainments of mankind and in the capacity of men to push on to greater heights.

The faith of science must be austere. It is a faith for the strong, a comprehending faith with sympathy for the beliefs of all men according to their need, a faith that demands of self before God, a faith of gratitude for what we are and of the obligation of man that has been put so well in the lines

> Work. Feed thyself. To thine own powers appeal
> Nor whine out woes thine own right hand can heal.

But it is still faith, the sustaining force that extends beyond intelligence to keep all men in harmony with the great Unknown. And in science it can still be beautiful with the austere beauty of precise thinking and noble with the dignity of self-reliance and gratitude.

GRAY EDGE

More than for building of a tower
Or lifting of a spire,
More than for stone on stone to house
The dust his hands desire,
Man wrestles with the arc of sky,
Horizon rim to rim,
Curving it for a smaller world
About the soul of him.
To circumscribe his going out
And shrink the nought of space,
Marking the center of the whole
Within his little place.
Yet all the while and without end
And none to make reply,
He paces on the gaunt, gray edge—
Alone against all sky.

ISABELLE BRYANS LONGFELLOW

THE UPPER LIMIT OF ABILITY AMONG AMERICAN NEGROES

MARTIN D. JENKINS

Professor Jenkins (Ph.D., Northwestern, 1935) has taught at Virginia State College, North Carolina Agricultural and Technological College, and Cheyney Training School for Teachers; since 1938 he has been teaching at Howard University. A specialist in educational psychology, Dr. Jenkins became interested in gifted children while studying for his doctorate.

MORE than three decades of psychometric investigation among American Negroes has yielded a rich fund of information concerning this population group. Perhaps the most generally known finding, and certainly the most emphasized, is that when "comparable" groups of whites and Negroes are tested, the Negro group is almost invariably inferior to the white in psychometric intelligence (intelligence as measured by psychological tests). Preoccupation with the significance of the low *average* performance of Negro groups has served to divert attention from an equally important phenomenon—the variability of the group, and especially the upper limit reached by its really superior members.

The question of the upper limit of ability among Negroes has both theoretical and practical significance. Psychologists generally attribute the low average performance of Negro groups on intelligence tests to cultural factors. It is well known that Negroes generally experience an inferior environment; and there is certainly no question but that an inferior environment tends to depress the psychometric intelligence. There are, however, many Negro children who are nurtured in an environment that is equal or superior to that of the average white child. Thus, we may hypothesize that *if race in itself is not a limiting factor in intelligence, then, among Negroes whose total environment compares favorably with that of the average American white, there should be found a "normal" proportion of very superior cases, and the upper limit of ability should coincide with that of the white population.* This hypothesis is especially attractive from a negative aspect; thus, if very superior individuals are not to be found in the Negro population, the environmental explanation would clearly be inadequate to account for the phenomenon. The existence of such individuals, on the other hand, would afford additional evidence, but not absolute proof, of course, of the validity of the environmental explanation of "racial differences" in psychometric intelligence.

The practical significance of the question is apparent. If Negroes are to be found at the highest levels of psychometric intelligence, then we may anticipate that members of this racial group have the ability to participate in the culture at the highest level. In these days of reconsideration of the role of the dark races throughout the world, this question has more than mere national significance.

Analysis of the literature relating to the intelligence-test performance of Negro children reveals that a considerable number of these children have been found within the range that reaches the best 1 percent of white children (I.Q. 130 and above) and at the level of "gifted" children (I.Q. 140 and above). There are at least sixteen published studies that give an account of Negro children possessing I.Q.s above 130; twelve of these report cases above I.Q. 140. These investigations were made by different psychologists in various localities and under varying conditions; moreover, the I.Q.s were derived by a number of different tests. Further, the populations studied were located almost exclusively in Northern urban communities. Consequently, one may not justifiably generalize, from a composite of these studies, concerning the incidence of Negro deviates.

It is of significance, however, that of the 22,301 subjects included in the thirteen studies for which N's are reported, 0.3 percent scored at I.Q. 140 and above, and fully 1 percent scored at I.Q. 130 and above. These percentages are similar to those obtained from a "normal" I.Q. distribution of American school children.

Of especial significance are the cases of very bright children of Binet I.Q. 160 and above. It may be estimated that fewer than 0.1 percent of school children are to be found at or above this level. As the I.Q. rises above 160 the frequency of occurrence, of course, decreases. Statistically, cases at or above I.Q. 180 should occur about once in a million times, although they actually occur with somewhat greater frequency. In his classic California study of the gifted, Terman found only 15 children testing as high as I.Q. 180; and Hollingworth reports: "In twenty-three years seeking in New York City and the local metropolitan area I have found only twelve children who test at or above 180 I.Q. (S-B)." It is apparent then, that children who test upwards of Binet I.Q. 160 are extreme deviates in psychometric intelligence and representative of the very brightest children in America.

I have assembled from various scores the case records of 18 Negro children who test above I.Q. 160 on the Stanford-Binet examination. Seven of these cases test above I.Q. 170, 4 above I.Q. 180, and 1 at I.Q. 200. Two of these cases were tested initially by me; the other 16 were reported by psychologists in university centers and public school systems. Analysis of the case records indicates that these children during the early years of their development, at least, manifest the same characteristics as do other very high I.Q. children: originality of expression, creative ability, and surpassing performance in school subjects. Some of these children, but not all, are greatly accelerated in school progress. Two, for example, had completed their high-school course and were regularly enrolled university students at age thirteen; both of these subjects were elected to Phi Beta Kappa and earned the baccalaureate degree at age sixteen.

It is of some significance that all these children were found in Northern or border state cities (New York, Chicago, Washington, and Cincinnati). No Southern Negro child, so far as I have been able to ascertain, has been identified as testing at or above Binet I.Q. 160. It is certain that among the 80 percent of the total Negro population that lives in the Southern states, children with potentiality for such development exist. Whether the fact that no children with this development have been discovered is due to lack of environmental opportunity and stimulation, or merely to lack of identification, is not surely known.

I am not attempting here to show that approximately as many Negro children as white are to be found at the higher levels of psychometric intelligence. There appears little doubt that the number of very bright Negro children is relatively smaller than the number of bright white children in the total American population. Nevertheless, it is apparent that children of very superior psychometric intelligence may be found in many Negro populations, and that the upper limit of the range attained by the extreme deviates is higher than is generally believed.

The performance of extreme deviates at the college and adult levels has not yet been extensively studied. Such evidence as is available, however, indicates that at maturity, as in childhood, some Negroes are to be found at the highest level of psychometric intelligence. In a recent unpublished study conducted at Howard University, it was found that of approximately 3,500 Negro freshmen entering the College of Liberal Arts over a period of seven years, 101 scored in the upper decile, and 8 in the upper centile (national norms) on the American Council on Education Psychological Examination. In a more extensive study, the National Survey of Higher Education of Negroes, there were, among 3,684 students in twenty-seven Negro institutions of higher education located chiefly in the Southern states, 23 cases in the upper decile and 4 in the upper centile on the A.C.E. Psychological Examination. It is of some significance that in the same study 12 upper decile cases are reported among the 105

Negro students in two Northern universities (almost half as many as were found altogether among the 3,684 students in the twenty-seven Negro colleges). This contrast is in accord with the general but undocumented opinion that among Negro college students there are proportionately fewer extreme deviates in psychometric intelligence in the Southern segregated colleges than in the Northern nonsegregated institutions.

The Army General Classification Test data assembled during World War II have not yet become fully available. One may predict with a fair degree of confidence, however, that these data will reveal some Negro cases at the very highest levels of performance. In view of the fact, however, that the Negro selectees were predominantly from communities that provide inadequate provision for the educational and cultural development of Negroes, we may expect that a very small proportion of the total population will be found at the higher levels of performance. Subgroups which have had a normal cultural opportunity should, in accordance with our hypothesis, yield an appreciable proportion of superior deviates.

The findings of the studies cited in this article support the hypothesis formulated at the outset. In some population groups there is to be found a "normal" proportion of Negro subjects of very superior psychometric intelligence, and the extreme deviates reach the upper limits attained by white subjects. Although the incidence of superior cases is much lower among Negroes than whites, a phenomenon which might well be accounted for by differential environmental factors, we may conclude that race per se (at least as it is represented in the American Negro) is not a limiting factor in psychometric intelligence.

The abstract mental tests that contribute to psychometric intelligence do not measure the factors of personality and motivation that largely determine success in life. The findings of studies of gifted children, especially those of Terman, Hollingworth, and Witty, indicate that the highly gifted child usually fulfils his early promise. But not always. Failure among the gifted is also frequent.

The data of this article bring into sharp focus the limitations that our society places on the development of the highly gifted Negro. These superior deviates are nurtured in a culture in which racial inferiority of the Negro is a basic assumption. Consequently, they will typically experience throughout their lives educational, social, and occupational restrictions that must inevitably affect motivation and achievement. The unanswered question relative to the influence of this factor on the adult achievement of superior Negroes is a problem for future investigators to solve.

DE SPIRITI, OR HISTORY IS RIGHT

JOSEPH HIRSH

Recently appointed acting director of The Research Council on Problems of Alcohol, Mr. Hirsh was given a leave of absence from that post to become director of public information for the World Health Organization of the UN; in that capacity he went to Geneva last January to participate in the Fifth Session of its Interim Commission. The article below will form part of a chapter in his book on the problem drinker, to be published this year by Duell, Sloan and Pearce.

MEN have been judged by the company they keep, by the books they read, by the clothes they wear, even—and often—by the women they marry; and if the current rash of writings—the books, the scholarly and popular articles—the radio talks, the speeches and conferences, are any index, by the liquor they drink. They are judged not only *if* they drink but by what they drink.

The abstainers hold, as they have always held, with the Old Testament's judgment that "There is death in the pot," that "A drunkard in the gutter is just where he ought to be," and that all alcohol—except possibly methyl alcohol—is "the devil's brew." Against a background of stately mansions, teardrop limousines, and tailored executives, the distillers pose their bottled brands and make their judgments in terms of "Those Who Know" and "Men of Distinction." And, finally, there are the users themselves, who generally hold that all alcohol (and an injudicious few include the methyl variety), in the words of Alexander Pope, induces "the feast of reason and the flow of soul."

In the center of these humorless judgments and counterjudgments, and of the inevitable controversy concerning prohibition, stands a small group of serious souls, the scientists (hereinafter referred to as The Triple S's; in no way to be confused with the American Association for the Advancement of Science, or The Triple A-S), who in their own inimitable way are also judging their fellowmen and their fellow-drinkers. But theirs is judgment based on scientific inquiry.

The simplicity of their questions belies the complexity of their task. Why, they ask, do men drink? Why do some—not all—become problem drinkers? How can we treat them? How can we prevent alcoholism? What is alcoholism anyway? Is there more than one type? And so on. Their task would be so much easier if they but lived by Cicero's counsel in *De Oratore* (II)—"Not to know what happened before one was born is always to be a child"—if they but turned to a study of history.

Although the great preoccupations of history are to determine "which came earlier" and "who did it first," in this *very* short history of drinking we are less concerned with these facets and more with the facts of problem drinking. The history of drinking is important less because of its medical, social, and religious prohibitions—usually directed at excesses only—than because from the beginning of the record attempts have been made to understand the nature and process of these excesses.

De spiriti, history may not have many of the answers, but with tongue in cheek it has much to teach. Take, for example the question "Why do men drink?" While our psychiatrists probe deep into the psyche, into the id, the ego, and the superego (naturally, I mean the Freudian psychiatrists) and The Triple S's search for "trigger mechanisms," a characteristically alcoholic cellular metabolism, or something awry with our enzymes, Thomas Love Peacock neatly packaged the answer 130 years ago in *Melincourt* (XVI). "There are," he said, "two reasons for drinking: one is when you are thirsty, to cure it; the other, when you are not thirsty, to prevent it."

The Bible records literally scores of references to drinking and excesses, serving as the touchstone for many judgments, critical and valued, that have achieved great currency. One of the earliest references is contained in the story of the amphibious Operation Noah. Upon alighting from the Ark, we are told, Noah immediately planted a vineyard, made wine, and got drunk. After 377 days and nights aboard ship he doubtless had sufficient reason for attempting to relieve what must have been one headache—by acquiring another.

Escapism, a popular modern explanation for drinking, was likewise reported in this same source book, some 350 years B.C. by an unknown scientist in Proverbs (XXXI, 7), who said: "Let him drink, and forget his poverty, and remember his misery no more." Although psychiatry as a medical science and art was not known until recent times, the ancient Roman dictum *in vino veritas* reveals a knowledge of the subconscious. The Romans saw the lid of the mind lifted by alcohol and the hidden things—thwarted ambitions, fears and frustrations, and the true self—laid bare.

History records as many fictions as facts about alcohol. Prometheus, beset by a vulture that preyed on his liver as a punishment for stealing fire from heaven, supplies one of the earliest symbols of the cirrhotic liver and the beginning of the modern myth that alcohol is responsible for that pathology. This myth has been perpetuated through more than 2,000 years of medical and social writings up to our own time, when the critical Triple S's have tackled the problem without hesitation.

Incredible amounts of time are spent pondering the true nature of the alcoholic in an attempt to classify him. But the guides are there for anyone to see. In 1592 Thomas Nash wrote in *Pierce Penilesse*:

> Nor have we one or two kinds of drunkards only, but eight kinds. The first is ape drunk, and he leaps, and sings, and hollows, and danceth for the heavens: the second is lion drunk, and he flings the pots about the house, calls his hostess whore, breaks the glass windows with his dagger, and is apt to quarrel with any man that speaks to him the third is swine drunk, heavy, lumpish, and sleepy, and cries for a little more drink, and a few more clothes: the fourth is sheep drunk, wise in his own conceit, when he cannot bring forth a right word: the fifth is maudlin drunk, when a fellow will weep for kindness in the midst of his ale, and kiss you, saying; By God, Captain, I love thee, go thy ways, thou dost not think so often of me as I do of thee; I would (if it pleased God) could I not love thee so well as I do, and then he puts his finger in his eye, and cries: the sixth is martin drunk, when a man is drunk and drinks himself sober ere he stir: the seventh is goat drunk, when in his drunkeness he hath his mind but on lechery: the eighth is fox drunk, when he is crafty drunk, as many of the Dutch men be, will never bargain but when they are drunk.

Nash in his own right drew heavily upon Hebrew, Hindu, and Arab legends. The explanation of the atavism of the alcoholic and of his association with sin was popularly ascribed in the days of Nash to the planting of the first vine. The devil, it was said, sprinkled the soil with the blood of monkeys, lions, pigs, and other animals. Their characteristics, in turn, were transmitted to the juice of the grape. Anyway, that's how the story goes.

The Triple S's may think they know all there is to know of the aphrodisiac qualities of alcohol in the most modern physiologic and psychologic terms, but no more comprehensive statement has been written on the subject since Shakespeare wrote in *Macbeth* (II): "Lechery, sir, it provokes and unprovokes; it provokes the desire, but it takes away the performance: therefore much drink may be said to be an equivocator with lechery."

The Russian physiologist Pavlov is best remembered for his bell-ringing, dog-salivating, conditioned-reflex experiments. A few years after Professor Pavlov published the results of his experiments, an Englishman, Dr. John Y. Dent, adapted this principle to the treatment of alcoholism. Using the drug apomorphine, which stimulates the vomiting center of the brain, he gave his patient a full glass of whisky immediately before the drug took effect. Within a matter of seconds, the whisky, much to the surprise of the patient, was returned almost in its original form, though not to its original container. This procedure was repeated at four-hour intervals until the victim was completely conditioned and refused to accept any alcoholic beverages.

If Dent and his followers had only read their history. . . For way back in the second century, Galen, the famous Greco-Roman physician, put live eels in wine and made the alcoholic drink it. This principle was modified variously, from time to time, involving earthworms and other miniscule, wiggling forms of "pink elephants," up until the eighteenth century. From all reports, this therapy was no less revulsive nor more effective than the modern use of apomorphine or emetine.

Our own Benjamin Rush, physician-general for a time to the Continental Army, the first professor of chemistry in America, and later professor of medicine at the University of Pennsylvania, wrote on the conditioned-reflex treatment. His essay, *An Inquiry Into The Effects of Ardent Spirits Upon The Human Body* (1785), a curious admixture of good medicine, sound common sense, and classic temperance preachments, is noteworthy, on the one hand, as one of the first scientific documents defining problem drinking as a medical problem and advocating the use of conditioned-reflex treatment as one modality. He said:

> The association of the idea of ardent spirits, with a painful or disagreeable impression upon some part of the body, has sometimes cured the love of strong drink. I once tempted a negro man, who was habitually fond of ardent spirits, to drink some rum (which I placed in his way) and in which I had put a few grains of tartar emetic. The tartar sickened and puked him to such a degree, that he supposed himself to be poisoned. I was much gratified by observing he could not bear the sight nor smell of spirits for two years afterwards.

On the other hand, Dr. Rush was responsible for perpetuating a canard that has since become a classic temperance favorite. Writing of a case of a Dr. Haller, he states a "notorious drunkard [was] suddenly destroyed in consequence of the vapour discharged from his stomach by belching, accidentally taking fire by coming in contact with the flame of a candle."

Despite the conditioned-reflex treatment, both modern and classic varieties, The Triple S's are far from satisfied. They might find a lead in another rationale. Milder than Galen's eels is the 1,000-year-old prescription of one of the world's medically great, Abu Ben Mohammed Ibn Zacariya Ar-Razi, known as Rhazes. "Apply vinegar and oil of roses to the head," he said, "and camphor and water to the nose. Employ an emetic, and put the hands and feet into cold water. After a long sleep, prescribe a bath."

If this holds no promise for future clinical research, they might look to Liebault's 341-year-old recommendation in *Maison Rustique*:

> If the Heade complaine itself of too much Drinke, there may be made a Frontlet of Wild Time, Maiden Haire; Roses, or else to drinke of the shavings of Hart's horne with Fountain or River water, or if you see that your stomach be not sicke, thou mayst take of the hair of the Beaste that hath made thee ill, and drink off a good glasse of wine.

Questions of research into the causes of alcoholism, its prevention and treatment, are not the only ones troubling The Triple S's. In spite of themselves, they are periodically drawn out of their dispassionate corners into the prohibition fracas. If they but knew their history, they could provide an answer that would probably satisfy the opposing side in the words of St. John Chrysostom, who said in *Homilies* (I):

> I hear many cry when deplorable excesses happen, "Would there be no wine!" Oh, folly! Oh, madness! Is it the wine that causes this abuse? No. It is the intemperance of those who take an evil delight in it. Cry rather: "Would to God there were no drunkeness, no luxury." If you say, "Would there were no wine" because of the drunkards, then you must say, going on by degrees, "Would there were no steel," because of the murderers, "Would there were no night," because of the thieves, "Would there were no light," because of the informers, and "Would there were no women," because of adultery.

For myself, as I write *De spiriti* here in Manhattan, the big toe of New England, I am convinced that history is right for a jugful of reasons. The Bay Colonists showed good sense in at least one respect, in repealing after seven years, in 1646, one of the most unique prohibition laws in history, directed at drinking "healths." History is right, not merely because in the old days "housewarming" meant the use of "ardent spirits" to achieve that end, but also because Manhattan is said to be a corruption in the Delaware tongue, meaning "Here we got drunk." The historical hospitality of the island has continued from the time of the Henry Hudson swindle to this day.

THE LIBRARY OF CONGRESS AS THE NATIONAL LIBRARY OF SCIENCE

LUTHER H. EVANS

Dr. Evans (Ph.D., Stanford, 1927), the tenth Librarian of Congress, has taught government and political science at Stanford, N.Y.U., Dartmouth, and Princeton. He has served as official adviser to a great variety of cultural organizations, and is on the Executive Committee of UNESCO. Despite the many demands on his time, he was able to complete a comprehensive study of the American administration of the Virgin Islands, published in 1945 as The Virgin Islands from Naval Base to New Deal. *In 1946 Yale University conferred on Dr. Evans the honorary degree of Doctor of Humane Letters.*

IN A letter of September 21, 1814, to his friend Samuel Harrison Smith, Thomas Jefferson spoke of his great collection of books, which was to assume such importance in the history of the Library of Congress, and mentioned that he had spent every free afternoon, while Minister to France, "for a summer or two in examining all the bookstores, turning over every book" and "putting by . . . whatever was rare and valuable in science." His library included, he believed, all that "is chiefly valuable in science and literature, generally." It need not be explained that Jefferson meant the term "science" in a broader sense than systematized knowledge of the physical or material world. It is worth noting, however, that the breadth of his principle of selection has been perpetuated throughout the history of the Library of Congress, with the consequence that the collection of books and periodicals pertaining to the natural sciences and technology has more than kept pace with the growth of the Library's collections in other branches of learning.

A description of the Library of Congress as a closely interrelated complex of special libraries would startle no one who has had occasion to acquaint himself more than superficially with its collections and services, yet the magnitude of the special collections seems to be imperfectly understood. It is not widely known that the Library's stacks contain more than 200,000 volumes in the medical sciences, a collection surpassed in size only by those of the Army Medical Library and the Library of the New York Academy of Medicine; that roughly a million volumes of the total holdings of the Library are classified as belonging to the various disciplines of natural science and technology; that more than 25 percent of the total circulation is concentrated on those subjects. Although many librarians, to say nothing of physicians and other members of the scientific fraternity, might not include the Library of Congress in a listing of great scientific and technical libraries, it is called upon daily to satisfy requests for every possible variety of scientific information from almost every conceivable source. And although there are numerous libraries with a greater concentration of materials in particular scientific disciplines, it is unlikely that any other institution has a diversified scientific collection of comparable scope and quality.

The most important stimulus to the development of this science collection was provided by a happy coincidence of events. The establishment of the Smithsonian Institution in 1846, the appointment of Joseph Henry as first secretary of the institution, a man of vision who immediately upon assuming his office began effecting agreements for the exchange of publications with foreign societies and academies, the lack of space and fireproof storage facilities in the original building of the Smithsonian Institution for the growing Smithsonian library, all led to the transfer to the Library of Congress in 1865 of the extensive Smithsonian collection which by that date had grown to 40,000 volumes. With this body of materials as the nucleus, the Library's scientific collections have grown with almost incredible rapidity through the decades, as a result of the operations of the Copyright Act, through discriminating purchase, by exchange with institutions in other countries, by transfer from other government agencies, and by gift. Apart from the vast accumulations of monographs, treatises, textbooks, encyclopedias, and sets of journals that one would expect to find, the collections include large numbers of documents which record the investigations of the scientific agencies and the reports of civil and military officers of our own and of foreign governments, as well as captured enemy documents that are being declassified and made public. Well over 50,000 German scientific and industrial reports have been received from the Office of Technical Services of the Commerce Department, and the complete file of more than 30,000 reports on research performed under the auspices of the Office of Scientific Research and Development have been deposited in the Library.

Of the significant smaller specialized collections, that in aeronautics deserves mention. In 1930 the generosity of Harry F. Guggenheim made possible the establishment of the Daniel Guggenheim Fund for Aeronautics in the Library of Congress, which was utilized for the purchase of several famous collections of aeronautic literature—those of Gaston Tissandier, Herman Hoernes, and Victor Silberer. The donation of the collection of the National Aeronautics Association and the deposit by the Smithsonian Institution of the Langley Aeronautical Library swelled the Library's holdings in this field and increased the eminence of the collection. More recently there has been a great influx of documents from the Office of Scientific Research and Development, and the National Advisory Committee for Aeronautics. The Department of the Air Force has been most cooperative and helpful, not only in making available for the Library's collections such of its publications and documents as may be released without danger to security, but also in placing its transportation and other facilities at the disposal of members of the Library staff engaged in the acquisition of aeronautical publications, with the result that the procurement of important aeronautical materials from Brazil and several Western European countries has been greatly facilitated. At present, the Library's collection in aeronautics is second to none in quality, and its file of periodical publications in this field offers a model of completeness yet to be emulated in many of the other disciplines.

Several distinguished special collections of the Library of Congress have been of great value to the historian of science, among them the Henry Carrington Bolton Collection on the history and bibliography of chemistry, the collection of incunabula, 536 of which are classed as "scientific," and the Toner Collection, rich in early American works in science, medicine, and surgery. Finally, the manuscript collections include personal papers of Franklin, Jefferson, Bigelow, Maury, Einstein, and others.

But the scope of the Library's collections in the sciences has not been, and will not be, conducive to lethargy in the Library's officers with regard to the continuing self-imposed assignment of acquiring all important current works in the natural sciences (with the exception of medicine and agriculture) published anywhere in the world. To hope for perfect achievement of this goal would be romantic, of course, but a high degree of success is assured by a well-developed worldwide program of purchasing, by constant negotiations for improved exchange relationships with foreign institutions, and by awareness on the part of all members of the Library's staff in executive positions, including those whose training and interest are remote from the natural sciences, of the pressing importance of the acquisition program in this field. Thus, it seems neither unusual nor especially worthy of comment, to mention one example, that the Library's specialist on India should be concerned with the procurement of materials from the remoter Indian provinces dealing with public-health programs there, or that numerous officers in various branches of the Library should be pondering the problem of collecting and organizing the myriad reports, ephemeral and fragmentary in many cases, which contain the results of the current scientific research sponsored by the agencies of the Federal government in the vast programs now being conducted.

But the size and quality of the collections assume significance only where efficient techniques are employed in making available for use the information stored on the shelves. It is the artful organization of the collections and the provision of bibliographical aids in their utilization that distinguish a library from a warehouse.

For the small library, simple finding devices are adequate. As the collections grow, however, the complexity of the mechanisms necessary to seek out precisely the publications that will be most helpful to the scholar increases in corresponding ratio. When a library reaches the halfway mark toward the pansophistic goal of comprehending within its collections every publication of importance for the enlightened activity of the Congress and the Federal agencies, and every important publication which reflects the civilization of the United States or of other countries whose civilizations have influenced our own, then the finding devices—the classification schedules, catalogs, and bibliographies—must be developed to the point of almost intolerable expense, and the problem of preventing "loss" of material because of errors in subjecting it to proper catalog control becomes a serious one for the library administrator.

Fortunately, the Library of Congress foresaw early in its history that its classification system would have to permit of almost indefinite expansion to keep step with the advancement of knowledge. Also, it seems to have been understood at an early stage that the collections would become increasingly more encyclopedic as the interests of the government broadened and the demands upon the National Library grew correspondingly diverse. Perhaps this foresightedness sprang from that same faith in human progress which is so characteristic of American political-social thinking, and which has so frequently been a source of amazement to representatives of older, wearier, and more cynical cultures. At any rate, the Library of Congress today is the beneficiary of the sound thinking that went into its system of classification. Essentially, with constant correction and addition, its classification has proved perfectly adequate for the needs of the rapidly expanding collections, and it has proved its merits to the more than two hundred large American and foreign libraries which have adopted it. The fact that this classification scheme is based on a principle of universality, that it attempts to deal with the whole of human knowledge, has not worked to its detriment in respect to the various fields of specialization. The Library's collections in the special fields have been so large as to necessitate expansion of the classification schedules for many branches of learning to a degree that makes them quite adequate for most special libraries. On the other hand, the fact that the Library of Congress does not set itself up as a special library in medicine, or in mechanical engineering, or in metallurgy, or in any other single discipline has prevented it from falling into the error of organizing its materials with a too-specialized viewpoint. It approaches the sciences not merely with the attitude of the physician, let us say, whose interest in current research on the digestion of carbohydrates may be solely in terms of its importance for the treatment of diabetes; it also considers the viewpoint of the organic chemist and the biologist, and, in attempting to find the organizational pattern that reflects a broader logic, seeks also to satisfy the needs of the practitioners of the various specialties through another device, the liberal use of subject headings. And, last, the classification schedules of the Library of Congress have been developed empirically to a great extent, on the basis of the actual books to be classified, not on any theoretical basis or *a priori* reasoning. They are, consequently, hampered by very few rubrics that serve no useful purpose.

It is sometimes charged that certain of the Library's classification schedules have not kept pace with recent developments in the sciences, and there is considerable truth in this allegation. It is inevitable that classification should lag behind scientific thought. At this moment the medical profession is concerned with the physiological and other medical effects of exposure to radiation from the atomic bomb, and there is a strong likelihood that the corpus of research publication in this field will soon be very extensive. However, the librarian is still somewhat at a loss as to how these publications should be classified, and his decisions will have to await further thinking by the medical profession. But beyond this unavoidable lag, it cannot be gainsaid that the Library of Congress classification schedules in Medicine (*R*) and the auxiliary sciences require accelerated revision and addition. This is not attributable to any failure on the part of the Library administration to realize the necessity of keeping the schedules up to date. It merely reflects the lack of highly specialized manpower in recent years adequate to undertake the necessary revisions and additions. In the main, however, these shortcomings are temporary and will be remedied as rapidly as the budgetary situation permits.

Perhaps the greatest need is for an extensive revision and expansion of the schedule for Medicine. It had been hoped that this expenditure could be avoided through adoption of the medical classification recently prepared for the Army Medical Library. Indeed, the preparation of a classification system for that library, financed by a generous grant from the Rockefeller Foundation, was originally conceived in terms of a revision of the Library of Congress *R* schedule. Early in 1945, however, it became clear that the direction the work was taking would lead to a product far removed in its very structure from the *R* schedule, and misgivings and reservations were expressed at that time as to the likelihood that the Library of Congress would adopt the resultant classification (*W*) for its own purposes. Recently it became imperative that a final decision be reached on this issue and it was reluctantly decided that a change-over from the Library of Congress classification for medicine to that of the Army Medical Library would be ill-advised. Intended for a highly specialized library, the *W* schedule of the Army Medical Library draws rubrics out of their position in the Library of Congress *Q* schedule (Science) for inclusion under Medicine. Considering the encyclopedic nature of the Library of Congress collections and the diversity of interest on the part of the Library's users, it would seem to do violence to logic if this arrangement were adopted. Moreover, the *W* schedule is far more detailed in many respects than is necessary for Library of Congress purposes and would prove cumbersome. Finally, since the *W* schedule represents a totally different approach to the organization of medical knowledge, its adoption would require the reclassification of most of the Library's holdings in Medicine to avoid the endless complications that would ensue if two classification systems were employed for what should be an integrated collection. The work and thought which has gone into the *W* schedule, however, should prove of great value in the expansion and modernization of the *R* schedule, which the Library of Congress hopes to undertake in the near future.

Also proposed for thorough revision as soon as additional specialized professional personnel is available are the Science and Technology schedules, particularly in the areas that have had the greatest growth in recent years.

Interest in the philosophy and techniques of classification and cataloging of scientific material has received a great impetus during the past few years as a result of the tremendously accelerated research programs sponsored by Federal government agencies, the radical alteration in the type of publication resulting from this research, and the dramatic need for bibliographical aids in the utilization of these publications. Before the war the results of scientific research found their way generally into monographic series and into the scientific journals, and the burning questions were the sensible division of subject fields between the learned journals, the making of scientific articles available widely through abstracting and translation services, and the compilation and maintenance on a current basis of bibliographies in specialized fields of research. To be sure, the prewar problems of bibliographical control seemed almost insuperable then, and they have not yet been solved. Nevertheless, the research chemist of 1939, although he might never be sure that he had acquainted himself with all research findings bearing on any particular problem, could derive some sense of security from the assurance that he had overlooked nothing in the major journals in his field, in *Chemical Abstracts,* and in recent monographic publications.

The major change effected by the advent of the Federal government as the great patron (for practical purposes) of scientific research has been a tremendous proliferation of publications in a previously little-used format, the interim or final, individual report by the contracting research institution reporting its findings to the sponsoring government agency. Another important change has been the increased possibility of needless duplication of effort as a consequence of the fact that several agencies may be sponsoring research along related lines of inquiry for similar or related purposes, each without adequate information as to the results being obtained by the other. In fact, it would be not at all astonishing if occasional research projects sponsored by the very same agency duplicated each other to some extent because of the difficulty of utilizing the reports on the research performed. The governmental coordinators charged with the elimination of just such duplication have long since analyzed the problems involved as falling within the purview of librarianship, and the hunt is on for bibliographical techniques and devices that will bring order into this situation.

Indeed, it has led to an astonishing, heightened interest in classification, cataloging, and indexing on the part of officers of the armed forces and other government agencies responsible for the administration of scientific research programs. It is impossible to estimate the number of man-hours that have gone into parallel efforts by these government officials and their assistants in exploring this terrain. However, the process of self-education involved has not been entirely wasteful. It has led to the conviction on the part of most of them that coordination of such effort would be extremely helpful and that there is more involved in the techniques of classification and in the development of subject headings than is apparent at first glance. In recent months, it has become less common to find cases similar to that of the nonlibrarian who, faced with the problem of classifying the research reports received by his agency, became enthusiastic over the development of an entirely new system of notation by one of his assistants. The distinguishing feature of this new notation, it turned out, was its use of both letters and numbers.

The initial period of interest in the problem of bibliographical control of this material seems to have ended as far as the administrator is concerned. The inclination now seems to be toward acceptance of the thesis that the work of cataloging and indexing these specialized scientific reports had best be left to librarians trained in the natural sciences who would experiment with various established systems of classification, utilize subject-heading lists already in existence as far as possible, and in consultation with the scientific specialists determine the extent to which it was necessary to break new ground.

One of the major difficulties in coordinating efforts toward a joint solution of the cataloging problems involved has been the special interests of the individual governmental bodies in the research. Organization of the reports on the basis of a universal outline of knowledge has seemed to them stodgy and impractical, since their immediate interest is the application of the research results, in many cases, to the problems of military, naval, and aeronautical science, and each group has felt that any system of bibliographical control designed for its purposes should be oriented with its own special interest as the center. It cannot be denied that this point of view does have validity, but the effort to achieve standardization and yet satisfy the demands of all agencies may well prove to be impossible.

The Library of Congress has been consulted frequently in regard to these questions by representatives of governmental agencies concerned with scientific research, and more than a year ago the Office of Naval Research, realizing that a satisfactory answer to its need for control over the thousands of research reports it was receiving could not be derived without the assistance of an institution devoted to the problems of librarianship, requested the Library of Congress to undertake the work of organizing its research files and to develop a system of classification and of subject headings for this purpose. With considerable breadth of viewpoint, the Navy stipulated that the resultant classification and subject-heading lists should be applicable not only to the reports it was receiving, but also to those of other agencies and, indeed, should serve as a first step toward the establishment of a system of bibliographical control of scientific publications that would be not only sufficiently elastic to withstand the pressure of current demands, but should be attractive to librarians and scientists throughout the country. To these requirements were added others falling well within the purview of librarianship; i.e., the abstracting of reports received, publication and limited distribution of these extracts, and the provision of a reference and bibliographical service to members of the Navy Bureaus and research workers under contract to them.

Supported by a transfer of funds from the Office of Naval Research, this project was begun last spring, and a scientific investigation of the adequacy of various systems of classification and of all known subject-heading lists for the various fields of science is now being conducted.

In September 1947, realizing its obligation as the National Library to assume the leadership in this field of controlling the published results of scientific research sponsored by the government, the Library of Congress called a conference of representatives of twenty-three agencies of the government concerned with similar undertakings. The discussion of mutual problems at that conference was especially helpful to all present, and the willingness—in fact, the urgent desire—on the part of those present to exchange experiences and inquire into the possibility of standardizing procedures and techniques was an extremely hopeful indication that in the course of the next few years a great advance may be expected in the elimination of wasteful effort and the standardization of cataloging and other bibliographical procedures.

It should perhaps be mentioned that one of the provisions of the agreement between the Library of Congress and the Office of Naval Research called upon the Library to investigate the use of mechanical and other technological devices for bibliographical purposes. In the past few years, the imagination not only of librarians and of government officials responsible for research programs, but even of laymen little concerned with questions of bibliographical control, seems to have been stimulated—and in some few instances, inflamed—by the news that rapid-scanning devices were nearing perfection that would enable a library, through electronic as well as mechanical techniques, to peruse literally millions of entries in a short period of time and automatically produce bibliographies of precisely those items in the total collections needed for specific purposes, thus eliminating the complexity and drudgery of the attack upon the Library catalog that must be made under present circumstances each time it becomes necessary to find the publications on a special subject. In addition, there had been the famous article in which Dr. Vannevar Bush allowed his creative imagination free play with the "memex" idea; there had been considerable experiment with techniques of microprint and with the use of punch cards and other rapid-sorting devices in the field of librarianship. Indeed, for some time the Library of Congress had been conducting experiments in the use of IBM equipment for the production of bibliographies, and it was quite conversant with the developments in automatic sorting techniques.

The Science and Technology Project of the Library, which is the unit supported by transfer of funds from the Office of Naval Research, continues to maintain a close watch over progress in the area of automatic aids, but it has been necessary from time to time to point out to enthusiasts that all automatic devices thus far devised or envisioned can give back in the form of selective or comprehensive listings only the material put into them, and that the relationships between the individual bibliographic entities offer the same basic problems of indexing and cataloging. No such device, immeasurably though it may speed the process of finding the reference desired, will substitute for the experience and the intelligence of the cataloger in the initial stages.

By virtue of its responsibility—a responsibility imposed upon no other library—to maintain a collection and a staff adequate for the research and reference demands of not only the Congress and the Executive and Judicial agencies of the Government, but of the private scholars, the research institutions, and the private citizens of the United States, the Library feels that it is justified in considering itself as the National Library of Science as well as the National Library generally. If one considers that it already possesses collections of publications and other materials in the disciplines of natural science comparable to, if not more comprehensive than, any other collection in existence, the likelihood that this claim can be challenged is small indeed. It should be borne in mind that to function as the National Library of Science, the collections must be extremely extensive in fields important not merely for laboratory research but for the administration of scientific programs, for legislation affecting the progress and sponsorship of science, for scholarship and research in those areas that reflect the constant interplay between the advancement of science and the other elements in national cultures. Perhaps as never before it is important to have information available on the cultural lags in various parts of the world which offer an impediment to the social, economic, and political progress that can follow in the wake of enlightened application of scientific findings. For any library to be a national library of science, its collections must be a great treasury of information in almost all branches of knowledge. They must contain the complete sets of documents of governmental, learned, and other institutions, the proceedings of parliaments, and the official publications of all countries. It would not suffice to have merely every last scrap of publication that can be classified directly as pertaining to natural science.

Where it has seemed advisable to share this national responsibility, the Library has done so. Thus, as long as the Library of the Department of Agriculture and the Army Medical Library continue to maintain adequate collections in their respective fields and to discharge the national responsibility for research library service at a high level, the Library of Congress will not purchase extensively in the fields of agriculture and medicine, but will limit itself to the works that are necessary to maintain a thoroughly encyclopedic collection.

The ideal which the Library of Congress has set for itself as the National Library of Science and the limitations imposed by budgetary considerations upon the service which it is at present able to give are quite disparate. For one thing, the Library of Congress has never possessed sufficient staff trained in the sciences to perform adequately the function of mediation between its collections and the users of the Library; nor has it had the personnel competent to work steadily in the selection of needed materials for the strengthening of its scientific collections. Finally, it has in the past lacked the staff to concern itself as fully as it should have with the problems of the cataloging and organization of scientific materials.

In order to interpret the materials it already has and recommend the acquisition of those it needs, in order to perform a reference service and develop a bibliographical program in keeping with its duty to the laboratory, the school, and to industry, the Library of Congress recommended to Congress a year ago that it should have a division of Science and Technology staffed by approximately fourteen subject specialists and twenty-one expert bibliographers, together with secretarial and clerical assistants, all together a total of more than forty people whose subject knowledge would comprehend geology; metallurgy; astronomy and mathematics, including statistics; nuclear physics; industrial physics, including optics, acoustics, and heat; organic chemistry and biochemistry; physical chemistry, including analytical, colloidal, and nuclear chemistry; chemical engineering; civil and structural engineering; mechanical engineering; electronics and electrical engineering; botany; morphological zoology, including comparative anatomy, cytology, histology, protozoology, and taxonomy; functional biology, including general and comparative physiology of plants and animals, physiological chemistry, biophysics, zymology, nutrition, and perhaps viruses, bacteriophages, germicides, antibiotics, vitamins, and bioassay techniques.

Beyond the requirements of personnel, there is the need for additional facilities that would make it possible for groups of investigators from the agencies of the government to be stationed at the Library and provided with office and other space, in order that they might work with the collections directly and most conveniently. As matters stand, the collections available are infinitely better than the space facilities for their exploitation by such research workers. Furthermore, there should be space and staff sufficient to permit the establishment of abstracting and bibliographical services based on the already existing collections and the current receipts of materials.

From time to time it has been apparent to other agencies of the government and to other libraries that the Library of Congress was best equipped to undertake certain operations of national significance in keeping with its scientific program, and they have found it advisable to support such projects financially.

The Science and Technology Project undertaken on behalf of the Office of Naval Research has already been mentioned. Recently, the Library undertook the distribution of the scientific reports of the Office of Scientific Research and Development for that agency. It was felt, after the termination of the war, that much of the research performed under contract with the OSRD could be made public without danger to national security. Many of the reports had been reproduced in multiple copies, and it was decided to make them accessible to as wide a public as possible by distribution of these copies to libraries. In April 1946, the Library, utilizing transferred funds, began the distribution of these reports to ninety selected libraries. More than 165,000 copies, representing 30,000–35,000 titles, were sent out.

In cooperation with the Commerce Department, the Library's Photoduplication Service has distributed at low cost tens of thousands of copies of German scientific and industrial documents in microfilm or photostat form. These reports had been filmed by the Office of Technical Services in Germany and stored, for the most part, with the Library of Congress. Their availability has been of tremendous advantage to scientific laboratories, industrial organizations, libraries, and individuals throughout the country.

The Library's cooperative acquisitions program, conducted in Western Europe with the support and on behalf of the nation's research libraries to provide copies of the publications of the war years that had not been available to them, has brought into the country a vast accumulation of scientific as well as other publications. Approximately 1,324,000 individual items have been distributed and are now at the disposal of the research worker.

In short, whenever it has been asked to render national bibliographical services within its purview and its technical competence and to the extent that support therefor has been forthcoming, the Library has undertaken to fill the need. There is sufficient evidence of the logic behind the Library's assumption that there exists a constant demand for the services which only a national library can provide, on the basis of a thoroughly comprehensive collection of materials through the employment of the technical skills developed over a century and a half, and stimulated by a feeling of responsibility to make its facilities, collections, and experience widely available for the national benefit. To the extent that its resources permit, it will continue in the endeavor to devise ways and means of improving its services to science.

THE UTILIZATION OF SCIENTIFIC KNOWLEDGE

J. W. PERRY

Mr. Perry took his S.M. at M.I.T. in chemical engineering in 1931, and then spent a year in Germany as an exchange student. He has been on the research staff of Allied Chemical and Dye Corporation and the Ballistics Research Laboratory at Aberdeen Proving Ground, where he did exploratory work on explosives, particularly rocket propellants. Since 1945 he has been at M.I.T. developing methods for using mechanical and electronic devices in correlating chemical information and in searching files. Mr. Perry is also working on two books: "Chemical Russian, Self-Taught" and "An Introduction to Scientific Russian."

SHIRKING boring tasks is a deeply ingrained human trait. This urge to avoid the tedious, though often called laziness, has motivated the invention and development of the highly efficient machinery characteristic of our modern world. Chemists—being human—dislike time-consuming, monotonous tasks. Perhaps the most distasteful of these is the job of literature searching, the task of hunting for what other chemists may have done in the field of interest. At first glance it might seem that this task cannot possibly be mechanized. Yet patterns of holes in cardboard (or transparent spots in films or magnetic spots on steel tape) can be used to convey intelligence. Such patterns can be sensed by mechanical fingers, electric eyes, etc., and impulses so generated can be compared with other patterns and sorting operations effected. These newer techniques for manipulating information appear capable of advantageously supplementing existing methods of abstracting and indexing.

CHEMISTRY has been defined as the science of the transformations that matter undergoes and the energy changes that accompany such transformations. In line with this definition, chemists are interested, first of all, in the various individual substances considered either as starting materials or end products of chemical transformations. As the result of much work, the molecular structures of a very large number of individual compounds have been investigated in great detail, and the relative position of the bonds between component atoms have been learned with precision for even complicated molecules. These facts are recorded faithfully in the shorthand of chemistry as structural formulas. Structural formulas, however, have their limitations. Although in theory it may eventually be possible to deduce all the properties of compounds from their molecular structures, this cannot be done at present. Hence, structural formulas cannot serve as a basis for so extensive a correlation of properties, phenomena, and concepts as do the symbols of mathematics or the definitional equations of electrical engineering. Structural formulas, nevertheless, do furnish a cohesive pattern in terms of which chemists think when considering not only the architecture of molecules, but also their properties. Consequently, in spite of the fact that not all chemical facts can be deduced from structural formulas, these formulas must be accorded a position of great importance in considering chemical information problems. It is equally true that such problems cannot be dealt with satisfactorily by limiting consideration to structural formulas alone.

Although the energy changes accompanying many chemical transformations are known with the same high precision as the structural formulas of many compounds, the field of chemical kinetics is by no means exhaustively described, or even dominated, by thermodynamics. It would be difficult to make clear to a nonchemist the degree to which chemists in general, and organic chemists in particular, are guided by precepts of a nonmathematical or even intuitive nature. This does not mean that a highly skilled chemist can afford to neglect thermodynamic considerations, but it does mean that success in his efforts is often the result of an intuitive feeling for his subject that can scarcely be described as other than that of an artist.

This combination of mathematical precision, as exemplified by structural formulas and thermodynamic measurements, with a more elusive quality requiring an intuitive approach gives the field of chemistry a peculiar character and charm.

From the foregoing, it is perhaps apparent that in indexing or classifying chemical literature one is confronted with three types of concepts: structural formulas, which by their very nature partake of a high degree of precision, since such formulas define certain precise relationships between each of the individual component atoms of molecules; concepts, such as those of thermodynamics, capable of mathematical definitions; and nonmathematical concepts, whose definition inevitably must involve problems in semantics.

The very nature of chemical information is such that the papers dealing with new results in the field inevitably must be rather long if valuable detail is not to be lost. This fact, coupled with the extensiveness of the chemical literature, has led to the development of a means for enabling chemists quickly to examine the essential features of extensive accumulations of information without the necessity of laboriously reviewing masses of detail. This method is the preparation of abstracts or summaries, of which there are two types. One mode of abstracting or summarizing the literature prepares a separate abstract for each paper, patent, or similar publication as it appears in the periodical literature. Abstracts of this type have been published for many years by various periodicals, of which *Chemical Abstracts, Chemisches Zentralblatt,* and *British Abstracts* are widely known and used. The other type of abstract summarizes information relating to a single compound, no matter how widely such information may be scattered through different publications. This latter type of abstract has formed the basis of the great German compendia *Beilsteins Handbuch der organischen Chemie* and *Gmelins Handbuch der anorganischen Chemie.* Both types of abstracts have proved their value, and chemists would be very badly handicapped in their work if either type were to be discontinued.

Useful as these two types of abstracts have been in decreasing the number of words a chemist must read in order to search out information, still more help is needed, since the abstracts themselves fill many volumes. It is necessary to provide some means that will enable a chemist quickly to locate those abstracts in which he is interested. Two different methods have been used to provide pathways to abstracts of interest. One method is the classification of abstracts according to their subject matter. This has proved particularly useful with those abstracts prepared in such a way as to be reasonably homogenous with respect to their content, as, for instance, abstracts of the Beilstein or Gmelin type, which refer essentially to single substances. Thus, for example, Beilstein groups the abstracts according to certain features of the structural formulas of the compounds being considered. Such grouping of abstracts has two useful effects. One is to assign to an abstract dealing with a given compound a place in the system which is fixed in relation to abstracts dealing with other compounds. The other effect is to group together on adjacent pages abstracts dealing with related compounds. It is impossible, however, for the Beilstein system, or, for that matter, any classification system, to provide for all the possible groupings of the compounds. As a consequence, it often happens that a chemist is interested in a particular group of compounds the summarizing abstracts for which are scattered through many pages and volumes of the Beilstein compendia.

Abstracts reporting the content of individual papers rather than summarizing information on individual substances cannot usually be arranged so accurately or advantageously within the framework of a detailed classification scheme. At present, abstract journals covering the entire field of chemistry do not undertake detailed classification of abstracts. Rather, the abstracts are classified into a small number of general classes and cross references provided for the relatively few abstracts pertaining to more than one general class. If detailed classification of such abstracts were attempted, it would be necessary to enter the majority of them under several different headings in the detailed classification scheme. The resulting necessity to provide and also to use a multiplicity of cross references renders such detailed classification of abstracts impracticable.

Since abstracts of the type published by *Chemical Abstracts, Chemisches Zentralblatt,* and other abstract periodicals do not adapt themselves well to detailed classification, another method has been developed for providing leads to such abstracts. This other method is subject indexing, which takes note of those individual aspects or features of each information item of interest to chemists. In indexing abstracts, attention is devoted quite naturally to the specific substances, phenomena, facts, and theories with which the abstract and its parent paper are concerned. Unless a given publication is specifically concerned with general concepts and broad principles, the latter do not form the basis of index entries.[1] For these reasons, subject indexing, as now practiced, does not as a general rule record relationships between individual compounds, phenomena, facts, and theories, on the one hand, and the generic groups and broad concepts on the other. Working out such relationships between the specific and the general is left as a task for the user of the index. Thus, for example, subject indexing as now practiced does not group all olefins under the index heading "olefins."

In considering this problem, another point should be noted. If conventional methods of printing indexes in book form are to be used, then it would be impractical to prepare indexes that would group specific entries pertaining to individual compounds, facts, etc. under generic and subgeneric headings. Moreover, if such indexes were prepared, they would be so bulky that they would fill many volumes and thus prove not only intolerably expensive to publish, but also so awkward and time-consuming in use as to run serious danger of defeating their own purpose.

It should also be noted that the collection of information relating to questions of narrow scope can be accomplished reasonably well by using existing indexes, aside from the not inconsiderable time and effort involved in handling bound volumes, making notes, etc. The situation is, however, less favorable when searching for information pertinent to an inquiry of generic scope. In conducting such searches, the user of the index is forced to look under a multiplicity of headings, with the expenditure of a great deal of time. His problem becomes even more difficult if his scope of inquiry is defined in terms of two or more general concepts. In such a case, it is necessary to search through a large number of index entries relating to one general concept, repeat the same general procedure for the additional concept or concepts, and then attempt to find as best he can those references that deal with the several defining concepts simultaneously. In other words, subject indexing proves an inefficient method for obtaining answers to generic questions, although in general it serves excellently as a means for finding answers to questions of narrow scope.

This brief discussion of the two types of abstracts should suffice to indicate the need for developing improved methods for answering questions of generic scope when using large collections of either of the two types of abstracts mentioned. This need for developing improved methods for selecting desired items of information from large collections of chemical information constitutes one of the most promising possible uses of punched cards and similar devices in the chemical information field. Various problems connected with this possibility of applying punched cards and related mechanical techniques are being studied by the Punched-Card Committee of the American Chemical Society. Consideration is also being given to other possibilities of applying such mechanical techniques so as to render published chemical information more easily and more quickly available.

During recent years the experience of a large number of scientific workers, both in chemistry and in other fields, has demonstrated that punched-card methods can be used to very good advantage in managing small files of information.[3] When used for this purpose, the various items of information are entered on individual cards, and the attributes characterizing such items are indicated by appropriate punching of the cards. In this way, it becomes possible to use mechanical devices to sort the file conveniently and quickly on the basis of any one attribute. For hand sorting, a very simple device may be used, such as a knitting needle mounted on a handle. For fully automatic sorting, complex machinery is required. The results obtained to date, particularly with relatively small hand-sorted files of information, have been so satisfactory that the American Chemical Society is supporting investigation of means of improving and extending the use of punched cards and similar devices in coping with problems posed by the chemical literature.

The question as to whether it would be possible and practicable to use punched cards or similar devices for handling files of information embracing the entire field of chemistry constitutes a challenging problem, requiring unusually careful study. It appears likely that such an adaptation of punched cards or related devices will involve the establishment of new methods for analyzing chemical information and also chemical concepts so as to permit their manipulation to best advantage. Such an analysis of information is a prerequisite to making it amenable to manipulation by machines which are caused to operate not by ideas, but rather by various physical means, such as holes in a card or magnetic spots on a steel tape, coded to represent ideas. Development of coding schemes for molecular structure constitutes the principal progress made in this respect to date. Here, the work of Dr. G. M. Dyson has been outstanding. Thanks to his efforts, there now exists a method for translating molecular structural formulas into a linear set of symbols consisting of letters, digits, and punctuation marks.[3] The Dyson symbolism represents the full details of molecular structure and is also amenable to handling on punched cards. Unfortunately, existing types of automatic equipment cannot handle the Dyson code to good advantage. This is scarcely surprising, since such equipment was designed and developed primarily for manipulating figures in bookkeeping operations.

The problem of developing a comprehensive punched-card code for chemical terminology other than that expressible in terms of molecular structure is being studied by the Punched-Card Committee. Work now under way is directed toward developing new methods for correlating the operations of indexing, classifying, and coding chemical information. Such work involves problems in semantics, logic, and the structure of language. It is hoped that a careful study of chemical concepts and related terminology may make it possible to build up an expansible network of relationships that will greatly increase the degree of usefulness of existing indexing methods. More specifically, it is hoped that establishment of relationships between specific and generic concepts and terminology will permit information indexed in terms of specific headings to be located readily when searching is conducted from a generic point of view. If this can be accomplished, it will be important not only with regard to locating information, but also with respect to analyzing information, establishing cause-and-effect relationships, etc. Such an approach to the chemical information problem appears to be the one that will utilize punched cards and similar sensing and sorting devices to best advantage.

Of more than usual interest in connection with these future possibilities is the recent demonstration by the Classification Division of the U. S. Patent Office that punched cards and accessory equipment can be used very advantageously as tools for facilitating the editing and printing of the usual type of index.[4] In preparing the currently available index to the *Manual of Classification of U. S. Patents*, a file of cards was punched so that, when used to operate a tabulator, successive cards in the file produced successive lines in the finished index. Particularly interesting features of this approach to the problem of preparing indexes are the possibility of cumulating and the ease with which revisions can be made at any time by merely removing, inserting, or rearranging those cards corresponding to the lines to be changed. Arranging the cards alphabetically or in accord with other orderly schemes can be effected by mechanical sorting devices. When using this method, proofreading consists of checking the accuracy of the punching on the cards; this can be done in various ways, one of which is to use automatic equipment to compare two sets of cards punched by different operators. It would appear, from the experience of the Patent Office, that there is an excellent chance that punched cards can be used to advantage to facilitate editorial work in the preparation of compendia and the publication of periodical abstracts. If such use of punched cards and related devices can be successfully combined with a new approach to the old problem of indexing and classifying chemical information, then there is good reason to hope that considerable improvement can be achieved in existing methods for making published chemical information more readily accessible to chemists.

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TWO SQUARES OF GLASS

Two squares of glass,
A purple and a blue,
Light my carrell;
And bring to grey stone walls
A brilliant hue
Of dreams not found in books.

Did some old monk
Immured with vellum tome
Find life too grey,
Reflected skies too few,
And add two squares of glass,
A purple and a blue?

Emily Ericsson

INTERNATIONAL CULTURAL COOPERATION

DAN MABRY LACY

Mr. Lacy (M.A., North Carolina, 1935) was recently appointed Assistant Director for Acquisitions of the Processing Department, Library of Congress. He was formerly Assistant Archivist of the United States and a member of the Committee on Conservation of Cultural Resources.

IT IS natural that scientists should from the beginning have taken the lead in programs for the international exchange of publications. As the Frenchman Alexander Vattemare, himself an amateur scientist, emphasized in a memorial presented to Congress on February 5, 1840:

> Science overleaps the boundaries which political systems interpose between nations. All the men consecrated by its worship are brethren. . . . From one end of the world to the other they understand each other's tongue; the discovery of one is the triumph of all; and, thanks to this fraternity, this federal union of intelligence, science is elevated, and daily extends the empire of civilization.

This memorial led directly to a Joint Resolution passed in July 1840 providing the first Congressional authorization for the international exchange of government publications. International exchanges, especially in the field of science, already had a long history in this country, however, having been carried on since early in the century by the American Philosophical Society and the American Academy of Arts and Sciences.

The real foundation of a comprehensive and systematic arrangement for the exchange of scientific and other publications was, however, the achievement of one of America's greatest scientific pioneers, the physicist Joseph Henry, who made such a system one of the cornerstones of his farsighted program for the Smithsonian Institution. The system of scientific exchanges initiated by Henry as the Smithsonian's first secretary has now been in operation for almost exactly a century and has assembled in the Library of Congress, as the beneficiary of the program, an unparalleled collection recording the advance of science in all its aspects throughout the world.

Out of the visions of such lovers of science as Vattemare and the practical work of such scientists as Henry, who realized the crucial importance of the widest international availability of scientific studies, has grown the whole elaborate machinery of international exchange, which now covers all fields of knowledge and all countries on the globe. The multilateral Brussels Convention of 1886, to which nineteen countries have adhered, twenty-six bilateral executive agreements between the United States and other powers, and literally thousands of individual exchange agreements made by the Library of Congress and other American institutions with foreign institutions provide an annual flow of hundreds of thousands of publications between the United States and other nations, enriching scientific endeavor throughout the world.

Scientists were among the principal losers from the disruption of this world-wide network in the recent war. Though scientific research went on at a greatly intensified pace in all countries, the requirements of national security and the cloture of communications with wide areas of the world stopped the free exchange of discoveries. It has been the work of governments, scientific agencies, and libraries since the end of hostilities to fill the gap of the war years and to restore the exchange network to its former efficiency.

These endeavors have met with signal success in making available to American scientists the results of scientific progress in enemy countries during the war years. Readers of THE SCIENTIFIC MONTHLY are undoubtedly familiar with the work of the Office of Technical Services of the Commerce Department in disseminating captured scientific and technical studies, and with the services of the Library of Congress, through the Cooperative Acquisitions Project, in obtaining and distributing to major American libraries European publications of the war period. Except as governmental censorship still curtains off the results of research affecting the security of the various nations, American scientists again may draw on the achievements of scientists everywhere in forwarding their own researches.

Scientists elsewhere are not so fortunate. Not only did the war cut off their receipts of scientific publications from other countries; its iron progress across their countries disrupted their universities, scattered their colleagues, and destroyed both laboratories and the painfully assembled collections of libraries. In war-torn countries the problems of exchange have not been those merely of filling gaps and restoring normal relations, but of re-creating the bases for scientific endeavor.

American institutions have moved generously to meet this need, acting principally through the medium of the American Book Center for War Devastated Libraries, Incorporated. This nonprofit corporation, representing diverse institutions and educational groups, has received as donations from American libraries or has purchased through donated funds 4,000,000 books and periodical issues, which have been distributed without charge to hundreds of libraries and institutions in thirty-four countries. A very large part of these publications is intended to provide the library basis for the revival of both teaching and research programs in the natural sciences and to make available in the rehabilitation of those countries American advances in such fields as agronomy, engineering, industrial technology, and medicine. In the Latin-American countries, which did not, of course, receive the special aid offered through the American Book Center to war-devastated areas, a special Hispanic Exchange Project, administered by the Library of Congress and financed by the State Department through the medium of the Interdepartmental Committee on Scientific and Cultural Cooperation, has permitted the wide distribution of scientific publications of the war and the postwar period, including studies[1] on subjects such as antimalarial drugs, of particular Latin-American interest.

THE nature of the need the American Book Center was created to meet is now changing. Much of what can be done by mass shipments to replace war-destroyed collections has been done. A more selective program to meet the specific needs of individual institutions and to supply them with technical journals and other current publications is required. At the same time, European institutions are now increasingly prepared to reciprocate by the exchange of their own publications. The program of the American Book Center, established to meet an emergency demand, needs to be converted to serve the permanent function of providing a central focus for the exchange relationships of American institutions with other institutions throughout the world.

To meet this need, there has just been created as a successor to the American Book Center a United States Book Exchange, established as a nonprofit corporation in the District of Columbia, with headquarters in the Library of Congress. It is hoped to draw into its operations and into participation in its direction all major American library and scholarly interests. The temporary directors of this corporation are: Scott Adams, William G. Carr, Leila F. Clark, D. H. Daugherty, Sidney B. Hill, Paul Howard, Dan Lacy, Harry N. Peterson, and Raymund Zwemer.

Though it can draw heavily on the experience of such large-scale cooperative ventures in the exchange and distribution of books as the Cooperative Acquisitions Project, the Hispanic Exchange Project, the Surplus Books for Veterans Project (through which the War Assets Administration, the Veterans Administration, the Federal Works Agency, and the Library of Congress cooperated in the distribution to American institutions of several million surplus textbooks used in the war by educational programs of the Army and Navy), and the American Book Center itself, it will, of course, be necessary for the new United States Book Exchange to develop its program and procedures gradually and experimentally.

Tentatively, it is proposed to accept from

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[1] Shaffer, K. R., and Kipp, L. J. Books—Agents of War and Peace. SM, 64, 5, 428–32.

American libraries and other donors duplicates of recent publications of substantial interest in subject fields needed by foreign libraries. Credit will be given American libraries in proportion to the quantity of material supplied for exchange with foreign institutions. These publications will be shipped by the United States Book Exchange to selected foreign libraries on the basis of their capacity to reciprocate by the exchange of their own publications and of any available duplicates from their own collections. Materials received on exchange from the foreign libraries will in turn be redistributed to American libraries, either on the basis of their interest in specified subject categories or on the basis of selections of specific titles from lists of materials received by the USBE. The distribution of materials received on exchange from foreign institutions will be in proportion to each American library's contribution of publications for shipment abroad, though it is not anticipated that this can be on a piece-for-piece basis for some time to come, until the collections of libraries abroad and the publishing activities of foreign scholarly organizations are restored.

It will thus doubtless be necessary to accept one foreign publication in exchange for several American publications. In addition, it will be necessary to make a small service charge to the American libraries participating in the program to help to defray the operating costs of the United States Book Exchange, though efforts will be made to secure other support for its activities in order to reduce the service charge as much as possible. Even though the exchange will be unequal at first, American libraries will still be the gainers. One European or Asiatic publication that a library does need is worth more to it than several duplicates of American publications that it does not need. Moreover, the services of the USBE will relieve American libraries of much work involved in the listing of their duplicates, the circulation of exchange offers, the solicitation of materials on exchange, and the carrying on of extensive correspondence.

The operations undertaken by the USBE itself will be aimed primarily at the relatively large-scale exchange of duplicates and other noncurrent publications. It will be the purpose of the organization insofar as possible to encourage and facilitate the direct exchange of their current serial publications between American and foreign institutions rather than to supersede any existing exchange of this kind or to undertake a permanent program of handling material of this type.

The recent enactment of the Smith-Mundt Bill, extending to the Eastern Hemisphere the Federal government's existing program for cultural cooperation with other countries, affords a further opportunity for the increase of international exchange activities. One of the principles of the legislation is that the State Department shall utilize insofar as possible the services of established nonprofit organizations in carrying out the purposes of the bill, and the USBE will be a logical medium to carry on activities in the international exchange of publications that the government may wish to support under appropriations made to carry out the provisions of the Smith-Mundt Act.

Now more than ever, when the opening up of new fields of scientific research as a result of developments in electronics, nuclear fission, and the use of radioactive elements offers the possibility of revolutionary advances in human well-being and when the destitution in so much of the world cries out for the swift application of the results of scientific progress to the restoration of agriculture, industry, and health, it is a matter of urgent importance to all men everywhere that there be the freest interchange of information about scientific advances and the most widespread pooling of knowledge. The restoration and enlargement and the making more immediately current of the already generations-old system of the interchange of scientific publications, on which so much of the scientific progress of the last century has been based, is one of the cheapest and most effective means by which scientists, librarians, and administrators can contribute to this end.

LIBERAL EDUCATION AND THE PHYSICAL SCIENCES*

H. M. DAVIS

After taking his Ph.D. in physical chemistry at the University of Minnesota in 1934, Dr. Davis took charge of the chemistry department at Itasca Junior College College for two years. In 1936 he went to Pennsylvania State College, where he is now associate professor of metallurgy. Dr. Davis' interests include molecular spectra and constitution and thermodynamics of process metallurgy.

THE extraordinary attention directed to a list of about 110 books, which has been publicized by the unconventional curriculum of St. John's College, Annapolis, Maryland (Mark Van Doren. *Liberal Education.* New York: Henry Holt, 1943. Pp. 150–52), has resulted in considerable discussion among educators as to the actual potentialities of the named books in the education of the contemporary college student. Naturally, opinion is divided. There are those who are sure that within these books lies the perfect passage to the realm of liberal education, whereas others are as firmly convinced that the list was prepared by confirmed classicists out of love of the classics for their own sake—and not from any sober evaluation of their usefulness to living men. Although it is a familiar observation that dissension is not always lessened by argument among the dissidents, this topic of "The Hundred Books" has aroused so much interest that its discussion flourishes. What follows is a segment from one such organized discussion. It is a consideration of a reading list intended to provide an important part of a liberal education in the physical sciences and is divided into three phases: an argument that a liberal education in the physical sciences is legitimate—even desirable; a definition of the type of book which I consider the best aid toward teaching youth the history and the significance of the development of scientific thought; and a listing of certain books well suited to this purpose, with notes on the merits of the individual books.

It would be well to begin by defining "education." To recognize this as a desirable need is somewhat easier than the formulation of the definition itself. Unnumbered persons have striven nobly to establish a concise and acceptable definition of "education." If all the ink and graphite thus expended were gathered together, they would make an awesome smudge. A principal difficulty in the attempt to circumscribe such an idea is that one may merely assemble a series of platitudes of pleasing sound but of little meaning. Nevertheless, with no pretense of writing a definitive expression of the whole concept, I offer as a working basis the following statement *about* education: *It is the gradual, voluntary adjustment of the conscious individual to his physical and spiritual environment through the progressive acquisition of his intellectual and social heritage.* With the spiritual I mean to include all things not physical but of the mind. Note that the educable individual is conscious—not hypnotized. He is free and he proceeds toward his adjustment voluntarily. Further, as the total heritage of man is hardly to be encompassed during the life span of any individual, its acquisition is an endless process.

These ideas appear to be harmonious with a recorded thought of Jacques Barzun which regards education as "a lifelong discipline of the individual by himself, encouraged by reasonable opportunity to lead a good life" (*Teacher in America.* Boston: Little, Brown, 1945. P. 7); and with that of a committee of the British Association for the Advancement of Science which, reporting on a study of postwar university education, stated that ". . . the task of education includes that of fitting a person for his environment, spiritual, human, and material . . ." (A.A.A.S. *Bulletin,* March 1945).

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* Based on a lecture given January 17, 1945, as No. 4 of the 1944–45 Wednesday Reading Series, The Library, The Pennsylvania State College.

Perhaps we can at least agree that the university's objective in this lifelong process should be to send forth a mind beginning to feel at home in its world and moderately well versed in some branch of human learning, but not yet profound in any field. This major endeavor becomes the "control panel" to which other lines of knowledge are strung, and thus it serves as a basis for, and impetus to, continuing self-education. Commonly, the major subject elected is a language, literature, history, or mathematics; but, quite as properly as one of these, the physical sciences may constitute the tower about which the structure of a liberal education is erected. Indeed, Mark Van Doren, reviving the trivium and the quadrivium of the ancients' seven liberal arts, insists that "the liberal arts survive more intact in [the laboratories of the physical sciences] than elsewhere in education today; other studies, studying them, could learn about themselves" (*loc. cit.* P. 136). And Barzun thinks: "There is no doubt whatever about the place of the sciences; they are humanities and they belong in the college curriculum" (*loc. cit.* P. 91).

There are, of course, many undergraduate students who are appalled at any association of the terms "liberal education" and "physical sciences." This attitude is perhaps a product of the popular misconception that liberal education means superficial education. The general acceptance of Van Doren's simple statement that liberal education is complete education (*loc. cit.* P. 12) would be of tremendous value to this country if it accomplished no more than the modification of that superficial attitude toward learning. Actually, it is an acknowledgment of the propriety of including the physical sciences in a liberal-arts curriculum that in many of our universities one can take a baccalaureate degree in the college of arts and sciences with the major in any of the biological or physical sciences. For example, it is often possible to take a Bachelor of *Arts* degree with a major in such a subject as physics. And why not? If the purpose of a liberal education is to improve a man's adjustment to his world and to leave him with a more serene philosophy, it can no more justly omit all learning in the natural sciences than it can avoid the history of mankind or the psychology of human behavior. In an age of radio communication, transoceanic air travel, radar fire control, guided missiles, and fission bombs—and yet an age in which the man in the street still does not understand the ordinary telephone—how can an intelligent person be truly at ease without sufficient understanding of the physical sciences to enable him to interpret and comprehend new inventions and new engineering achievements as they appear? (Perhaps it is enough to ask how, in such an age, *any* person can be truly at ease.) Too few of us have any worth-while knowledge of physics (I include chemistry as a branch of physics). We drive an automobile (because it is nearly foolproof) with little appreciation of the hidden, beautiful mechanism that powers it, and with no conception of the creative thought that went into its development; meanwhile, we demand the family airplane. We listen to a radio receiver whose complex operation is utter magic to us, and we demand the even more complex television. We are a race of lever-twiddlers, button-pushers, and knob-twirlers, enjoying but not comprehending the products of the prodigious technical labors of a comparatively few men. To produce a people who are fitted and *worthy* to live in this age, there is need of a revision in our educational outlook, to the end that the physical sciences shall receive greater attention in the schooling of the representative individual.

SINCE the circumstances (fortunately, perhaps) provide no opportunity for immediate debate, it suits the pragmatic purpose to assume that the desirability of a liberal schooling in the physical sciences is recognized and accepted. Thus we proceed to a consideration of the method of attaining the desired end.

To present the facts and principles of the sciences as they are known today, there is probably no completely satisfactory substitute for laboratory and classroom instruction by able and interested teachers, coupled with the faithful digestion of up-to-date textbooks that make rigorous use of mathematical examples of those principles. But it is a rare textbook which, while dealing amply with facts and principles, can impart more than a smattering of the history of their discovery and evolution. Hence, such formal courses—unless they be under omniscient teachers with limitless time at their disposal—need to be supplemented and augmented by the reading of the student. Let it be clear at once that it is the purpose of the books to be used in this connection not to make of the student an expert or master in the sciences, but to convey to him the history of the development of those sciences, the biographies and the philosophies of the men whose lives have extended man's scientific horizon, and an awareness of the place of the sciences in modern life.

It is to be doubted that a reading of the science classics themselves will achieve the desired results for any but the most extraordinary undergraduate. Here it is that we part company with the St. John's list insofar as the classics in the physical sciences are concerned. Certainly, the list contains many books of great literary and historic significance. One is impressed, however, by the fact that in the natural sciences it is both deficient and antiquated and, consequently, that its books are undesirable as primary sources of knowledge. The books in the physical sciences, particularly, promise very limited educational effectiveness. Although there was a time when each of these books was in the vanguard of knowledge, the present valid truths in many of them are so diluted with misconceptions that one may reasonably question the wisdom of assigning them to the typical or even to the superior undergraduate. In others, the contents are still reliable, but the difficulty of reading them is prohibitive. For example, without several years of intensive preparation in physics and mathematics at the university level, few persons can extract much of value from the uninterpreted writings of Newton or Maxwell.

If the student essays a direct study of the earlier science classics, he has desperate need also of the capacity for evaluation. Whereas with Newton and Maxwell his understanding will be supremely taxed, when reading Boyle, Huygens, Dalton, Lavoisier, or the like, he must know how to choose between the kernels of truth and the chaff of doubtful import. This the young scholar is not prepared to do. He needs, instead, a commentator who has already sifted the pile and who will point out his cupful of gleanings. The student can admire a scientific ancestor for the valid fragments of truth he occasionally wrote, without also wading through the confusion of many pages of now-useless speculation on many now-meaningless questions, expounded in now-unintelligible terminology.

It is currently argued by thoughtful persons who are considering the problem of civilizing the youth of Germany that the primary source of the difficulties expected in any attempt to revise the viewpoint of the fanatical young Nazis is the fact that they as children were filled with the Nazi doctrines before they were able to distinguish good from evil, the valid from the spurious. Yet the apostles of the St. John's list want to do a similar disservice to young college students by directing them, before they have learned the scientific right and wrong, to read science classics in which there is often to be found a treacherous commingling of wrong and right. The mature scholar who is already secure in his facts will derive pleasure and profit from reading such classics, but it appears unwise to permit immature students to absorb the untruths that mingle with the wisdom in these works.

Lastly, a reading of a book, even if it be understood, is no guarantee of an appreciation of the author or his times. The aspirations and exultations, as well as the struggles and disappointments, of the man are often only poorly revealed in his bare scientific writing. The student needs biography and commentary.

The value of biography may be emphasized by the example of Henry Cavendish. It is interesting enough to read of his experiments on gases and of his clearly reasoned deductions from them, especially his proof of the composition of water and his demonstration, late in the eighteenth century, that air consists of oxygen and nitrogen plus a gaseous residue of 0.83 percent of the original volume. This residue he cautiously ascribed to experimental error. He also made extensive researches in electricity but never published them, so that it was left to Faraday to rediscover the same results a generation later. The facts alone are interesting. But how fascinating these facts become in the light of contemporary revelations of the amazing timidity and reclusiveness of the wealthy bachelor, and the history of later researches on the atmosphere!

His scientific reticence seems but the parallel of his lifelong shyness. The enormous wealth he had inherited from his father had been augmented by a large legacy from another relative. Provision for the modest wants of his semiascetic existence required less than the accrual on his banked cash, so that the already great fortune continued to grow. Thus, at the end of his life, Cavendish was the wealthiest depositor in the Bank of England. Even so, his banker frequently urged him to invest part of his money in some profitable enterprise (I suppose that then, even as now, time deposits drew no great interest). Ultimately, Cavendish became so annoyed with this insistence that he threatened to withdraw his money from the bank unless the suggestions ceased. In his home, to promote his isolation, this island in an island of islanders had a special servants' stairway constructed so that he might be ensured against the possibility of encountering one of the women servants on the stairs. To avoid meeting his housekeeper face to face, it was his custom to leave money and a note of instructions for her at a designated place in the house. When, at a social gathering, an admiring guest from another land was introduced to Cavendish and gave him a somewhat effusive greeting, the poor fellow became so disconcerted that he suddenly darted out of the group, entered his carriage, and hurried home. Even on his deathbed, when he felt that the end was near, he sent the attending servant out of his sickroom so that he might die as he had lived, alone. Many twentieth-century psychiatrists would doubtless like to have known Henry Cavendish.

For an increased appreciation of the experimental genius of this remarkable recluse, one should read of later studies on the composition of the atmosphere. In his unidentified 0.83 percent residue, Cavendish had narrowly missed the discovery of argon, revealed by Ramsey about 110 years later to constitute 0.94 percent of the air by volume. It seems almost unnecessary to mention that such intimate glimpses of the life and character of a man are hardly to be obtained from purely scientific treatises or reports of investigations, however trüth-full and scholarly these may be.

These arguments may be summarized in the following opinion. In general, the best books for the undergraduate who seeks to acquire a background in the history and the philosophy of the physical sciences are not the classic writings *inviolati* of the ancients, nor those of the scientific fathers of the modern period. It is usually far better that the student make his acquaintance with the giants of the past through the biographies and commentaries by modern scholars who have carefully—and often lovingly—interpreted and evaluated the earlier work in the light of present knowledge.

THE books listed here are mainly of the type specified. Although some of them cannot be thus classified, but are more in the nature of critical surveys, not one of them is an unassayed classic. They are listed with little attention to chronology, but with some consideration for parallelism with collegiate courses being pursued and the maturity and attainments of the student. Thus the list is intended to follow, in a general way, the progress of the reader from freshman to senior status (a sort of *Reader's Progress*). The last two books are of a rather highly scientific content and are intended to satisfy the needs of readers who have a particular bent toward scholarship in the physical sciences. Probably they could not be read by every student.

Perhaps one should interpose a remark about the inclusion of books concerned with mathematics. Although it may be true that a pure-blooded mathematician is never so happy as when he does not know what he is talking *about,* and although the faithful worshipers of the Goddess of Mathematics insist that she loses some of her pristine beauty when prostituted to the mundane service of physics or chemistry, nevertheless her protracted intimacy with physics has been so fruitful and is now so universally and shamelessly recognized that I have not hesitated to name a few titles on mathematics in a list of useful works on the physical sciences.

Although I have been quite opinionated in defining the type of book which should be included in such a list, it must be emphasized that in offering the following reading list no claim to perfection or completeness is presented. Other, equally effective, lists could be assembled.

Discovery of the Elements. M. E. WEEKS. *J. Chem. Ed.*, 1939.
Simple, interesting, well illustrated.

Out of the Test Tube. HARRY N. HOLMES. (4th ed.) Emerson, 1943.
Vigorous, graphic accounts of the drama in research and of many of the outstanding contributions of chemistry to modern life. Good companion for Paul de Kruif's *Microbe Hunters* and *Seven Iron Men.*

Creative Chemistry. E. E. SLOSSON. Century, 1920.
Although nearly thirty years old, this is still a fascinating and instructive story of great achievements of the young American chemical industry.

Torch and Crucible. SIDNEY J. FRENCH. Oxford Univ. Press, 1941.
Much the best account of the life and times of Lavoisier known to me. In addition to the excellent record of scientific and biographic facts, there is also a delightful portrayal of the social mores of the period and of Revolutionary politics.

Lives of Men of Letters and Science in the Time of George III. LORD BROUGHAM. Carey & Hart, 1845. (Out of print.)
It is regrettable that this near-contemporary evaluation of a number of outstanding figures in a brilliant period is now so long out of print that it is rare, even in large libraries. Because I have not seen a copy of the book in many years, I cannot recall all its content; but it does give interesting stories of Lavoisier and Priestley, and the best glimpse of Cavendish known to me. One of the best things in the book is the sketch of J. J. Rousseau. I should be happy to see this book reprinted.

Life of Pasteur. R. VALLERY-RADOT. Doubleday, Page, 1923.
Factual, quietly inspiring, this is the best of the Pasteur biographies. If anyone should ask why such a book is in the list, let him remember that Pasteur was primarily a chemist.

Romance of Leonardo da Vinci. DMITRI MEREJKOWSKI. Heritage, 1938.
One of the most rewarding books I ever read. Leonardo's scientific side receives just treatment, along with the artistic. There is a fascinating inquiry into the working of the great mind itself. One of the richest treats is the panorama of Renaissance life that is provided. The conflict between the light of doubt and the shadow of authority is strikingly revealed. There are alchemy and witchcraft; ascetic religious zeal and profound hypocrisy; the glory, the magnanimity, the vices, and the revolting intrigues of petty princes. The pages glow with the personalities of Savonarola, Machiavelli, Raphael, Michelangelo—even the unbelievable Borgias. Magnificently illustrated with sketches and paintings by the incomparable Leonardo himself.

De Re Metallica. GEORGIUS AGRICOLA, 1556. (Translated and profusely annotated by H. C. and L. H. Hoover.) London: *The Mining Magazine,* 1912.
A rare instance in which the translators of an early Latin technical work possessed the understanding necessary to a satisfactory rendition in a modern language. In using Latin, Agricola undertook to record the most advanced chemical-metallurgical technology of his day in a language that had undergone no growth for some ten centuries, and thereby he achieved an obscurity that magnified the task of translation. But Mr. Hoover, an able engineer, succeeded in avoiding the blunders of meaning which nontechnical translators have committed. Moreover, his generous notes (which, in aggregate, must exceed the length of Agricola's text) constitute an authoritative running commentary. Copies of this marvelous book were never abundant, but it is to be found in the better libraries. It would be a splendid service to English-reading students everywhere if someone would make this unique work available in a new edition.

A History of Science, Technology and Philosophy in the 16th and 17th Centuries. A. WOLF. Macmillan, 1935.
A comprehensive work of great breadth and detail, yet sufficiently critical that the young reader need never flounder for the thread of truth. Arranged according to topic, it treats an imposing array of subjects: from astronomy and chemistry to mineral technology, the social sciences, and philosophy. In time, it reaches from da Vinci and Galilei to Leibnitz and Halley. Well illustrated and documented.

A History of Science, Technology, and Philosophy in the 18th Century. A. WOLF. Allen & Unwin, 1938.
A continuation of the above book, built upon the

same plan, and almost equally well done. The great developments in the fundamental sciences and in philosophy, during what the author likes to call "The Age of Enlightenment," are fully and carefully recorded. In keeping with the inventive growth of the period, there is an expanded and vitalized section on mechanical technology. The documentation and illustration of these books leave hardly anything to be desired.

Men of Mathematics. E. T. Bell. Simon & Schuster, 1940.

Biographical and critical essays on the giants of mathematics.

Science Remakes our World. James Stokley. Ives Washburn, 1942.

Concerned with technological adaptations of recent advances in chemistry and physics. Chemurgy, explosives, metallurgy, electronics, and illumination are emphasized. An introduction to the physics of the atom leads the reader to the threshold of a revelation of the fission bomb. Illustrated with photographs.

Foibles and Fallacies of Science. D. W. Hering. Van Nostrand, 1924.

A lively and thought-provoking book that will seem timely for a long while, since the man in the street is very slow to relinquish his well-established fallacies and superstitions. Even the advanced student will occasionally discover one of these of which he has not been entirely purged.

Introduction to Modern Physics. (1st ed.) F. K. Richtmyer. McGraw-Hill, 1928. Chaps. I-III.

A thoroughly enjoyable historical survey of the major developments in physics which led to the emergence of what is known as "modern physics."

The Autobiography of Science. F. R. Moulton and Justus J. Schifferes. Doubleday, Doran, 1945.

History of scientific progress, told by means of excerpts from the writings of many of the ablest thinkers of the past and present, each selection being prefaced with a helpful explanatory note—often biographical—by the editors. Subjects are rather well balanced, the physical sciences receiving an important share of attention.

The Search for Truth. E. T. Bell. Williams & Wilkins, 1934.

An uninhibited examination of the bases of philosophic thought, with especial attention to mathematics. Critical, occasionally cynical; brilliant, provocative, possibly irritating. Nothing is sacred except truth—and it's hard to tell what that is. Worthy of repeated readings. Out of print, but common in libraries; still frequently to be found at the better secondhand dealers. Surely there is no other book like it. Deserves a reprinting.

Mathematics and the Imagination. Kasner and Newman. Simon & Schuster, 1940.

A very modern presentation of some of the most fascinating topics in mathematics, including mathematical puzzles, chance, and a variety of geometries. It is pleasantly, even vivaciously, written. Stimulating to the imagination; delightfully instructive. By the time the authors are through with him, the reader should have been joyously cleansed of all traces of veneration for Euclid—and perhaps others.

Atomic Energy in Cosmic and Human Life. George Gamow. Cambridge Univ. Press or Macmillan, 1946.

The epidemic of books and articles on atomic energy that has scourged the English-reading world since Hiroshima can be forgiven and largely forgotten by the happy reader of this little book. Here, in 160 small pages, a mature authority on atomic physics has set down the physicochemical history (including the revelations of astrophysics), the present status, and the probable future of atomic energy. Professor Gamow's relaxed manner of treating the most complex topics is a reader's assured delight. The already lively discourse is further brightened by a number of half-whimsical drawings by the author. Regrettably, the spritely text is occasionally marred by rough grammar.

The Nature of the Physical World. A. S. Eddington. Cambridge Univ. Press, 1928.

When Professor Eddington wrote "world," he meant what the average literate American calls "universe." Although some have disagreed with Eddington's picture, few have written as lucidly about the organization of that world. No novel, this book should be read studiously. For him who will read it thus, it offers a rich reward.

The Nature of Thermodynamics. P. W. Bridgman. Cambridge Univ. Press, 1941.

This book is unique in that it deals with thermodynamics without the definitive aid of mathematics. Professor Bridgman, apparently aware that thermodynamic understanding is not, *ipso facto,* a possession of all who use thermodynamic equations, has sought to clarify the basic concepts and the fundamental reasoning of the science through the use of words only. The result is worth all the effort that must have gone into it. I am ready for my fourth reading of this ageless book.

THE APPLICATION OF SCIENTIFIC PRINCIPLES TO SCIENTIFIC PUBLICATIONS

ALBERT ELLIS

Dr. Ellis took his Ph.D. in psychology at Columbia University in 1947 and is now a consulting psychologist in New York City. His principal interests include clinical psychology and projective and nonprojective testing. He is working on a book to be entitled "The Nature and Nurture of Human Love."

THE main point of this paper may be expressed in the form of a simple question: When will scientific principles be applied to the publication of scientific articles?

A cursory glance at almost any unabridged dictionary will show that the term *scientific* goes hand in hand with such adjectives as *systematized, classified,* and *exact.* In the areas of social as well as natural science, those who wish to obtain the respect of their co-workers and associates always insist on the use of *standardized* instruments and procedures. All, it would seem, except the editors of scientific publications, who, in the most unsystematic, inexact, and unstandardized manner, blithely continue to turn out their products in a bewilderingly diverse array of almost all possible shapes, forms, sizes, types, and contents.

Take, for example, the important matter of bibliography. Assume that Richard Scientist, a reputable member of the A.A.A.S., has spent some time gathering data for an article to be entitled, let us say, "The Psychobiological and Psychophysical Organization of Social Groups." He has done a thorough job of background research for his article and has enough material for about forty pages of typewritten manuscript; in addition, he has a bibliography of some two hundred items to put into shape for publication.

Since there are a great many scientific periodicals—including those in the field of physics, biology, psychology, sociology, medicine, anthropology, and education—in which Dr. Scientist might have his paper published, he has not quite decided to which one he will submit it. He realizes, moreover, that the journal to which it is first submitted may reject it, even though it is a worthy article, for a number of reasons: because, for example, it may be too lengthy, or of not quite sufficient interest to the publication's specialized readers, or too similar to some other recent inclusions in this publication. Also, he may himself not wish to publish it in a certain periodical if he discovers, from communication with its editor, that its publication time lag is considerable.

Richard Scientist's problem, therefore, is this: In what *form* shall he have his paper typewritten, in view of the fact that it may go the rounds of several scientific publications before it is finally printed? For, although you might think that scientific periodicals, at least, would adhere to a standardized manuscript form, this is far from being the true situation. Thus, magazine A may require that all bibliographical items be alphabetized and consecutively numbered in the manuscript; magazine B, that they be alphabetized but not numbered; magazine C, that they be numbered but not alphabetized. Again, periodical A may insist that bibliographical references be noted in the text of the manuscript by parentheses, like this: (13); whereas periodical B may insist on superscripts, thus:[13] Furthermore, publication A may require bibliographic citations to be collected and typed at the end of the article; publication B may ask that they be typed at the bottom of each page of manuscript, or in the body of the manuscript immediately after the reference is made.

This means, of course, that if Richard Scientist first has the manuscript of his article typed so that the bibliographical citations are made in the manner approved by, say, periodical A, and he eventually has his paper accepted for publication by, say, periodical C, the chances are excellent that the editor of C, on accepting the paper, will return it to Dr. Scientist with a notation to the effect that, before it may be sent to the printer, the manuscript must be completely retyped. Consequently, unless Dr. Scientist is employed by some institution that generously accords him free secretarial service, he must expend considerable time, effort, and possibly money in order to revise his typescript.

Remember, moreover, that Dr. Scientist has a bibliography of some two hundred items for his paper. This alone will normally require at least fifteen typewritten pages. Let us assume that he has decided, hoping for the best, on a given manner of referring to his bibliography: by, say, alphabetizing and numbering it. After the main body of the manuscript has been typed, he now has reached the point of appending the alphabetized and numbered bibliography. But in what *form* shall this bibliography be presented? Periodical A, perhaps, insists on the full name of all authors; periodical B wants only their initials. Publication C requires, for article references, the month and year of publication; publication D requires only the year. Periodical X insists on the complete pagination of articles used in the bibliography; periodical Y insists only on mention of the first page of the articles; periodical Z wants no mention of pagination. Publication A desires the volume number of references put in Roman letters; publication B wants Arabic numerals; publication C dispenses with volume numbers entirely. Periodical R insists that the sequence of citation be date, volume number, pagination; periodical S insists on volume number, pagination, and date sequence; periodical T requires volume number, date, and pagination. Magazine A demands commas as separation points between date, volume, and pagination citations; magazine B requires semicolons; magazine C wants colons; magazine D asks for parentheses. And so on and so forth.

IT MAY be thought that this pointing up of the heterogeneity of bibliographical format in scientific publications is a gross exaggeration of the existing conditions. It may be felt that occasionally, perhaps, some periodicals vary from the one mode of bibliographical format usually prevalent, but that such variation occurs so infrequently as to make this matter hardly worthy of serious consideration. The facts, however, are quite otherwise. To illustrate them empirically, let us suppose that reference No. 15 in Dr. Scientist's article consists of a paper by John Doe on "Man and Life," published in the July 1947 issue of *Scientia* magazine, Volume 3, No. 1, pages 2 to 9. Cursory reference to publications at hand in my personal library shows that, among some thirty-seven scientific publications that might possibly accept Dr. Scientist's article for publication, there are no less than twenty-nine different ways of citing this bibliographical material. Space limitations preclude the listing of all these differing bibliographical models, but here, by way of illustration, are a few of them:

DOE, JOHN. 1947. Man and Life. Scientia, vol. 3, pp. 2–9.
15. Doe, J.; *Scientia;* 1947; 3:2.
J. Doe, "Man and Life," *Scientia,* 1947, 3, 2–9.
15. DOE, J. Man and life. *Scientia.,* 1947, **3,** 2–9.
John Doe, "Man and Life," *Scientia,* Vol. 3, No. 1 (July 1947), pp. 2–9.
15. DOE, JOHN.: *Man and Life.* Scientia., III, 1947, pp. 2–9.

Detailed examination of the thirty-seven publications showed that their approved bibliographical forms of citation differed widely in relation to spacing, alignment, punctuation, italicizing, typography, numeralization, parenthesizing, order of presentation, and inclusion of relevant material.

It must be emphasized that the twenty-nine observed variations on a bibliographical theme were not found after an exhaustive research of contemporary scientific publications. On the contrary, they were all secured from recent publications and reprints at hand. Further research could doubtless triple or quadruple the number of examples. The point, though, is not to find how many such variations actually exist, but to discover some quick and effective method of reducing their number.

Some periodicals—e.g., THE SCIENTIFIC

MONTHLY and the *American Sociological Review*—permit their contributors a certain amount of leeway so far as bibliographical notation is concerned. Thus, in the June 1947 issue of the latter publication, there are at least four different ways of stating similar bibliographical references. Obviously, the *Review* editor did not consider it worth while to insist that all contributors follow precisely the same citation pattern.

This kind of liberal policy toward bibliographical citation, although it unquestionably has certain advantages as far as individual authors are concerned, has outweighing disadvantages that should preclude its general acceptance for scientific presentation. In particular, when the form and content of bibliographical inclusions are left largely to the author's choice, a good many citations are bound to be made in which even the most salient information, including date, volume number, or pagination, are unfortunately omitted. Scientific editors should demand, at the very least, that there be no such omissions.

The question therefore remains: What, as scientists, are the editors of scientific journals going to do about the haphazard, unstandardized, and unscientific state of affairs that now exists in regard to bibliographic citation? The answer seems all too obvious: that, from the present look of things, they are going to do absolutely nothing. There appears to be so little connection, let alone coordination, among even those scientific editors whose publications go to identical or overlapping groups of readers that it seems almost utopian to expect the editors in all fields to organize for some effective action in relation to this problem.

Yet, with a lot of good will and only relatively little effort, help in this area could easily be forthcoming. For the problem is far from being insoluble. It has but two main aspects: first, deciding on what particular form (or perhaps handful of forms) of citation is most advantageous and least disadvantageous to both authors and publishers; and, second, the agreement of virtually all scientific editors to adopt this particular form(s) of citation and make it mandatory for all contributors.

Some of the important points to be considered in adopting a standardized form for bibliographic citation would probably be these:

1. The form adopted should be comprehensive enough to include all important bibliographic material; such as, in the case of citations from periodicals, date of publication, volume number, pagination, and perhaps issue number.

2. It should, at least where an article includes many bibliographical citations, provide for a separate listing at the end of the paper, because such a listing may (a) provide interesting additional reading material for the reader of the article; (b) make all the references in the article quickly accessible at one point; and (c) obviate the author's having to repeat, except by citing the reference number, a single source quoted several times in the same paper.

3. It should be arranged for easy typing. Thus, the text references to the bibliography may consist of parenthesized figures instead of superscripts; and all unnecessary uses of capitalization, italicizing, and bold-face type (which usually cannot be duplicated or are difficult to indicate on the typwriter) may be avoided.

4. The numbering of the references should be arranged in such a manner as to make it easy for the author to insert additional references in later drafts of his paper without his having to renumber the entire bibliography. (One method of doing this is to have the bibliography alphabetized but not numbered; and to have the reference numbers in the text, if any, consist of the date of the bibliographical source cited. Then new sources can easily be inserted into both the text and the bibliographical list at the end of the paper without unduly disturbing the material already typed or printed.)

There are doubtless other considerations than these to be taken into account before any final standardized bibliographical form of citation can be decided upon. But the point is that, through their common membership in an organization like the American Association for the Advancement of Science, or through other channels of communication, the scientific editors of this country (and other countries, of course) should get together as quickly as possible to discuss this matter, to decide for themselves which standardized bibliographical form they deem best, and then universally to adopt this form for *all* types of scientific publications. Not to do so will be to tolerate a haphazardly developed, multiclassificatory, and obviously unorganized method (or lack of method) that no group of scientists worthy of the name should for a moment consider perpetuating.

So MUCH for the matter of bibliographic standardization. But this, of course, is only one example of the present unscientific state of affairs that exists in relation to scientific periodicals. Equally as good a case could probably be made for, say, the matter of size of publications. For why, indeed, should scientific journals be printed in literally scores of different shapes, sizes, and general formats? In the first place, many scientists collect reprints from a wide variety of journals; and the inconvenience of handling and storing these is in no small measure a result of their diverse sizes. In the second place, individual scientists as well as librarians often have quite a problem finding shelf room for oversized, medium-sized, and undersized publications that should rightfully rest side by side. In the third place, certain large- and small-sized volumes are by no means easy to read, to handle, or to take on a journey. Why, then, all the variation in the size of scientific journals? If the commercial publishers of this country, who have never been accused of being particularly scientific or cooperative, can arrange to print nearly all their catalogues on paper of a uniform size, so that they may be conveniently bound in a yearly edition of the *Publishers Trade List Annual*, surely scientific editors should be able to arrange something equivalent. Perhaps all scientific journals cannot be printed in one optimum-sized form; but surely the number of different sizes could be reduced to a handful.

Let us not belabor the point, however. From what has already been said, it should be obvious that *some* application of scientific principles to scientific publications is sorely needed at present. Exactly at which points this application is most needed, and precisely how it should be accomplished, is hardly for the present author—who is not an editor of any publication—to say. The subject, only the periphery of which has been touched in this article, is broad and highly complex. Every reader of scientific journals has a vital interest, as well as a definite stake, in it. But the ones most concerned are the present editors of the various scientific publications and the organizations that sometimes control the policies of these editors. It is up to these editors and these organizations, if they would adequately measure up to their obligations as scientists and as scientific publishers, to get together for purposes of discussing, planning, and acting to eradicate the existing disgraceful state of affairs: namely, that scientific principles seem to be last and least applied to those publications that ostensibly are trying to do most to further the cause of science.

BOOK REVIEWS

WORLD OF ILLUSION

Magic Shadows. Martin Quigley, Jr. 191 pp. Illus. $3.50. Georgetown Univ. Press. Washington. 1948.

READING this book must be a revelation and a humbling for anyone even remotely connected with motion pictures or, for that matter, for anyone who ever sees a motion picture. For nowhere through this fascinating adventure and success story are any of the familiar names connected with the art or industry, whichever you chose to call it. There is no Gable nor Peck nor Garbo nor Chaplin nor Mayer nor Selznick here. Indeed, with the exception of Edison, there isn't a single name that the layman has heard or that has ever been in lights or drawn a salary or made a gossip column. Yet Kircher and Plateau and Von Uchatius and the Langenheims are actually the gods of the machine who made these other people possible.

And Quigley has made them, in many instances, far more exciting than the shadow children who came after them and owe them their birth. He has traced the invention of motion pictures as far back as man has thought, even to the sun itself, and followed it forward in time through the perils and disappointments, the heartbreaks and elations, the mishaps and the lucky breaks, to the final consummation.

Nowhere does the interest slacken; one reads on and on, fascinated by the steps that seem so natural now, but were so uncertain then, steps that led to the perfection of an art and a machine that we all take perhaps too much for granted.

An immense amount of research must have gone into the book's creation; one has a sense of complete documentation. One also has a feeling of eminent fairness. The reader is sure that Mr. Quigley has carefully weighed every smallest claim of the least contributor to inclusion for credit toward the forward movement of the machine. And one knows that each piece of the jigsaw is in its proper place at last and the puzzle is completed, the picture whole.

This should in no sense be considered a source book or a technical one to be put aside to dip into for leisurely reading. It is an imperative must, not only to the student or the technician but to all readers of adventure stories—adventure into the courage of the human heart and the profundity of the human mind.

JESSE L. LASKY

RKO Studios
Hollywood, California

GENIUS

Whom the Gods Love. The Story of Evariste Galois. Leopold Infeld. ix + 323 pp. $3.50. Whittlesey House. New York. 1948.

THIS is the first full-length biography in English of Evariste Galois (1811–32), who, in the opinion of Sophus Lie, was one of the four men who determined the course of nineteenth-century mathematics, the others being Gauss, Cauchy, and Abel. Galois' mathematics is different in kind from that of his compeers. It minimizes formulas and calculation and deals directly with fundamental concepts.

Professor Infeld balances his account fairly between Galois' two passions, his love for mathematics and his hatred of tyranny. In both, Galois was frustrated. The brutish stupidity of his teachers and the callous obtuseness of academicians all but succeeded in depriving mathematics of one of its greatest glories. The treachery of the police and of friends tricked him into the duel over some brainless nonentity in which he was killed at the age of twenty. All this is simply and movingly elaborated by Professor Infeld. He has one advantage over some of Galois' previous biographers: he is not French, and therefore is not in dishonor bound to wield the whitewash brush.

The book should be required reading for all teachers, academicians, and would-be fosterers of loyalty. There is a sardonic irony in the jest that the reactionary Bourbons of the 1830s, whose crowd did Galois in, should have shed their name on the party for which Galois died. He was a Democrat.

But, as Liouville exclaimed a hundred years ago, "Galois is no more! Let us beware of useless criticisms and look for the merits. . . ." Indeed yes. If Galois were with us today no doubt he would find all teachers competent and sympathetic and all academicians enlightened, and no inquisitor would bedevil him with, "Are you a member of the Democratic Party?"

E. T. BELL

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IS MAN FREE?

Ideas Have Consequences. Richard M. Weaver. 190 pp. $2.75. Univ. of Chicago Press. Chicago. 1948.

THE thesis of this book is: The world is intelligible. Man is free. Those consequences of man's actions which we are today expiating are the product not of biological or other necessity, but of unintelligent choices. We see hecatombs of slaughter; entire nations desolated by war; half of mankind looking upon the other half as criminal. Western man has made an evil decision which has become the cause of other evil decisions.

The author is evidently a man of religious convictions, trained in metaphysics, which we used to call philosophy. He dislikes the specialism and "fragmentation" of modern science; he believes that "democracy" has replaced faith and authority and has tried to substitute equalitarianism for the gentleman of high ideals. For these reasons modern civilization has failed.

The author's development of his thesis runs as follows: There is a source of truth higher than man. The denial of everything transcending experience is the denial of truth. There is no knowledge at the level of sensation. Science, the careful study of nature, denies the transcendental and leads men to regard nature as a self-regulating mechanism. Religion, therefore, becomes ambiguous, and man, instead of being divine protagonist in a great drama, becomes merely a wealth-seeking, wealth-consuming animal.

Having asserted this thesis, Mr. Weaver, without pausing to prove it or to concede that it needs proof, launches into a devastating attack upon modern civilization:

> Man becomes a fatalist: his life is practiced without theory; his institutions crumble; wars must be fought, but immersion in matter unfits him to deal with problems of matter.
>
> In the Middle Ages the philosophic doctor was the possessor of the highest learning. He was succeeded by the Gentleman, the idealist. The American South cherished this ideal and gave it fresh strength.
>
> The world today has no use for a liberally educated class, because generalization has been abandoned for specialization.
>
> Comfort becomes a goal when distinctions of rank are abolished and privileges destroyed.

The author's chief wrath falls especially on democracy:

> Equality is a disorganizing concept.
>
> No man was ever created free and no two men were ever created equal.
>
> Democracy does not recognize worth but demands conformity. Democracies if logical should choose rulers by lot.
>
> Programs like the four freedoms codify error.

Having now laid down his thesis and having indicted modern civilization, especially democracy, Mr. Weaver turns to remedies:

> Consciously, Man has three levels of thought: immediate needs; general beliefs; a metaphysical dream of the world.
>
> Logic depends upon dream; reason depends on faith.
>
> Some source of authority must be found; the only source is knowledge and knowledge means distinction and hierarchy.
>
> Every attack upon religion is an attack upon mind.

Coming to particulars, the author says:

> The right of private property is the last right remaining. Private property is the last domain of privacy. We must have small properties, independent farms.
>
> The state is not responsible for the poor or the criminal.
>
> Woman will regain her superiority when she finds privacy in the home.

One reads a book like this with a certain amazement and impatience. The author tends to aphorism and brilliance of phrase; I have therefore quoted largely verbatim, but I still realize that phrases taken out of context may be misleading. Yet the main thesis is clear: Man has the power to control this world and has deliberately turned from truth to error and landed us in a mess. The less spectacular fact is that man assumes that his ideas and wishes have influence on the course of history, and human beings have usually worked on this assumption; but of course scientific proof of this assumption is impossible. Moreover, the bald statement that there is a source of truth higher than sensation, which man has deliberately ignored, simply goes beyond any known facts.

There was a time in this world when a single mind like that of Aristotle could comprehend most known knowledge; it is not the fault of the modern world, but rather its glory, that the vast accumulations of knowledge are such that no single mind dare assume it is master of them. The result, as Mr. Weaver reiterates, is unfortunate; this is no deliberate fault, but it is a situation to which we must give increasing thought. It will avail us nothing to toss away our hard-won knowledge and go off on a spree of unproven "Faith."

It is timely to rail at our assumptions of progress and power, but to regard the medieval gentleman or his successor, the Southern slaveholder, as better than the follower of modern democracy is nonsense. Our democracy is indeed pitiful in its accomplishment, but that is chiefly because we have not tried it, and not because human equality is not a defensible concept. The doctrine of equality does not deny authority nor hierarchy based on knowledge or moral character; but it does deny that the peasants of France belonged permanently in the gutter, just because some fool happened to be his father's son. Equality does not mean that slaves should rule Southern white men; but it does deny that a white man should own me body and soul just because he can spout Cicero.

When the author raves of the age of faith, the cathedral builders, and the complacent medieval doctors of philosophy, he forgets of what a small segment of the nation he is talking; he ignores the abject poverty, ignorance, and degradation of most Englishmen, Frenchmen, and Germans, and regards a handful of aristocrats as the whole nation. It is better that the majority of men should have food and shelter, even if they yet lack common sense. The doctrine of equality aims to release wasted ability, so that out of a far vaster sample of mankind than any previous age has used we may in time achieve leadership, reverence, and authority based on fact, and not on chance and exploitation.

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LAND OF THE SHOGUNS

A Scientist with Perry in Japan. Allan B. Cole, Ed. xxvi + 307 pp. $4.00. Univ. of North Carolina Press. Chapel Hill. 1947.

Dr. James Morrow was "Agriculturalist" on the Perry Expedition to Japan in 1853. This book is, with only a modicum of editing, the journal kept by Dr. Morrow from the time he left Washington until shortly after he turned homeward from Java. The first twenty-three pages of the Journal are devoted to the trip to the Far East; pages 24–90 deal with stops at Hong Kong, Canton, and other places in China; and pages 91–112 report observations in Okinawa (called great Lew Chew by Dr. Morrow). Eighteen weeks spent in Japan, particularly in Tokyo Bay near Yokohama, at Shimoda, and in the harbor of Hakodate, Hokkaido, are reported in pages 113 to 205. Forty-seven pages are devoted to the appendices of the Journal, being primarily lists of seeds, plants, agricultural implements, and articles of commerce collected and distributed in China, Okinawa, and Japan. These appendices might well have remained unpublished and reference made to their existence among Dr. Morrow's papers.

The Journal is of some interest in reporting observations on customs of the people and the agricultural practices of the places visited. Readers who have seen this part of the Orient during the past few years will be impressed with how little change has occurred in almost a century since the Perry

Expedition. Throughout the Journal, references are made to the different crops observed in each locality. Unfortunately, however, these crops are often not sufficiently well named to permit accurate identification. Few scientific names are used, and common names are often vague. This is particularly disappointing in the frequent references to trips on which flowers were collected, without identifying any of the flowers found.

It seems unfortunate that the editor did not take more liberties in editing the Journal. In his day-by-day account, Dr. Morrow reported such prosaic tasks as pressing flowers that were collected or changing flowers put in press at an earlier date. These are necessary parts of a collecting trip, but they hardly add to the interest or value of the book.

This book seems to be worth while in placing in readily available form some additional facts about the Perry Expedition and some observations on Oriental culture, customs, and agriculture of that period. It is doubtful whether it will be of much interest to botanists, agriculturists, or general readers.

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UP FROM SLAVERY

Liberia. Charles Morrow Wilson. 226 pp. $3.75. Wm. Sloane Associates. New York. 1947.

THIS volume was appropriately timed to appear in the centennial year of Liberia as an independent nation. In a perusal of the book one is reminded of the many unique features in the long history of this African republic. As early as 1773 Samuel Hopkins of Boston advocated the creation of a colony of freed slaves in Africa by recruitment from America. In 1781 President Jefferson strongly seconded the movement. The project had advanced far enough by 1822 for the establishment of a "commonwealth" under the guidance of Jehudi Ashmun, a sort of crusading prophet. Meanwhile, some 1,500 freed slaves, descendants of captives taken from West Africa, had made the journey under appalling hardships, in which more than half of them perished of tropical fever. These hardy pioneers laid the foundation of Monrovia, their capital, which they named after President Monroe.

The task that lay ahead of this little handful of ex-slaves was that of organizing an unsurveyed territory of tropical jungle, inhabited by twenty-three or more independent tribes of natives, into a workable Negro republic.

Finally, the little group of determined Negro founders, mostly from America, in July 1847 proclaimed the birth of the republic of Liberia, with a constitution fashioned somewhat after that of the United States, but with suffrage and land ownership limited to persons of African descent. It was promptly recognized by several European nations, but the slave interests of the United States prevented American recognition of Liberia until 1862. Meanwhile, France and Great Britain seized about 44 percent of Liberia's original territory and annexed it to their adjacent colonies of Sierra Leone, French Guinea, and Ivory Coast. At last in 1910 the United States committed herself to the moral support of the young nation.

As clearly shown by Mr. Wilson, Liberia has, almost without outside help, rounded out a century of history without serious internal dissensions; has brought about the federation of twenty-three native tribes; established roads, schools, hospitals, and other governmental institutions; and is now operating on a balanced budget with a national debt of only fifty cents per capita, an accomplishment the rest of the world might well envy.

Moreover, the unification of native tribes into a federated republic has not interfered with the very efficient traditional system of tribal control of local affairs. All such matters are handled by frequent community "palavers" in each village, palaver being the term applied to the strictly democratic town meetings throughout Liberia and elsewhere in Africa.

Even the vast development of the rubber industry by the Firestone Company, involving, as it does, more than half the cultivated land of Liberia, as elaborately described by Mr. Wilson, has not unbalanced the economy of Liberia to the extent similar enterprises have done in other colonial countries. Employing 30,000 native laborers and producing 45,000,000 pounds of rubber yearly, this enterprise provides eleven of the twelve million dollars of exports annually.

Satisfactory progress is in evidence in educational opportunities, agricultural research, road building, and sanitation. The American Foundation for Tropical Medicine, with the cooperation of several American universities and the U. S. Public Health Service, has established The Liberia Institute for the study of the tropical diseases of man and animals. This is to be an international research center of outstanding importance.

Mr. Wilson has presented an excellent summation of Liberia's achievements on her hundredth birthday.

E. V. Wilcox

Washington, D. C.

RESTLESS SPIRIT

Maya Explorer. Victor Wolfgang von Hagen. xviii + 324 pp. Illus. $5.00. Univ. of Oklahoma Press. Norman. 1947.

IN THE 1840s a young New York lawyer, driven to the Guatemalan jungle by a restless and burning curiosity, discovered and unlocked a door that led deep into America's past. Now, a hundred years later, Victor von Hagen has drawn the first full-length portrait of this remarkable man in a biography that has authority and real narrative charm.

When John Lloyd Stephens finished the legal education supplied by his prosperous merchant father, he looked around—not for clients, but for somewhere to go. This was in a pattern that was to remain for all his forty-six years. The nation was young, and he tasted adventure first in a trip down the Ohio to the Mississippi. Then, his health weakened in the political campaign of 1834 (he was a Jacksonian Democrat and "a good talker"), he went abroad for a rest—and stayed two years. In Eastern Europe and the Holy Land he gathered impressions for two books, which later established him as "The American Traveller." In Egypt he acquired the passionate interest in ancient monuments that dominated his life. And in London he met a struggling artist and architect, Frederick Catherwood. Together they pored over the fanciful plates of Del Rio's *Ruins near Palenque.* Together they resolved to see for themselves, and from that compact grew the first and most fruitful collaboration in American archeology.

With bounding energy and imagination Stephens put his plan to work. Back home he talked about his travels with one of the Harper brothers, who suggested he "dish up something," whereupon he crammed his European experiences into two books that became best sellers. He got himself appointed by President Van Buren as special envoy to revolution-torn Central America. Then he sent for Catherwood, and they sailed south.

Stephens didn't find a government with which he could treat. But he did find a lost civilization in the jungles of Middle America. He rediscovered the cities of stone which the Mayas had erected centuries before Columbus—Copan, Palenque, Uxmal, Chichen Itza. With remarkable intuition he realized that these magnificent temples and pyramids fashioned by the ancestors of his impassive native guides were all related remnants of one great culture, a culture quite apart from the Old World and uniquely American. Through mud and jungle brush he drove on, with mosquitoes and malaria as constant companions, while Catherwood made the myriad meticulous drawings that are models even today. When Stephens published his *Incidents of Travel in Central America* in 1841, it was an instant and international success. But, more, it marked the beginning of the scientific study of the Maya.

The romantic quality of Stephens' life and the importance of his contribution are the two themes of von Hagen's book. Both are richly developed and blended against a background of the political and cultural currents of the period. Stephens has been, until now, almost completely neglected, and the author has done some very industrious picking of literary bones in order to present a rounded picture of his central figure. Familiar personalities of the 1840s cross the pages—Prescott, of course, and Melville and Poe, Mme Calderon de la Barca and Philip Hone. The volume is richly and aptly illustrated with reproductions of Catherwood's on-the-scene drawings. A useful bibliography is included, and Maya glyphs as chapter headings add distinction to the handsome typographic design by Will Ransom, of Oklahoma. Victor von Hagen has written a useful and entertaining book. Any lover of the Maya (such as your reviewer) will warmly welcome it. Any others will find it an admirable romantic introduction.

Ben Grauer
National Broadcasting Company
New York, New York

ONE MAN'S OPINION

Doctor Freud. Emil Ludwig. 317 pp. $3.00. Hellman, Williams. New York. 1947.

THIS sketch of the life, work, and influence of Sigmund Freud is in glaring violation of the ancient adage *de mortuis nil nisi bonum.* Alleging Freudianism to be of baneful effect upon human sanity and morals, the author launches here a one-man crusade of derision and vilification against the man who fathered psychoanalysis and is generally credited with revolutionizing psychiatric thought of our day.

Four indictments are straightway brought: that Freud erroneously confuses the motivation of abnormal behavior with that of normal behavior; that Freud explains "all neuroses as repressed sexuality"; that Freud endangers men and women by his teachings through the power of suggestion; that Freud has all art, culture, and religion originate in these same "suppressed desires."

In undocumented proof of these accusations an *argumentum ad ridiculum* is made out of fragments and inconsistent statements culled from the writings of Freud which bear on sex symbolism, dream interpretation, free association, sexuality of children, Oedipus complex, penis envy, transfer-therapy, etc. In addition, Freud is represented as having ceased to be scientific in order to become a seducer of all manner of men, particularly the fairer sex and sex-starved Americans, from the day he seized upon the notion of psychoanalysis. Considerable space is allotted to the perverted sexual motivations ascribed by Freud or members of his firm to well-known characters: Napoleon, Bismarck, Leonardo da Vinci, Goethe, Homer, Hamlet, and Moses. Special attention is drawn to psychoanalytic derivations of art, morality, and religion from *intèr faeces et urinam.* Finally, the personal life of Freud is invaded, and the master psychoanalyst is himself analyzed as inordinately obsessed with craving for power and recognition, a stranger to nature, to music, to children, to play, to love-emotion, to the world outside his own, and ultimately to religious faith. About the only thing that the author sees to the credit of Freud is a "tremendous imagination."

This work by Ludwig falls far short of the merit of his other biographical studies. As a first effort at satire, it possesses commendable features. As a funny book, it brings down the house. As an erotic book, it is far more appealing than any tome of Freud. Hardly convincing is the sincerity of the author's protest à la Comstock.

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ASTRONOMICAL HISTORY

The Life and Times of Tycho Brahe. John Allyne Gade. xii + 209 pp. Illus. $3.50. Princeton Univ. Press for the American-Scandinavian Foundation. Princeton, N. J. 1947.

TYCHO BRAHE was to Danish science in the latter part of the sixteenth century what George E. Hale was to American science in the first part of the twentieth century. In several respects the similarity is remarkable. Both men had wealth, love of travel, extraordinary activity in varied interests, wide acquaintance, and vast correspondence. Although not reared in scientific families, both early acquired a fervent love of science; as boys, both had generous allowances, which they spent largely for scientific apparatus. Both promoted astronomy by great skill and originality in devising instruments; and both were successful in obtaining from friendly sources funds for building the world's largest observatories.

Although genuinely devoted to science, Tycho unfortunately lacked the tact, modesty, and unselfish consideration for others that won for Hale continued cooperation and support. He was self-centered and neglectful of duty to an extent that alienated his patrons and partially wrecked his career. A scientist, like anyone else, can suffer from egotism and poor judgment.

In spite of his faults, which are frankly discussed in Mr. Gade's book, Tycho was a strong character and one of the world's greatest astronomers. He invented and constructed new and powerful measuring instruments; he assiduously accumulated vast stores of accurate data concerning the positions of stars and planets; and he made possible Kepler's analysis of the orbits of the planets, which in turn enabled Newton to find the general law under which they move.

The book will make good reading for those interested in astronomy, in the personal characteristics of a great man, or in the customs of the sixteenth century.

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SO MANY A MILLION OF AGES

Men Out of Asia. Harold Sterling Gladwin. xviii + 390 pp. Illus. $4.00. Whittlesey House. New York. 1947.

SUPPOSE you claimed to be of the *Mayflower* lineage, and proudly hailed yourself "100 percent American" (good old Scotch-English-Irish ancestry!), and then some chap came along and said that the ship was filled with emigrants from Menangkaban, the Irrawaddy, Kamchatka, and a few fellow-travelers from the Kremlin for local color. Wouldn't you hang your genealogical head in chagrin? What price then your D.A.R. membership—you'd *never* get into Constitution Hall!

Well, Professor Gladwin does all this—and more—to the much pushed-around aborigines of North, Central, and South America. He soundly wallops several cherished and time-honored (not to say timeworn) ideas about the early Americans and their cultures: first, they aren't "100 percent Mongoloid," but are quite a polyglot bunch; second, their cultures were not conceived and delivered on this hemisphere, but instead were born in Asia, weaned crossing the Pacific and the Bering Strait, and were brought to adolescence over here; third, their civilizations, instead of being ancient, are relatively quite recent.

With a few crumbling skulls to go by, a broken pot here and a spear point there, a bit of historical evidence hither, and a most lively imagination thither and yon, Professor Gladwin weaves an intriguing pattern with the warp of space and the woof of time. (I promise you, you'll not stop reading once you start.)

Who *are* the American aborigines? Let the Professor tell you. It is possible that prior to 25,000 B.C. a few Negritos may have come in. But in 25,000 B.C. the treks began in earnest: (1) Australoid, 25,000—15,000 B.C.; (2) Negroid, 15,000—2,500 B.C.—the men of the Folsom culture; (3) Algonquin, 2,500—500 B.C.; (4) Eskimo, 500 B.C.; (5) Mongoloid, 300 B.C.; (6) Arawaks, 300 B.C.—A.D. 500. (That's really M.D., B.C.—Moving Day, B.C.)!

The Mongoloid migration, *ca.* 300 B.C., is Professor Gladwin's favorite. It was activated by the unrest in the Far, Middle, and Near East caused by the conquests of Alexander the Great. But he did more than start the migration: his followers, in the great "lost fleet," actually reached this hemisphere via a Pacific crossing, bringing fresh genes and a flock of cultural stimuli. As a result, aboriginal cultural traits in North America stem to China and Northwest Asia, while those in Central America and the Andean region stem to Polynesia, Melanesia, India, and the Near and Middle East.

Professor Gladwin is quite sure that some of his professional readers will be Doubting Toms. Such a one he labels P.D. (Phuddy Duddy). Please, Professor, I'm a P.H.D. (Pretty Honest Duddy). I like your book immensely. It is challenging and irritating (in the neurological sense of "stimulating").

Campbell Grant's line drawings will make the slick-paper "mags" look to their laurels. But really, I just can't see your Q.E.D. (Quite Easily Demonstrated). Instead, I must employ my own I.Q. (I Query!).

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HETEROSIS

The Hybrid-Corn Makers. A. Richard Crabb. ix + 199 pp. $3.00. Rutgers Univ. Press. New Brunswick, N. J. 1947.

THIS is a readable book. It portrays the relative value of the early work of East and Shull in great detail and evaluates more accurately the importance of the work of East than other writers have done. Crabb rightly credits Jones with important discoveries. These discoveries include the double cross plan, which made hybrid corn feasible, and an up-to-date statement of a Mendelian explanation of hybrid vigor. Certain technical discoveries are overemphasized or overlooked. Two important errors of omission or commission are the inference that early testing is a desirable procedure with all breeding material, and the failure to emphasize prediction methods, which played so large a part in the rapid selection of desirable double crosses.

Some will believe the work of a few large seed companies, and of certain of their leaders, is overemphasized, even though all will agree that these companies played an imimportant part in developing hybrid corn. Others may wish that the work of hundreds of smaller seed producers and the organizations to which they belong had received more emphasis. And still others will doubt that sufficient credit has been given to the research work of the U.S.D.A. and state stations, for it is generally recognized that most early inbreds used by corn seed producers were selected by these research agencies. Without doubt the story would have been more accurate scientifically if the work of many who played an important role in the program had been described in a somewhat less laudatory manner.

The reviewer may be perhaps pardoned for a personal correction. Selection in self-pollinated lines was carried out seriously in Minnesota from 1915 on, although on a somewhat modest basis. Administrative leaders supported the work from the start, although it was not clear just how the inbreds obtained could be used to advantage.

Considering the difficulties, the wealth of available information from so many unpublished sources, and the fact that corn breeding is so highly technical, it seems probable that there will be conflicting viewpoints regarding this latest addition to the hybrid-corn story.

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ILLUSTRIOUS PENNSYLVANIAN

Alexander Dallas Bache. Merle M. Odgers. vii + 233 pp. $2.75. Univ. of Pennsylvania Press. Philadelphia. 1947.

THIS biography of Alexander Dallas Bache, great-grandson of Benjamin Franklin, is one of a series of volumes covering the lives of noted Pennsylvanians. Bache's achievements as scientist, educator, and engineer, culminating in his service of some twenty-four years as the second superintendent of the U. S. Coast Survey, clearly warrant his place in this group.

The first chapter sketches the early period of Bache's life up to his appointment to the United States Military Academy at the age of fifteen. It contains various items of interest concerning his famous great-grandfather and other members of a large and, on the whole, exceptionally gifted family.

Bache's unusual attainments are brought out in the next three chapters, which treat of his graduation from West Point at the head of his class, his appointment and service as professor of natural philosophy and chemistry at the University of Pennsylvania (1828–36), and his election as president of Girard College on his thirtieth birthday, July 19, 1836.

Subsequent chapters cover a two-year trip abroad during which Bache collected material for a comprehensive report on European educational institutions; his service as head of the newly organized Central High School of Philadelphia (1839–42); and his research and published papers on chemistry, terrestrial magnetism, and other subjects.

The last half of the book is devoted to his services to the nation in various capacities, but mainly as superintendent of the U. S. Coast Survey. His able administration of this bureau and his success in advancing its work are described.

The book is not only a biography of an outstanding American, but is also an account of the period in which he lived. It presents an interesting picture of the status of education in America in the early nineteenth century and of the trends in scientific thought at that time. The author uses many quotations and extracts from various sources. The gem of period letter writing appearing on page 14 is one of these that should not be overlooked.

A brief foreword by Rear Admiral L. O. Colbert, present Director of the U. S. Coast and Geodetic Survey, admirably summarizes the high lights of Bache's career.

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THE SOCIAL ORDER

Family and Civilization. Carle C. Zimmerman. 829 pp. $6.00. Harper. New York. 1947.

IN AN introduction the author states as the aim of this book—

> to discuss the family in its relation to civilization with four standards or ideals as to what constitutes a perfect sociological analysis. First comes *criticism*. Are the previous theories of the family valid? . . . Second, what has been the previous *history* of the civilized family. . . . Third, there is *thoughtful* analysis. Can the family be examined from the same analytical point of view applied to social problems by the great fathers of thought, Socrates, Plato and Aristotle? . . . Fourth is *causal* examination. Is the family a cause of development and change in civilization, or is civilization a cause of variation in the family? (pp. 16–17.)

The author has achieved the ends here set forth with varying degrees of success. For criticism, his chapters on Schools of Family Sociology and The Evolutionary Theory register comprehensive disagreement with the methods and conclusions of contemporary investigators. In thoughtful analysis he does maintain the analytic point of view of the great fathers of thought, Socrates *et al.*, with few concessions to modern methodology. In causal examination he fails to reach any very clear-cut conclusions. The most significant parts of the book are those dealing with history. Here the author has drawn upon an extraordinarily thorough and comprehensive knowledge of the source material for family studies as presented in early epics, classical authors, and the writings of early fathers of the Church. As a result he has been able to give an excellent picture of formal family institutions as they existed in the heroic period of the Indo-European-speaking peoples at various periods in Greece and Rome, and in Western Europe through the Dark Ages up to the present time. Although he makes a few slight gestures toward the Islamic, Indian, and Chinese cultures, he generally excludes these from his discussions.

Within the Western European frame of reference, he finds three types of family organization emerging again and again. These are the trustee, the domestic, and the atomistic families. The trustee family is based on the concept of the family as a self-perpetuating corporation with extensive functions relating to group property and the protection and control of family members. Such families are inevitably in conflict with emergent political authority. The domestic family develops out of the trustee family through increasing individuation of the primary family groupings within the trustee aggregate or "is revived by governmental or religious sanctions from the atomistic type" (p. 130). The atomistic type is one in which the individual is as far as possible freed from family bonds and the state is regarded as an organization of individuals rather than family units.

The author finds significant parallels between our own recent history and the Greek and Roman developments, with their progression from trustee through domestic to atomistic families, with resultant collapse. He even finds the primitive trustee type of family still surviving among our Southern mountaineers, and the descriptions of these units is one of the most interesting sections of the book. Unfortunately, he has no concrete suggestions to offer on how the current degeneration of the family is to be halted.

RALPH LINTON

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WHERE THE HIGH WINDS BLOW

Cache Lake Country. John J. Rowlands. 272 pp. Illus. $3.50. Norton. New York. 1947.

ALTHOUGH the subtitle of *Cache Lake Country* is "Life in the North Woods," this fascinating book could with all propriety be called "How to Live in the North Woods." John J. Rowlands tells us little of himself, but a great deal of how and why he lives in the cold-water country.

Author Rowlands' cabin looks over Cache Lake, "protected by distance, mile after forgotten mile of woods and water, and it is still clean and clear and safe from civilization." More than thirty miles from the nearest settlement, his human neighbors are Henry B. Kane and Chief Tibeash, a Cree Indian. That these three men, whose cabins are a couple of miles apart, find extraordinary satisfaction in their environment is evident in every page of the book.

Cache Lake Country is in fact a manual for self-sufficient living, for it contains directions for obtaining shelter, living off the country, transportation, first aid, cooking, recipes, sled-dog training, all neatly indexed for ready reference. The book reads as interestingly as a series of letters from your brother.

Mr. Rowlands is a modern backwoodsman. He writes of balsam-bough beds, birchbark canoes, and snowshoes, but he also talks of mattresses, DDT bombs, and airplanes. Instructions on how to lift a pack or how to keep warm are balanced with writing like this: "When May comes to the north country it reminds me of a fawn walking out of the woods alone for the first time, wide-eyed and uncertain about what to do next."

Hank Kane's illustrations complement the text on nearly every page. These drawings often picture a practical point made by Rowlands and are always decorative. This is a handsome book of real use even to people who do not venture much beyond their own back yards.

Here are a former timber cruiser who tells how to avoid forest fires, an artist who hunts with a camera, an Indian who "made a speech to the bear, explaining that he would not take his life if he did not need his fur for warmth and the grease for his people." You will like these men.

ROBERT D. MCMILLEN

Farm Journal
Washington, D. C.

MATHEMATICS—AND BEYOND

One Two Three . . . Infinity. George Gamow. xi + 340 pp. Illus. $4.75. Viking. New York. 1947.

AMONG the so-called popular books on science, there are very few that will appear exciting to the scientist and both exciting and comprehensible to the layman. To say that Gamow's book has just this quality means to bestow on it the highest possible praise. Indeed, it deserves it.

One often finds that a reviewer regards Jeans' popular writings as the highest level of popularization, and that he judges other writings by their distance from Jeans' masterpieces. It is high time to debunk this legend. Let me say (and here I mean it as a compliment) that Gamow is very much unlike Jeans. Gamow does not mix science with metaphysics or theology; he does not frighten the reader because the stars are far and lonely and because the atoms are small. He neither produces metaphysical thrills nor talks as a high priest of learning; he does not analyze the mathematical abilities of the creator of the universe. All that Gamow does is to tell a fascinating story with grace, clarity, and wit.

The book starts with Mathematics; then follow chapters on Relativity, Atomic Physics, Biology, Astronomy.

Thus, it is a tale "of many things; of atoms, stars and nebulae, of entropy and genes."

Yet I am neither surprised nor impressed by the knowledge that the author shows. Knowledge can be easily enough collected from books. But I am impressed by the author's good taste in collecting the proper things, in putting them in the right perspective, so that they form an exciting and dramatic story, made even more so by the artistic simplicity of presentation.

If you read Gamow's book, you will find in it something much more than knowledge. You will find an understanding of the spirit of modern science; the realization of its achievements and failures; the great progress gained through blunderings and retreats (as in the theories about our solar system, for example). All this is done in a pleasant, unpretentious way with excellent humor, which only rarely seems slightly strained.

During my lifetime, I have read hundreds of popular books on subjects overlapping those in Gamow's book. Many of them seemed to me like rewritings of the same book. Yet I could not lay down Gamow's book until I read it through. I also liked very much the author's delightful drawings. Let us hope that the book will sell as well as Jeans' *Mysterious Universe,* of which it did not remind me.

LEOPOLD INFELD

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MAN VERSUS INSECT

Insects and Human Welfare. Charles T. Brues. xiii + 154 pp. $2.50. Harvard Univ. Press. Cambridge, Mass. 1947.

THE reappearance of this 1920 book in a well-modernized revision makes available to the general reader a very thoughtful summary of the present stage in the battle between one species—man—and 800,000 other species—the insects. The struggle is analyzed on four fronts: Insects and the Public Health, Insects and the Food Supply, Forest Insects, and Household Insects.

In the fifty years since insects were first suspected of carrying human diseases, no period has given such a sweeping opportunity for their spread as the recent war, when men and materials were shipped from hemisphere to hemisphere, from temperate civilizations to tropical jungles. Yet in reviewing the malarial fevers, yellow fever, filariasis, and others, Professor Brues is decidedly optimistic. He looks forward to no serious outbreak, but to a better understanding of diseases now known to be transmitted by insects and to new revelations that will make possible control of still other human ills.

The food front is not doing so well. The annual twenty-million-dollar loss in crops and stored harvests in the U.S.A. alone is a measure of the failure of even the best insecticides to cope with the problem, and an indication of the importance of long-range biological control through propagation of insect diseases and parasites, protection of predators, and of improved crop management practice to prevent development of epidemics.

Forest insects have an even freer hand, partly because infestations are harder to handle, partly because trees grow slowly and any money for protective measures is begrudged owing to a "deep-seated human aversion for rewards in the too-distant future." The sequence with which insects and fungus diseases attack and destroy trees is emphasized for the New England elms, where several European pests cooperate like workers on an assembly line.

Professor Brues compares man's homes and cities to an ant or termite nest, and shows how the various household insects fit into their niches. Some of them have troubles too. The fruit flies that hover around ripe fruit are alcohol lovers that may drown in an open flask of Kentucky corn whisky, "never having had experience with the more powerful condiments to which we ourselves have grown immune." House spiders and various wasps that annoy us turn out actually to be friends. He even points out a kinship between the pestiferous clothes moths and some whose larvae "feed on the fur of living animals, such as sloths, and about the base of horns of certain African ungulates." Perhaps the first fur coat presented by a cave man to his wife had moths in it from the very start! And the "bookworms," which include termites, the larvae of certain beetles, cockroaches, silverfish, and others, probably prefer "antique volumes or first editions . . . due to previous invasion by molds which enhance their food value." *Insects and Human Welfare* is interesting, informative, and thought-provoking without being technical. An index adds to the usability of the new edition.

LORUS J. and MARGERY J. MILNE
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SCIENCE AND SOCIETY

What Is Life? J. B. S. Haldane. x + 241 pp. $3.00. Boni & Gaer. New York. 1947.

Education for What Is Real. Earl C. Kelley. xiv + 114 pp. $2.00. Harper. New York. 1947.

THIS outstanding British biologist has stepped down from the Ivory Tower and presented for the lay reader, factually and clearly, what is actually known as to the complex life processes of man and other animals in health and in disease. Some of the sections of this volume had prior publication in the London and the New York *Daily Worker,* the World Union of Freethinkers, and in the Penguin Series. The four sections of the book have the following headings: Is Man a Machine? Heredity and Environment; Science and Society; Science for Society. The author is convinced that "people can and should think scientifically about themselves" (Preface). To do that we have to present our children with better education in human biology than that available, even in Great Britain and America, in the past fifty years. This book tries to remedy that deficiency through education of the adult generation of 1948. A careful study of it will aid in that direction. This reviewer is puzzled by one sentence in the Preface. Dr. Haldane tells us that "Science made him adopt Marxism as a working hypothesis about how men behave and how changes in nature and society occur." Science, yes, *real science,* antedates Karl Marx, and I find nothing of the Marxian political philosophy, or practice, in the author's presentation of the known facts in this book, unless such a meaning may be conveyed in this paragraph (Preface): "I want Americans to be healthy and happy not only because they are human beings but because the happier they are the less likely they will be to interfere with other peoples as we British have done in the past, with disastrous effects." In my opinion, science alone, philosophy alone (including Marxism), religion alone, has not yet made all men honest, sane, humane, and just. Period.

Few informed citizens will object to the title of Kelley's book: *Education should deal with the known, the real in man and nature.* But, in the judgment of this reviewer, the author deviates now and then from this fundamental theme. Vision, though important, is just one of our senses through which we perceive, learn, and understand. Nature does not always play the tricks with our visual sense perpetrated by the experimenters at the Hanover Institute. Some will put a question mark after the statement, "Individual purpose is basic to perception" (p. 103). As to average normal persons, there are at least as many and as important identities and similarities in body machinery, functions, and experience as there are unique differences. We do have "a common world," and "nature does repeat." We are told (p. 68) "when we make children learn that for which they see no need, it is doubtful if learning goes on at all." If the author means that a real teacher can or should make the child see the "need," I agree. But, as I understand man and other animals, conditioning and other forms of learning (and understanding) cannot be delayed or made entirely dependent on the infant or the young child understanding his personal and social needs in future years.

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HISTORICAL MEDLEY

400 Years of a Doctor's Life. George Rosen and Beate Caspari-Rosen. xvii + 429 pp. $5.00. Schuman. New York. 1947.

THIS book attempts to present, through autobiographic excerpts from more than eighty doctors, a composite picture of the medical man. The excerpts are arranged to represent a life-history sequence, with the following major headings (the number of excerpts for each section is given in parentheses): Early Years (11), School Days (8), The Medical Student (31), The Practice of Medicine (28), Scientist, Scholar, Teacher (22), The Doctor Marries (19), The Doctor as Patient (12), The Doctor Goes to War (15), Writing and Politics (6), and Reflections on Life and Death (9).

Most of the selections are introduced with a brief biographical characterization of the man, and since nearly every well-known doctor (and some not so well-known) from the sixteenth century on is represented, the total provides quite a potpourri of medical history —or better, a historical lagniappe.

The book should provide the established doctor with much pleasant browsing, the eager medical student with inspiration, and the patient—each of us at one time or another —with a frankly humanizing portrait of the man who often plays God.

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FIGHTER FOR AMERICA'S FORESTS

Breaking New Ground. Gifford Pinchot. xvii + 522 pp. $5.00. Harcourt, Brace. New York. 1947.

GIFFORD PINCHOT died on October 4, 1946, leaving the manuscript of this book. It is an important book because it treats authoritatively a subject of vital concern to every citizen—conservation of natural resources; no one else could have written it. Pinchot was the first professional forester in America, the organizer and first chief of the Forest Service, and coauthor with Theodore Roosevelt of the Conservation Movement in this country. His book, autobiographical in approach, is the story of Pinchot's career from 1889 to 1912—an eyewitness account of how practical forestry and conservation came to America.

Even as an octogenarian Pinchot is true to form and pulls no punches. His account of the struggle to get the control of the forest reserves transferred from the Department of the Interior to the Forest Service, of the famous Pinchot-Ballinger-Taft Controversy (which cost Pinchot his government job), and of the enemies of the Rooseveltian conservation policies is told fairly and squarely. It is rich in reminiscence, articulately appreciative of colleagues, critical but unvituperative. Although as a literary masterpiece the book leaves much to be desired and would have profited by further editing, it is nevertheless impellingly readable because of the vital nature of its message; for there is little doubt that the Conservation Movement, which Roosevelt and Pinchot sold to the country, was the most important public awakening in our history—and the fight is still going on. Pinchot's ideas on the subject of conservation as a basis for permanent peace were a projection of his conservation thinking, and it may be that we have not yet heard the last of them.

I am sorry that Gifford Pinchot did not live to complete his memoirs, to give us the story of the last thirty-five years of his colorful life. (After he was sixty he was twice governor of Pennsylvania.) But we must be thankful that he finished the most important segment, representing the part that must have given him the greatest satisfaction. He was a great personality and a great American; his book deserves a wide reading.

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SCIENCE ON THE MARCH

THE NUCLEAR CYCLE IN THE FUNGI

WITHIN the past two decades there has been a noticeable increase in the study of nuclear cytology and nuclear behavior in the fungi. This is due to a number of factors, including the recent discoveries of important fungus-produced antibiotics such as penicillin; the increasing importance of fermentation products of various fungi, particularly the yeasts; the ease with which the majority of fungi may be cultured and made to complete their life histories in a few days on simple agar media; and the unusual nuclear cycle through which they pass. This discussion is concerned primarily with the last-mentioned factor.

In most species of fungi, almost the entire life cycle is characterized by the presence of haploid nuclei in the plant body, whereas the diploid nuclei are restricted to one short phase —in fact, to one type of cell, the zygote—produced during the latter part of the life cycle. Obviously, this is in contrast to the condition in higher plants, where the diploid phase is more elaborately developed and the haploid phase is confined to the much-reduced gametophytes. Herein lies one of the unique advantages of the fungi in genetical work. They offer an unexcelled opportunity for the study of haploid nuclei and their effect upon the development of the organism. In essence it is possible in fungi to study the effects of one set of chromosomes upon the organism, instead of the two sets that are inevitably present in the diploid cells of the higher plant body. This is applicable also to the gametophytes of algae, mosses, and ferns; but these are generally more difficult to culture in the laboratory and have therefore not attained the popularity of fungi in genetical work.

The cytologist and geneticist have turned most of their attention to the study of the higher fungi (Ascomycetes and Basidiomycetes) rather than the lower fungi (Phycomycetes). This preference exists primarily because it is seldom possible, in the latter group, to analyze all the products of a single meiotic division, and the nuclei are generally too small to allow accurate observations of chromosomal behavior during nuclear division. In the higher fungi, however, the nuclei are frequently of a size that will allow more accurate cytological observation. This is particularly true of the diploid fusion nuclei, which in some species may attain the size of an average higher plant nucleus. Even more important to the geneticist is the fact that, in these higher fungi, all the products of a single meiosis may generally be recovered.

In Ascomycetes, nuclear fusion occurs in the young ascus (zygote) and is followed ordinarily by three successive divisions of the diploid nucleus, of which the first two divisions are reductional and segregational. Eight haploid nuclei are produced. By the process of free cell formation in the ascus, the 8 nuclei are enclosed in 8 separate cells—the ascospores. By analysis of the mycelia obtained from germination of these spores it is possible to obtain a considerable amount of information on what happened during meiosis, particularly with regard to segregation of such factors as those that influence sex, rate of growth, pigmentation, and substances produced in the medium. Factors controlling parasitism are also receiving increasing consideration.

In Basidiomycetes, nuclear fusion typically occurs in the young basidium (zygote) and meiosis follows. Here there are characteristically two divisions of the diploid nucleus, resulting in the production of 4 haploid nuclei, which pass separately into 4 different spores —the basidiospores—produced externally on the basidium. These 4 spores, containing the products of meiosis, may be analyzed in the same manner as the ascospores mentioned above.

In certain species of pink bread molds (*Neurospora*), in which 8 ascospores occur in a single series within the ascus, and in rusts and smuts with septate basidia, it is frequently possible to recover and analyze the products of meiosis in the exact order in which they were produced. In these cases it is possible for the investigator to determine where reduction and segregation occur and to obtain much valuable information on the crossing over of chromosomes.

When an ascospore or basidiospore germinates, it ordinarily produces a mycelium whose cells contain one to several nuclei, all of which are haploid and genetically alike. In many species, if the life cycle is to be completed, it is necessary for two compatible mycelia—that is, mycelia whose nuclei have a mutual attraction for each other—to be brought together before the life history can be completed. Such fungi have been termed *heterothallic*. At some point in their development there is an association of the two types of nuclei. This is often brought about through hyphal fusions or by a copulation of sex organs. Eventually, a different kind of hyphal growth appears, in which the compatible nuclei become associated in pairs, usually one pair in each cell. During cell division the two nuclei divide simultaneously in such a way that each new cell will contain a pair similar to the first. Such nuclei are known as *conjugate* nuclei, each pair also being frequently referred to as a *dikaryon*. They represent two genetically different sets of chromosomes in two separate but closely associated haploid nuclei. Eventually these dikaryotic hyphae give rise to asci or basidia in which the pairs of nuclei fuse to produce the diploid nuclei. This is followed by meiosis, as has already been pointed out.

Other species of fungi, termed *homothallic*, normally complete their life history when grown from a single haploid spore. Such a single-spore mycelium may give rise to dikaryotic hyphae as in heterothallic forms, and in the majority of species investigated there is a nuclear fusion. This has generally been considered an association and fusion of two genetically similar nuclei. The possible advantages of such a nuclear cycle are not so apparent. A similar condition exists among certain of the algae, mosses, and ferns.

In recent years it has been found that a number of fungi, ordinarily considered homothallic, may produce certain distinct strains which, when mated, will cross with one another. The parental types can be recovered after meiosis, proving that both parent strains contributed to the production of the fusion nuclei. Although the significance of this behavior is not fully understood, the recognition of it requires that we revise further our old concepts of homothallism and heterothallism.

In higher plants there is no phase equivalent to the dikaryophytic phase in the higher fungi. In mosses, ferns, and seed plants a similar condition would prevail if, at fertilization, the egg and sperm nuclei failed to fuse, but remained closely associated and underwent simultaneous divisions so that each cell of the sporophyte would contain two haploid nuclei. There has recently been considerable debate over whether the dikaryotic cells in fungi can be considered diploid. There are convincing arguments pro and con. Obviously, in the higher fungi only the ascus and the basidium are the exact equivalent of a diploid cell in higher plants. They are the only cells containing two sets of homologous chromosomes in the same nucleus. True dominance and recessiveness may be expressed here with respect to the factors controlling the development of the asci and basidia.

One of the chief questions raised lately is whether true dominance can be expressed in the dikaryotic phase and whether a dikaryon may have the same influence upon a cell as would a single diploid nucleus containing the same two sets of chromosomes. The evidence pertaining to this question is scanty, but one applicable case may be mentioned.

In wheat rust the uredospores contain a pair of haploid nuclei representing a dikaryon. The teliospore cells at first contain dikaryons, but there is an early nuclear fusion, so that each cell contains a single diploid nucleus. It has been shown that the color of the spore walls is controlled at a single locus on a chromosome. A dominant and a recessive gene have been found at this locus. It has been further demonstrated, through hybridization experiments, that when one or both of the dominant genes are present in either spore the wall is distinctly colored. Likewise, when two recessive genes are present in either spore, the wall is pale. Thus it appears that, at least in some cases, two homologous sets of chromosomes located in two different haploid nuclei in a cell may have the same influence upon certain characters as when the two sets of chromosomes are combined into a single diploid nucleus.

The above discussion has emphasized the fact that fungi in general are characterized by the presence of haploid nuclei throughout most of their life cycle. There are several noteworthy exceptions. There is some reason to believe that certain Phycomycetes may produce in a single life cycle both haploid and diploid thalli, but this is in need of confirmation. However, it has been genetically (and possibly cytologically) proved that in some yeasts, including brewers' and bakers' yeasts, the vegetative phase is normally diploid, each cell containing a diploid nucleus. It has been possible to establish true hybrids among these yeasts, some of which are superior to the parent types in their fermentative abilities. As would be expected, true dominance and recessiveness are expressed here as in higher plants.

The more critical cytological and genetical studies have shown that the nuclei of fungi behave essentially like those of higher plants during division. In both somatic mitosis and in meiosis, spindles develop and the chromosomes arrange themselves upon them, subsequently splitting apart and passing to the poles, as in higher plants and in most animals. In the fusion nucleus, synapsis of chromosomes occurs, followed by the other characteristic meiotic prophase stages found in higher organisms. Reduction in chromosome number is now believed to occur generally during the first division of the fusion nucleus, as is characteristic of higher organisms. Segregation may occur in both the first and second divisions. Chiasmata have been detected cytologically in some cases and crossovers have been proved genetically in a considerable number of fungi. In the pink bread mold, *Neurospora crassa,* the sex chromosome has been mapped out, with a number of loci plotted upon it. Evidence of gene mutation has been recorded for a number of fungi.

In view of the above considerations, it is obvious that the fungi have obtained a well-deserved recognition in providing a rich field for cytological and genetical investigation, the results of which have already contributed, and will continue to contribute, much to our knowledge of the structure and behavior of nuclei in general.

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ITINERARY*

FIRST ANNUAL INTERNATIONAL PHOTOGRAPHY-IN-SCIENCE SALON

BROWN UNIVERSITY and PROVIDENCE ENGINEERING SOCIETY Providence, Rhode Island	April 22–May 6
GAMMA SIGMA EPSILON University of Florida Gainesville	May 13–June 1
THE JOHN CRERAR LIBRARY Chicago, Illinois	June 8–June 21
AUBURN CAMERA CLUB Bureau of Animal Industry U.S.D.A. Auburn, Alabama	June 28–July 12
AMERICAN OSTEOPATHIC ASSOCIATION ANNUAL CONVENTION Boston, Massachusetts	July 19–23
ACADEMY OF SCIENCE Illinois Wesleyan University Bloomington	Otober 1–15

* Dates available for showing both 1947 and 1948 Salon pictures.

COMMENTS AND CRITICISMS

LAST WORD ON GARRETT

Mr. Garrett's article in your October 1947 issue has just been brought to my attention. The article implies that even after environmental influences have been equalized as much as possible, the intelligence of the Negro is still considerably lower than that of the white.

If the above be granted, however, one is forced to a conclusion of doubtful validity. There are many hereditary factors, such as shape of head, eye color, size of head, color blindness, and the like. No correlation has been found between any of these and intelligence. The only hereditary factor purported to show any correlation with intelligence is skin color. This would force the conclusion that the genes for intelligence and the genes for skin color are in some way linked, since otherwise there would be no reason for the alleged relationship. Inbreeding among people of the same skin color will not serve as an explanation unless it can be shown that mulattoes have a higher intelligence than Negroes; i.e., the mulattoes living in a Negro community must be compared with their neighbors in the same economic group. No such differences have been found to exist. By this *reductio ad absurdum* we are therefore thrown back to the hypothesis that intelligence differentials of this sort must be explained in terms of environmental factors.

HERBERT CAHN

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ONE MAN'S MEAT . . .

"Crumbling Rocks," by Janssen (SM, August 1947), has drawn forth more or less disparaging remarks from a couple of correspondents (SM, January 1948). I cannot resist the temptation to add a few remarks, not for the purpose of praising or of criticizing the content of Dr. Janssen's article, but to bring out what I believe to be a fact, namely, that too few of us possess enough tolerance toward the ideas of others on questions of publication policy. A. O. Kelly apparently would proscribe the publication in SM of any article made up largely of old stuff not comprising a step in the advancement of science. During the past 10–15 years I can remember reading in SM what for me were very enlightening articles on such subjects as the development of microscopy, of astronomy, of cosmological theory, and the theory of isostasy. In the journal of February 1948, Professor Hobbs contributed an article on "American and Eurasian Glaciers of the Past." Some of the ideas in this article date back to Hobbs' *Earth Features and Their Meaning*, published 36 years ago, and might therefore be called "old stuff" and "hardly a step in the advancement of science." However, the article in question tells of much growth in the comprehension of glacial theory during the past 30–40 years by filling in several vacuous spaces in the fund of information formerly at the command of the casual observer.

It is probably true that portions of many SM articles are old stuff to the specialist. Nevertheless, some readers will consider these to be contributions to the advancement of science, since they help to furnish an enlightening background for more general comprehension of trends in scientific investigation. If only articles equivalent to a Ph.D. dissertation were to be published in SM, the journal's appeal to the nonspecialist member would depreciate greatly, and, in consequence, the over-all contribution to the advancement of science would be slight. In the long run, scientific advance is related to the general philosophical attainments of humanity.

As a member of the Association, occupying a humble position in technology, I crave to be somewhat acquainted with the thoughts and ideals of those greater scientists who are shaping our conceptions of the universe in which we live. Let us remember that new generations of scientifically interested people are continually emerging, and that they, even as specialists, wish to know something about the historical aspects of other fields than their own. Therefore SM, as a journal of enlightenment, must continue to serve up a considerable amount of rehashed material.

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CLOTHO, LACHESIS, AND ATROPOS

Professor Leslie A. White's article on "Man's Control over Civilization: An Anthropocentric Illusion" (SM, March 1948) presents a thoroughly deterministic viewpoint. Admittedly having no free will, Professor White could have done no other than write precisely the article that he did. However, my own background being somewhat different, I find myself moving toward an argumentative response.

First, let us examine the consequences of belief or disbelief in free will in a deterministic world—not that we can do anything about it, but simply to see whether this isn't a case where the folly is in being wise. We may take as a starting point Professor White's article, which will have exerted an influence on all who have read it. We may suppose that some persons, who never previously questioned the freedom of the will, will have been started on a new line of thought leading eventually to complete acceptance of determinism. Others, not wishing to acknowledge that they have been in error, will have reacted in opposition. They will engage even more actively than before in exhortations to their fellow-men to make the world better. Where will all this lead?

Of course, a deterministic world can change only to the extent to which it was bound to change anyway. Professor White was already a part of the system. He was bound to write his article, and it was bound to set in play the kinds of forces that will be described here. What is being considered is simply how the results will be different from what they would have been had some *deus ex machina* been able to intervene and snatch the pen from the professor's hand.

Obviously, the people who have been influenced to become complete determinists will hereafter be more inclined toward snap judgments and ill-considered actions; for, inasmuch as directed thinking is hard work, the determinists, after having turned over one or two possibilities in their minds, will suddenly and thankfully recollect that what will be, will be. They will then cease to pommel their brains. They will also shirk disagreeable physical activities more than ever before. As soon as a man finds that he can drop his hoe and lie in the shade he will do so, knowing that he could do no other. Except in cases of considerable need, there will be little striving against adversity on the part of these thoroughgoing determinists. Their philosophy will give them no stomach for it. They should not be blamed, for they actually have no free will. Nevertheless, their last state will be worse than their first.

But what of the people who react in opposition and set out to prove the author wrong? I have high hopes for some of those who found themselves called "educators" (complete with quotation marks). By way of protest they will work more actively than before for a better world. It will be purely a matter of stimulus and response—but what a response!

The now more ardent educators will work still harder and achieve greater influence. If they are really great educators with great messages they *will* succeed as never before in improving their civilization. What matters it if they preach free will when there is none? If they convince their hearers, these too will see no inconsistency in striving for a better world. Only through such endeavor will civilization actually improve, even in a deterministic world. The small results from some of the efforts at improvement which Professor White mentions are not evidence against the possibility of more marked improvements when the leavening agents are more powerful.

It seems to me, then, that to the extent to which people disbelieve in free will, the effect will be undesirable, and to the extent to which they believe, the effect will be good. The argument does not presuppose that there *is* free will. It assumes the opposite and examines simply the effect of a belief or disbelief in free will in a world in which there is actually none. It may be deduced, however, that the effects will be similar if there is actually freedom of will. The essence of the matter is that "faith inspires to effort, while doubt freezes the soul and paralyzes the arm. Men who do not believe do not achieve." Willard C. Selleck, who used these words in another connection, prefixed the word "intelligent." However, even though it may not be intelligent to believe in free will, the effect will still be good. If this be scientific heresy, let us make the most of it. We'd better!

I intend no disrespect to Professor White. He occupies what many regard as a very strong logical position. However, the complete denial of free will is one philosophical doctrine that does not seem to wear well in the world of practical affairs. Professor White has had the fortitude to venture out there with it, and he appears to have handled it consistently—a difficult task. Consistent application of out-and-out determinism in the world of man is deserving of some admiration, purely as a mental accomplishment. Sir James Jeans, who in spite of Professor White's quotation, admitted the force of the arguments against free will, wrote of the "characteristic irrationality" of the thoroughgoing determinist. Also, Esmé Wingfield-Stratford said that "if you turn out free will by the front door, there is nothing for it but to leave the back door quietly ajar, and let mum be the word."

It will have become evident that I believe that our wills do have some measure of freedom. Neither the argument from science nor the argument from pure logic seems to me to be profound enough to rule this out. I am encouraged by knowing that there are some eminent and highly accomplished scientists of like mind. But even in cases where close reasoning does seem to leave no loophole, it is well to remember that there have been logical paradoxes before now, some of which have been resolved in favor of the common-sense view. Zeno seemed to have an airtight case against the possibility of Achilles getting past the tortoise. I confess that the argument still fascinates me with its apparent conclusiveness. Nevertheless, it is evident, now, that Zeno got into a blind alley by adopting an approach that did not permit him to apply his reflections to a sufficient length of time. We have Bertrand Russell's word for it that the paradox is now completely resolved, with the race going to the swift, barring the contingencies of Ecclesiastes 9 : 11.

In subscribing to the freedom of the will, I place considerable confidence in what might be called "the argument from the existence of consciousness." Consciousness is a unique and most extraordinary manifestation, the very existence of which constitutes a striking anomaly in any deterministic world. As Arthur Compton says, "We find that among the higher animals the course of evolution has brought consciousness to an ever higher level of development. Why should this occur if consciousness were of no value to the life of the animal, and how could it be of value to the animal if it were incapable of affecting its course of action?"

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TECHNOLOGICAL NOTES

Men and fish. Something about an island is cozy and snug. You know exactly what the limits of territory are, without choosing arbitrary boundaries. That definiteness of subject appears in a 140-page study called *South Bass Island and Islanders,* by Thomas Huxley Langlois and Marina Holmes Langlois. He is director of the Franz Theodore Stone Laboratory, of The Ohio State University, at Put-In-Bay, on South Bass Island in Lake Erie. She is Mrs. Langlois.

Dr. Langlois is a limnologist, and his professional interest is reflected in the approach to the social and economic relationships of the 273 human beings on the island. The Foreword is mostly quotation from a bulletin on the lives and habits of a colony of bass in a rearing pond where "The most desirable crop consists of the maximum number of individuals with the maximum mass weight." The parallel isn't exact; the way to prevent cannibalism in the fish is to provide plenty of food, and presumably lean years in the island economy would not lead the islanders to prey on each other.

Indeed, as the study shows, members of the human colony work together. This symbiosis produces a far higher return than the sum of individual efforts, no matter how energetic. The workings of the division of labor show up sharply in the various industries—real estate, wine, fishing, innkeeping, and transportation—and the union for protection is evident in the group decisions about education and religion, in the arrangements for sanitation and health, and in the organization for fire protection. For example, getting and keeping a doctor required community action when such behavior was unusual; since about 1880 the islanders have furnished an office-home for the physician. The post has its hardships; three of the sixteen doctors who have practiced on the island have drowned in Lake Erie.

South Bass Island was settled in 1811. Since then the industries have developed and fluctuated. Cutting and shipping ice had a boom at the time of the great ice famine in the early nineties, and then disappeared. Vineyards require the growing of a certain amount of rye, to tie up the vines. Fishing is not what it used to be, and hotel keeping, which flourished early in the present century, has now become largely a matter of catering to short-time visitors and furnishing an occasional meal to the summer residents, many of whom own their own cottages and have become part-time permanent islanders.

Sugared marble. We have all seen marble walls become dull and dark, a grievous disappointment in public monuments and buildings, but probably we have supposed that only the surface is spoiled. A study at the National Bureau of Standards indicates that the situation may be more serious. Weathering is not simply the effect of rain and acid fumes, freezing and thawing. When the dense tangle of calcite crystals—perhaps less than 1 percent voids—in a typical marble is heated, the crystals expand along one axis and contract in the other dimensions. On cooling the movement is reversed. The crystals cannot always return to their original positions, and the block tends to swell or grow. As much as 3.5 percent expansion has been observed. The solid block becomes a porous mass of sand. Marbles vary in their sugaring tendencies. When this property has been fully catalogued architects will have guidance for the selection of the right stone.

Hunger for copper. Battelle Memorial Institute, founded in 1929 at Columbus, Ohio, for cooperative research on metals and fuels, affords many examples of the ramifications of subjects and interests. Metals affect vegetation, so Battelle is engaged in agricultural research. A recent report from the Institute tells of the need for copper as one of the "micronutrients" in growing plants. Fertilizers enriched with copper sulfate improve soybeans, cotton, potatoes, and other vegetables noticeably, though the benefits haven't been definitely measured. Tobacco, in particular, responds to a copper ration by improvement up to 30 percent or more in quantity and quality.—M.W.

THE BROWNSTONE TOWER

FOR the past six years the first evidence of spring in Washington has been the appearance of a new crop of science talent in the persons of forty boys and girls, the winners of the annual Westinghouse Science Talent Search, administered by Science Service. This year I was able to attend all three evening events to which the older Washington scientists were invited. It was a rejuvenating experience to see and hear these splendid young people.

On February 29 I entered the building of Science Service to find a heterogeneous mixture of youth and age in the larger rooms. I say "heterogeneous" because the fortunate forty were not uniformly distributed among the larger number of older people who had come to meet them. They were attracted to the stars of the gathering. The largest number stood two deep around Dr. Lark-Horovitz, physicist and general secretary of the A.A.A.S. Another circle of boys enclosed Dr. Harlow Shapley, retiring president of the A.A.A.S., and a third group hemmed in Dr. L. B. Tuckerman, physicist of the Bureau of Standards. Dr. Tuckerman held the attention of his group by folding paper into geometrical figures. It was hard to find an unattached youngster, but I persisted until I had isolated a boy from Ohio and a girl from Long Island. The latter was from the Forest Hills High School, a student of Dr. Paul F. Brandwein, who last year described in the March SM the opportunities for scientific development provided in that school. She glowed at my mention of his name, and it was not hard to understand how her interest in research on *Planaria* had been kindled. But her story was not unique; the enthusiasm and encouragement of some teacher stood behind the accomplishments of every one of those boys and girls. I failed to find the students of Mr. Subarsky (Bronx High School of Science), whose article on the characteristics of science talent appears in this issue.

The next evening was reserved for showing documentary motion pictures at the U.S. National Museum. Pictorially, the best offering was a kodachrome film on the birds of the Bear River Wildlife Refuge, but in the question period following the showing of the pictures the young scientists asked so many questions about the reels on high-speed photography, solar prominences, and rockets that Dr. Shapley, who presided, feared the birds would be completely neglected. Certainly, physical science claims the interest of most of these young people.

The big banquet of the following evening was the final event of the most exciting five days in the lives of these young scientists. Here, after the speeches, they would learn the results of the scholarship competition among themselves. The fortunate forty were distributed throughout the huge ballroom, not more than one at a table seating ten people. Each was the center of attention at his own table. At our table was an attractive girl who looked so young and demure that it was hard to believe she had already graduated from high school and had entered a technical institute to study biophysics. She talked modestly about herself in response to questions. Her hero, I think, was Dr. Shapley, whose address from the speakers' table thrilled her. She did not win a principal Westinghouse Scholarship, but she already had a scholarship from the school of her choice. The final winners were congratulated while two television cameras were aimed at them, and at the end the great audience sang "The Star-Spangled Banner."

The boys and girls have gone home, taking glowing memories with them and a feeling of responsibility for the role they are expected to play in science of the future. Not for years, if ever, will they again receive the attention that was showered upon them here. A long road of hard study and strenuous work lies ahead before they can reach their first goals of service to science and humanity. But they will arrive, and perhaps one will gain the highest honor—the Nobel prize.

F. L. Campbell

THE
SCIENTIFIC MONTHLY

JUNE 1948

THE DESERT STOREHOUSE*

MACY H. LAPHAM

Mr. Lapham took his B.S. at Michigan State College in 1899, and then came to Washington as a scientific aid in the Division of Soils, U.S.D.A. He conducted soil surveys in California, Arizona, Montana, and North Dakota during 1901–10, when he was made a field inspector with supervision of field parties in the states west of the Rockies. In April 1944, Mr. Lapham retired as senior soil scientist and was given an appointment as collaborator.

A TRAVELER passing through the Mormon towns and settlements of Utah frequently comes upon neat buildings of brick or stone upon which may be observed the words "The Bishop's Storehouse." Here are collected and preserved the products of the near-by farms and homes, religiously contributed to the Church and held as assurance against a time of need. These stores of abundance, maintained by thrift and frugality, give to the people of the scattered communities confidence in their ability to weather a storm of adversity.

As the drier regions of the south and west are approached, the farms and settlements become even more infrequent; gray sagebrush gives way to the dark green of the creosote bush, and the shrubs and grasses of the northern plains are replaced by the Joshua trees (*Yucca brevifolia*) and smaller yuccas, rabbit brushes, shad scale, and the grasses and annuals of the Mojave Desert. Further transition to the lower and warmer Sonoran Desert of southern Arizona is signaled by a diversity of forms of thorny shrubs, dwarf trees, cacti, and other woody, succulent, and herbaceous plants. Although some of these, such as the creosote bush (*Larrea tridentata*), may cover many miles of desert slopes in almost pure stand (Fig. 1), others, differing widely in size, form, color, and in means of survival in the competition for moisture, are often intermingled in interesting and fascinating association (Fig. 2). Here the giant cactus (*Cereus giganteus*), or saguaro, which may reach a height of 30 or 40 feet and a weight of several tons, may occur next to the bushy ocotillo (*Fouquieria splendens*); together with smaller globular, segmented, or dwarf forms of cacti no larger than a man's thumb and covered with near-microscopic spines they form little cactus fairylands.

To the stranger, these desert wilderness areas seem to have little to contribute to the subsistence of the native Indian or pioneer white. In this land of desolation there is no Bishop's Storehouse to offer visible means of succor or relief. Notwithstanding this forbidding aspect, to the initiated there is revealed a veritable storehouse of the desert,

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* For much of the information regarding the use made by the Indians of the plants of the desert, the author is indebted to Kearney and Peebles: *Flowering Plants and Ferns of Arizona*, U.S.D.A. Misc. Pub. No. 423, Washington, May 1942.

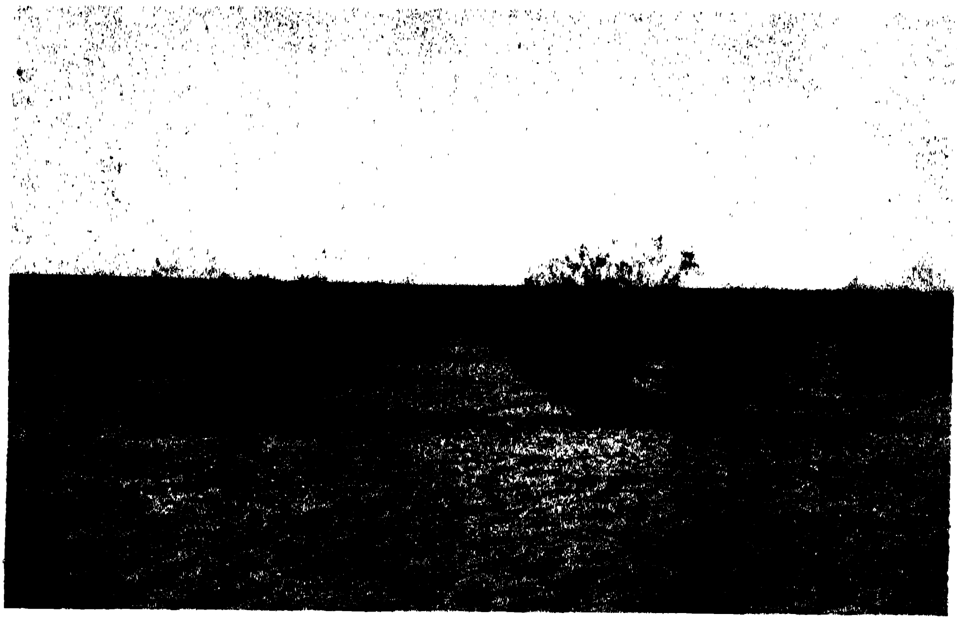

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FIG. 1. CREOSOTE BUSH (*LARREA TRIDENTATA*) IN PURE STAND

COMPETITION FOR MOISTURE RESULTS IN WIDELY SPACED HABIT OF GROWTH. FROM THE BRANCHES OF THIS PLANT THE INDIANS OBTAINED GUMS AND RESINS; FROM THE LEAVES THEY DERIVED CERTAIN DRUGS.

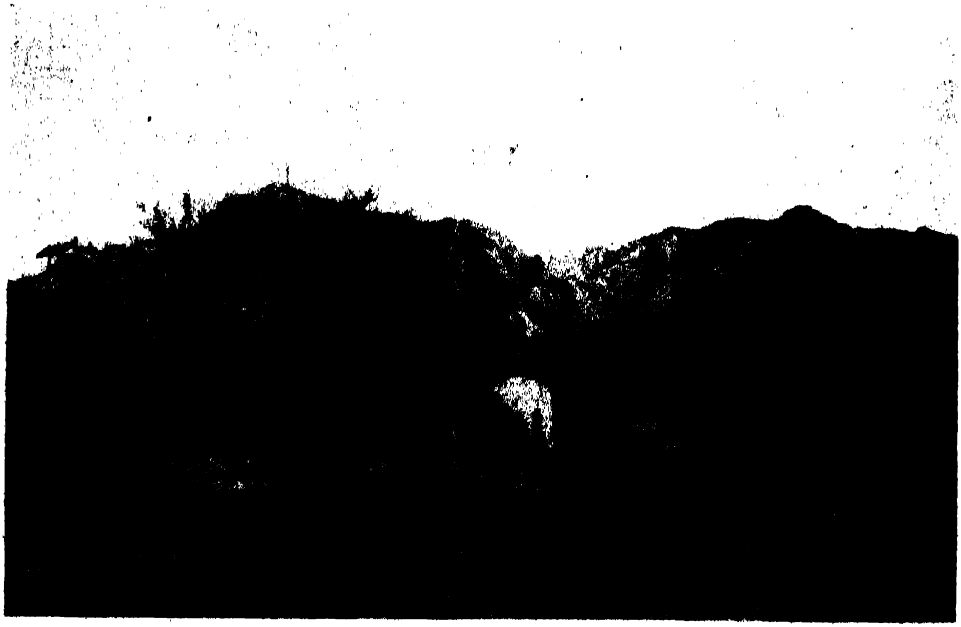

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FIG. 2. CREOSOTE BUSH AND OTHER PLANTS

HERE, IN CONTRAST WITH FIGURE 1, ARE INTERMINGLED THE CREOSOTE BUSH AND VARIOUS SPECIES OF CACTI, SHRUBS, AND DESERT TREES, CONTRIBUTING FUEL, BUILDING MATERIALS, MEDICINES, FOOD, AND DRINK.

from the widely scattered resources of which essentials in food, clothing, shelter, tools, cooking utensils, fuel, medicine, and articles of adornment or those sacred in ceremonial rites have contributed for generations, and still are contributing, to the needs of the Indian and the pioneering white.

Although the food resources of the desert are not always such as to tempt the fastidious, to the native Indian who knew how to find and use them they would support human life. To the white adventurers and the pioneer settlers who followed them, these resources were often the means of averting starvation. I have been told by an authority in one of the Western universities that, insofar as food essentials are concerned, it would be entirely feasible for an informed traveler to make his way on foot across the Mojave Desert, living on the country by subsisting on the seeds and other resources of the desert flora and by trapping rodents and other forms of desert life.

In essentials of human habitation, the thorny mesquites, of which there are two common species (*Prosopis juliflora* and *Prosopis odorata*), have contributed abundantly. In dry, sandy areas, where there may be no water table within many feet of the surface, this shrubby plant is forced to rely on surface moisture supplied by transient and uncertain rainfall (Fig. 3). Here the roots, developed near the surface, are greatly enlarged—frequently they form the greater part of the plant. These roots were dug from the soil to furnish a much-needed source of fuel. Today, in the vicinity of towns, they are mined with pick and shovel and sold as firewood to town and city dwellers. In the moist stream bottoms, the mesquite forsakes its desert habit of growth, develops a dual root system, and takes on the form of a tree as much as a foot or more in diameter and 20 to 30 feet in height. A system of lateral radiating roots at uniform, shallow depths is supported by the surface moisture, and a taproot extends downward many feet to the moist zone of the water table (Fig. 4). Dense thickets of this large treelike mesquite supply abundant fuel; poles and logs for the construction of huts and other shelters; materials for wooden utensils; and, in the farming and stockraising districts, fences and corrals (Fig. 5). Related thorny shrubs and treelike forms of the catclaw (*Acacia greggii*), in which the thorns are recurved, and species of mimosa, though less abundant, are used for similar purposes. The desert ironwood (*Olneya tesota*), a small tree, is known for its extremely hard wood, is prized for the campfire, and has been used in the past for arrowheads and implements.

All these plants are legumes and bear pods enclosing beanlike seeds. Both pods and seeds are eaten by livestock and have contributed extensively to the food resources of the Indians. Beans of the mesquite are made into meal and baked as cakes; at times of scarcity of other foods, they have been the main means of subsistence. The split and shredded inner bark, along with similar materials from the willow and cottonwood, furnish fibers and strands for binding and for woven baskets. Some of these baskets are so finely woven that, coated with gums and resins obtained from the desert plants, they may be used for liquids. Baskets made by the Apaches and Pimas were so enormous that they were used for storage of grain. The flowers of the mesquites and acacias and of some of the mimosas are frequented by wild and domesticated bees and are important honey-producing plants of the desert.

Cottonwoods along the stream bottoms provided firewood and building materials, and the catkins were sometimes eaten when other foods were scarce; beams and timbers of the Joshua tree have been found in ancient cliff dwellings, in localities where this plant, the largest of the yuccas and a member of the lily family, no longer occurs, indicating a former more extensive habitat. The flowers of this weird, misshapen denizen of the desert—which may be 30 or more feet high and several hundred years old (Fig. 6)—are pollinated by a moth. Plant and insect are thus interdependent. The seeds of the yucca were made into an edible meal. The buds and heads of the young flower stalks of the Joshua tree and of some of the smaller yuccas, known as the Spanish dagger or as *Candelabro de Dios* ("Candle of God"), from their upright,

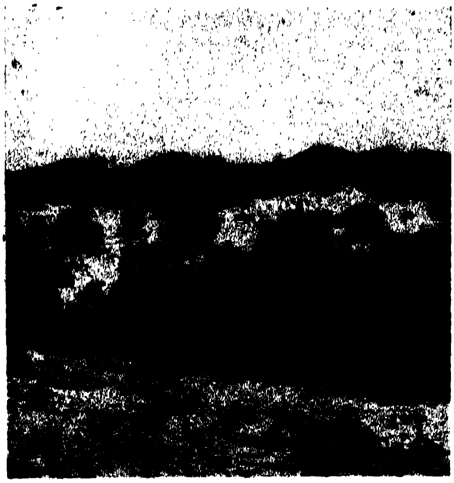

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FIG. 3. SHRUBBY MESQUITE

ON SANDY LAND THE ENLARGED ROOTS OF THIS UBIQUITOUS SHRUB FURNISH MUCH-NEEDED FIREWOOD.

showy, creamy-white flower spikes, and of species of *Agave*, or century plant, and sotol, which are high in sugar content, were boiled, eaten as a salad, or roasted in pits. The pulpy fruit of the *Yucca baccata* is also reported by Kearney and Peebles to have been eaten in fresh condition, roasted, or dried for later consumption. Various species of yucca also supplied fibers for thread and materials for weaving mats and sandals. The roots of most of the smaller yuccas, when macerated in water, will serve as a fair substitute for soap, thus giving rise to the common name of soapweed. They may even have found their way into the ingredients of commercial preparations for toilet use (Fig. 7).

Nuts of the native walnut (*Juglans rupestris*), although inferior to the Eastern black walnut, were eaten; acorns from the native oaks, roasted and leached of their bitter astringent properties, ground, and baked into cakes, still form an important item in the basic food supply of many Indian tribes. Seeds of the piñon, or nut pine, especially the *Pinus edulis*, harvested from cones or pilfered from the hoarded stores of squirrels and other rodents, were highly esteemed, and the nuts are an important item today in the stock of the city confectioner and dealer in delicacies. The pine nuts and acorns supplied food for squirrels, wild turkeys, and deer. Other mammals, utilizing the oaks

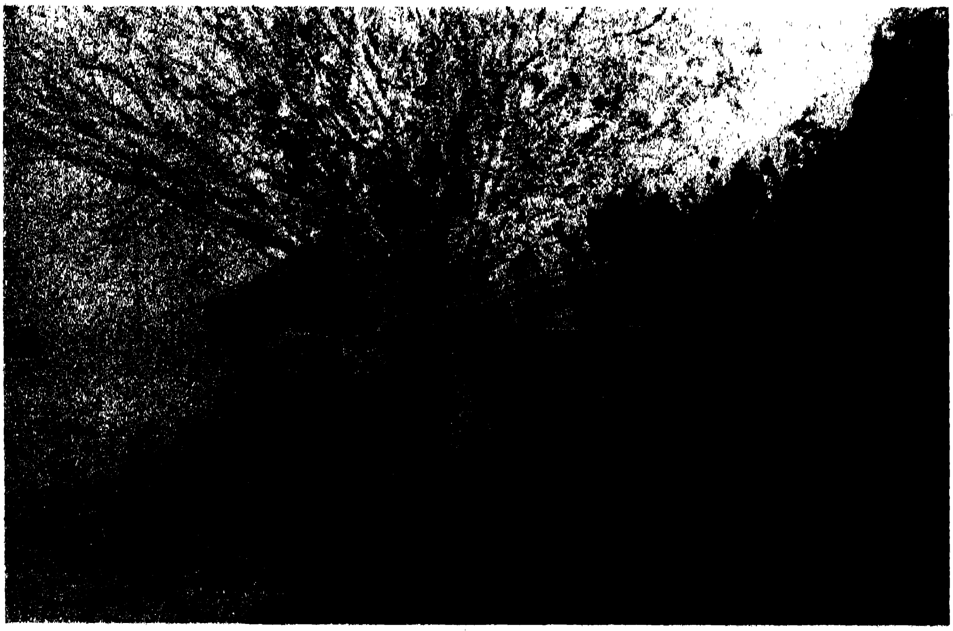

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FIG. 4. TREELIKE FORMS OF MESQUITE IN ALLUVIAL STREAM BOTTOM

LATERAL ROOTS TAKE SURFACE MOISTURE, WHILE THE LARGE TAPROOT REACHES THE DEEPER MOIST ZONE.

Photo by U.S.D.A.

FIG. 5. MESQUITE THICKET IN RIVER BOTTOM (*COTTONWOOD ON RIGHT*)

MESQUITE THICKETS SUPPLY FUEL, POLES, AND TIMBERS FOR BUILDINGS AND FENCES, AND FIBERS AND STRANDS FOR BASKETS AND BINDING MATERIALS. FROM THE MESQUITE'S BARK, SEED PODS, AND BEANLIKE SEEDS COME FOOD, BROWSE FOR LIVESTOCK, MEDICINES, GUMS AND DYES, AND AN ALCOHOLIC BEVERAGE.

and shrubs as browse, in turn entered into the food resources of the Indians.

Most of the ubiquitous cacti are of minor importance in the economy of the Indians, but the spiny, crimson fruits of the saguaro and of the organ pipe or pitihaya cactus, are highly esteemed by the Pimas and the Papagos, who make use of them in fresh condition or as a preserve. The jars of cactus preserve form an important article of trade or barter. The fruits also contain numerous small, black, shiny seeds that are dried, ground, and baked into cakes. The red, spiny fruits of the prickly-pear cacti are also eaten by the Indians and Mexicans and are sometimes seen in city markets. Other fruits that are eaten in a fresh state, cooked, or dried by today's Indians include those of the elder, mulberry, mistletoe, wild currant, and ground cherry. Jellies and preserves made of these berries were often to be found in the larder of the early pioneer.

The trunk of the saguaro consists of a column with a tough green rind studded with spines, formed around a series of upright woody ribs, or rods. These rods, arranged in a circle enclosing the succulent, fleshy interior, are very hard and durable and remain on the ground long after the fleshy part of the plant has succumbed to decay (Fig. 8). They are used by the southern desert Indians in the supports and roofing of their flimsy dwellings. Branches and stems of arrow weed, willow, and reeds are interwoven and plastered with mud to make the unsubstantial walls that suffice under the prevailing mild climatic conditions.

The gaunt, thorn-studded stalks of the ocotillo are also a hard wood. Covered with a resinous coating that resists weathering and decay, they are especially useful in the construction of desert homes. Known also as candlewood, coachwhip, and monkeytail, the wood burns with a white flame and is highly regarded as a fuel for campfires (Fig. 9). In early spring the leafless stalks are tipped with spectacular scarlet, tubular flowers, which form one of the most attractive and fascinating floral displays of the desert. After a spring shower, the stalks become covered with small, round, scalelike

Photo by U.S.D.A.

FIG. 6. JOSHUA TREE (*YUCCA BREVIFOLIA*) ON MOJAVE DESERT

FROM THIS WEIRD, MISSHAPEN PLANT, LARGEST OF THE YUCCAS, BEAMS AND TIMBERS WERE OBTAINED FOR CONSTRUCTION OF SOME OF THE ANCIENT CLIFF DWELLINGS. THE LATER INDIANS ATE THE TREE'S SEEDS.

Photo by U.S.D.A.

FIG. 7. YUCCA FOREST IN SOUTHERN NEW MEXICO

FROM THE BUDS, YOUNG FLOWER STALKS, AND FRUITS OF SPECIES OF YUCCA ARE OBTAINED FOOD AND A FERMENTED BEVERAGE. THE LEAVES FURNISH FIBERS, AND THE ROOTS SUPPLY DRUGS AND A SOAP SUBSTITUTE.

leaves, which wither and fall away when the moisture in the soil has become depleted; these may be replaced later if showers again occur. An apparently dead and lifeless section of one of these stalks, placed in a jar of water, seemingly comes to life as the small green leaves appear. The stalks are frequently collected from the desert and set in the ground or bound together as flimsy but effective fences, which sometimes take root, thus robbing the desert of some of its natural charm (Fig. 10).

mallow or cheeseweed, cow parsnip, milkweed, and dandelions as greens or as boiled vegetables.

Roots of the night-blooming cereus (*Cereus greggii*), known as *reina de la noche* ("queen of the night"), whose beautiful, heavily scented flowers last but a few hours, are reported by Kearney and Peebles to have been eaten by some Indian tribes. Roots, bulbs, and tubers of the wild carrot (*Daucus pusillus*), or rattlesnake weed, sandroot, species of wild potato, tobacco root (*Valeriana*), arrowhead, or tule potato (*Sagittaria*), and the *Brodiaea*, or grassnut, contributed to the starchy foods of the Navahós, Pimas, Papagos, and some of the other tribes.

Photo by author

FIG. 8. TRUNK OF THE SAGUARO, OR GIANT CACTUS (*CEREUS GIGANTEUS*)

Right, HEALTHY SPECIMEN; *left*, DEAD PLANT. THE OUTER RIND OF THE LATTER HAS BEEN REMOVED BY DECAY, EXPOSING THE HARD, WOODY RIBS THAT ENCLOSE THE FLESHY INTERIOR. THESE RIBS RESIST DECAY TO SUCH A DEGREE THAT THEY HAVE BEEN USED BY INDIANS FOR CONSTRUCTION OF HUTS AND SHELTERS.

Species of wild beans furnish forage for livestock; one of these, known as the tepary bean, has been grown by the Papago Indians to such an extent that they became known as the "bean eaters" or "bean people." Seeds of tumbleweed (*Cycloloma*), *Suaeda*, or seepweed, and of species of saltbush and sage were parched and made into flour for mush or pinole. Various Indian tribes are reported to have used the leaves, tender shoots, and petioles of the canaigre, or wild rhubarb (*Rumex*), the saltbush (*Atriplex*), plants of the mustard family, *Stanleya*, or desert plume,

The Indians of the southern desert, such as the Papago, Pima, Maricopa, and the Yuma tribes, were of more sedentary habit and not so skilled in hunting as the Apache, the Navaho, and other more northern Indians. These less accomplished hunters depended on rabbits, small mammals, and an occasional small Sonoran white-tail deer to add to the larder and to provide skins for

Photo by author

FIG. 9. OCOTILLO (*LEFT FOREGROUND*) AND CHOLLA CACTUS

THE GAUNT, THORNY, AND USUALLY LEAFLESS STALKS OF OCOTILLO ARE COVERED WITH A WAXY RESIN. THEY ARE SOUGHT BY THE INDIANS FOR BUILDING MATERIALS AND ARE USED FOR FIREWOOD AND FENCES.

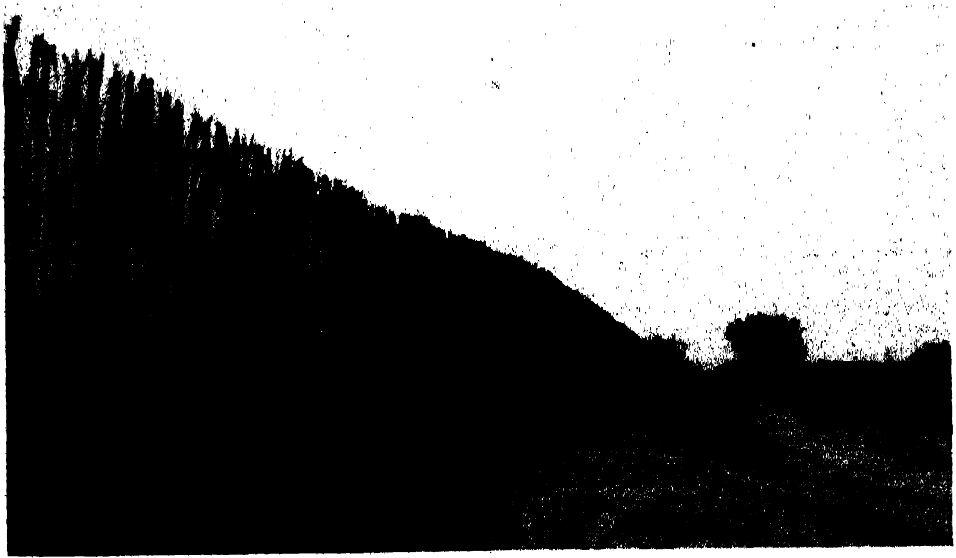


Photo by author

FIG. 10. FENCE OF OCOTILLO STEMS AND WOVEN WIRE

THE THORN-STUDDED STALKS OF THIS PLANT MAKE EFFECTIVE FENCES AND WHEN PLACED IN MOIST SOIL MAY TAKE ROOT AND GROW. MUCH OF THE DESERT CHARM IS SPOILED BY SUCH OCOTILLO FENCING.

robes and clothing. Birds that were trapped or netted included the bandtailed pigeon, wild turkey, and the Sonoran dove, the "white wing" of the sportsman. This dove often seems to call out inquisitively, "Who cooks for you?" The mule deer (larger than the white-tail), the antelope, and the desert bighorn supplied meat—some of it used in dried or smoked form—to the more skillful hunters of the mountains and northern plains. Small rodents and other inhabitants of the desert were resorted to in time of need. Of these, the chuckwalla, largest of the desert lizards aside from the sluggish Gila monster, was much esteemed by some of the Indians. This interesting animal, when pursued, secretes himself in a crevice in the rocks, wedging himself in by inflating his body, and daring the pursuer to pull him out. The Indians, however, soon learned to puncture his hide with a thorn and withdraw him ingloriously by the tail.

Even condiments and seasoning for food, before the present era of the tin can and the cellophane package, were obtained from native mints, peppergrass, sage, and other aromatic herbs. Ashes of the saltbush, which grows in saline soils, were used as a substitute for baking powder. Other plant products containing sugar and mucilaginous substances yielded substitutes for candy and chewing gum. In the absence of other liquids, the macerated pulp of the barrel cactus (from which the cactus candy of the confectioner is made) may provide a drink for the thirsty traveler.

In the fashioning of mats, nets, sandals, baskets, and in the thatching of roofs, further materials were contributed by the *Sesbania*, the tule (*Scirpus*), the cattail, and branches of various shrubs, the marsh fleabane, and reeds of the genus *Phragmites*. Split seed pods of the *Martynia pariflora*, known as the unicorn plant, or devil's-claw, formed the pattern for the black figures woven into many of the baskets made by the artistic Pimas, who excelled in making baskets of all kinds. Wild cotton was cultivated and harvested by the Indians before the white man and his wool-bearing animals found their way into the desert.

In his arts and crafts the Indian used gums and resins from the mesquite and the creosote bush as adhesives; awls made of cactus spines or sharpened bone; and dyes obtained from species of the indigo bush, sumac, mesquite, the fetid marigold, seeds of the sunflower, and from minerals. Pendants and other articles of adornment were made from fragments and beads of carnelian, garnet, topaz, turquoise, and other semiprecious stones, colored seeds, animal claws, and feathers.

In the absence of the family drugstore, the Indian and the old-timer of the desert resorted to a wide range of desert plants for cures for various ailments. Some of these were of doubtful value, but others are to be found upon the shelves of the modern druggist. These remedies included materials for poultices and infusions and decoctions of the manzanita, creosote bush, catnip, canaigre, or wild rhubard, yerba santa, or mountain balm, yerba mansa, the inner bark of the cottonwood, winter fat (*Eurotia*), golden aster, goldenrod, yarrow, horsebrush (*Tetradymia*), and species of sunflower. They were used for sore throats, coughs, respiratory diseases, boils, toothaches, fevers, sore eyes, headaches, and as tonics and emetics. Mullein leaves were smoked and used for medicinal purposes, and roots of the yucca, winter fat, and four o'clock and leaves of the seepweed were used as laxatives and for burns and stomachache. There was even an insecticide—a sweetened infusion of the leaves of the *Haplophyton*, or cockroach plant, which was used as a poison for mosquitoes, cockroaches, flies, and other pests.

The Indian drew on his environment, also, for paraphernalia for his social hours. From clay and stone he fashioned tobacco pipes, with reed stems. Mild beverages were brewed from species of sage, the young flowers and leaves of the *Thelesperma*, the *Ephedra*, or joint fir (commonly known as Mormon tea), and from seeds of the deernut or coffeebush (*Simmondsia*). More potent, alcoholic drinks were made from the roasted and fermented young flower stalks and heads of species of the century plant and the sotol, from pods of the mesquite, and from fruits of the *Yucca baccata*, the saguaro, and the pitihaya cactus.

In supplications for rain and in other sacred ceremonies of the Hopis and others, drums of hide stretched over wooden forms, gourd rattles, and reed flutes were among the stage effects; leaves and flowers of larkspur, paintbrush, and other plants to which were attributed magic powers, were smoked or otherwise employed in religious rites. Thus, as the Indian made his round of this self-help commissary in an apparently empty wasteland, he found an impressive stock to be harvested or added to his market basket. We can only marvel at the wisdom and the vast store of knowledge accumulated by these primitive peoples as they made the desert feed, clothe, and shelter them.

LINES TO A SKYSCRAPER

Preposterous pile of stick and stone and steel!
How men shall marvel, a few cycles hence,
At this gaunt hive, that now seems scarcely real,
And wonder at the strange magnificence
Of folk who, given Eden, lost their way
And ended in these cells of their own making—
Not temples, save to greed, where, souls forsaking,
They delved and schemed and hid themselves from day.

My temple is the hills, said Gabe McCord;
Its nave is cloistered by tall rows of firs.
Its choral is the birds; its Ancient Word
Comes on the evening breeze, that incense stirs
And plays soft hymns on pine boughs to the Lord.

The hills will stay when man's grim towers are gone
And sanity returns, some far-off dawn.

And so, for blasphemy in stone, Forgiveness, Lord!
Said Gabe McCord.

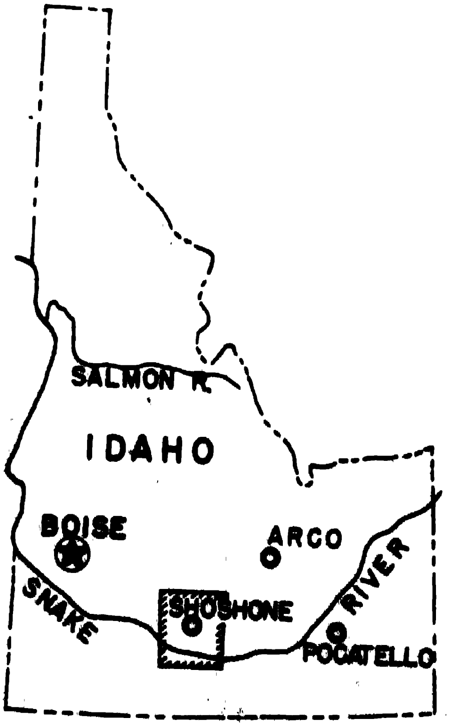
LAURENS C. BOLLES

BLACK BUTTE, A RECENT SUBSIDENCE CRATER

E. R. HARRINGTON

Engineer, schoolteacher, and free-lance writer, Dr. Harrington (Ph.D., University of Southern California, 1940) has been head of the Science Department, Albuquerque (N. M.) High School since 1930. In the course of his work as a consulting engineer in many Western states, he has had numerous opportunities to observe the geologic formations of which he writes.

FOR the study of basalt flows the Idaho Snake River Plain can scarcely be equaled. Here great outpourings of black lava have covered the southern third of the state, some depths ranging up to five thousand feet. The plain is a tremendous gray-green sea of sage-covered lava lapping against the south slopes of the great granite mass that forms the central Idaho mountains. This lava sea has everything! Lava flows interstratified with lake sediments, ash, dust, and gravel beds; frozen lava cascades and large areas of smooth ropy lava resembling beds of giant black snakes (geologists refer to such ropy lava as "pahoehoe"); long cracked ridges and smooth domes left by red-hot molten rock as it cooled long ago; subsurface channels still showing the flow marks along their walls; cave channels more than a mile in length; streams which flow from the mountains and vanish in the plain; water tables "perched" at a depth of fifty feet in some places and five hundred feet below the surface a mile away; caves filled with ice throughout the year; hot springs; cold springs; springs that pour from the canyon walls by the thousands; old lavas dusted over with wind-blown sand anchored by sagebrush; islands of lava that may have been poured out within the recent centuries; great jumbled masses of angular lava fragments known to geologists by the Hawaiian name of "aa"; steep-sided cones built up of volcanic ash, froth (called scoria), rounded "bombs," and viscous lava; great flows so fluid that they left no certain indication of the fissures from whence they came; and so on and so on. Whatever volcanic flow characteristics are sought, the Snake River Plain will come close to furnishing them: old flows dating back millions of years; new flows less than a thousand years in age. Name it, and the Idaho lava plain can show it.



BLACK BUTTE IN IDAHO

THE SHADED RECTANGLE AROUND SHOSHONE LOCATES THE VOLCANIC BLACK BUTTE AREA IN THE STATE.

To the student of geology, all the flows, both young and old, will be of interest. To the casual observer, the recent flows will stand out more sharply and attract the most attention. To the visitor who wishes to observe the new and see where it meets the old, the Black Butte area offers the ideal position.

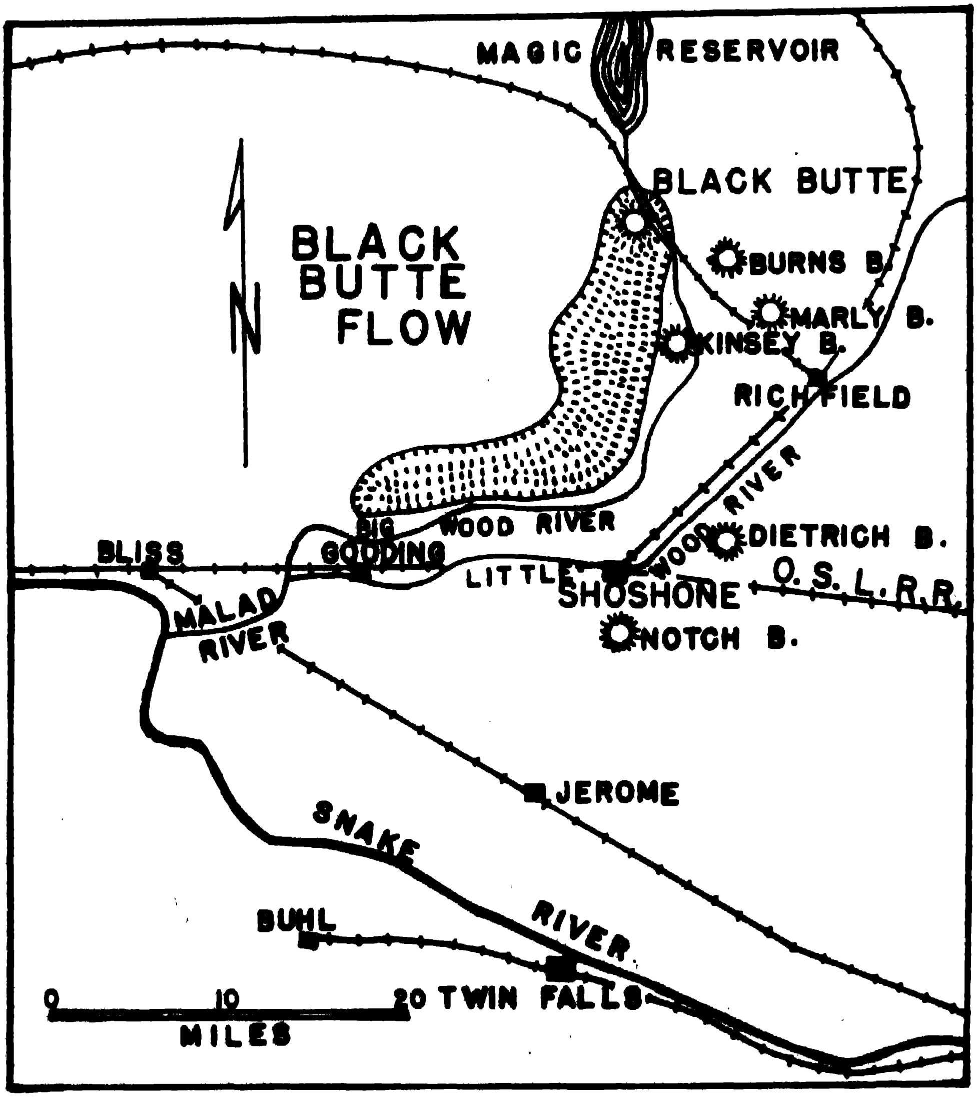
The great outpourings of Idaho's black lava (basalt) began back in Eocene times, considered by the geologist as recent, although they date back more than thirty millions of years. The lava flows have continued, intermittently, almost down to the present century. Through a considerable area of the Plains the basalt is so fresh that it preserves all the surface features associated with flows that have been poured out within the past thousand years. Soil is largely absent in such areas, and the same can be said of water. Washington Irving, in his *Adventures of Captain Bonneville,* describes one of these areas as "A place where nothing meets the eye but a desolate and awful waste, where no grass grows nor water runs and nothing is to be seen but lava." This special quotation might have referred to the Craters of the Moon region near Arco, or it might have referred to the Black Butte flows mentioned here. Stearns estimates that the Craters of the Moon activity dates back 250 to 1,000 years, and the Black Butte flow has the same apparent age.

The Black Butte flow is a large black island resting on the sea of older lava. To the east and south a number of broad, flat cones show some of the sources of the older lava. The Dietrich Butte east of Shoshone is representative of this class. Its great flat cone is fifteen miles in diameter at the base, four hundred feet high, and capped by a subsidence crater three hundred feet deep and a mile in diameter. The crater floor is covered deeply by fine, wind-borne sand, or "paloose," more than 90 percent of which is as fine as wheat flour. Notch, Marley, Kinsey, and Burns buttes are similar cones, and there are numerous less prominent ones that mark vents through which the lava once poured.

Big Wood and Little Wood rivers rise in the central Idaho mountains and flow across the lava plain, joining for their last ten miles to form the Malad River, the only surface stream to reach the Snake River from the north in a distance of three hundred miles. Other streams have made the start, only to become "lost rivers" when they reached the porous lava plain. The Wood rivers carried through in part because of their strength as flowing streams, but chiefly owing to the fact that through most of their course they flow in comparatively well-established channels,

BLACK BUTTE AS SEEN FROM THE OLDER LAVA ON THE EAST

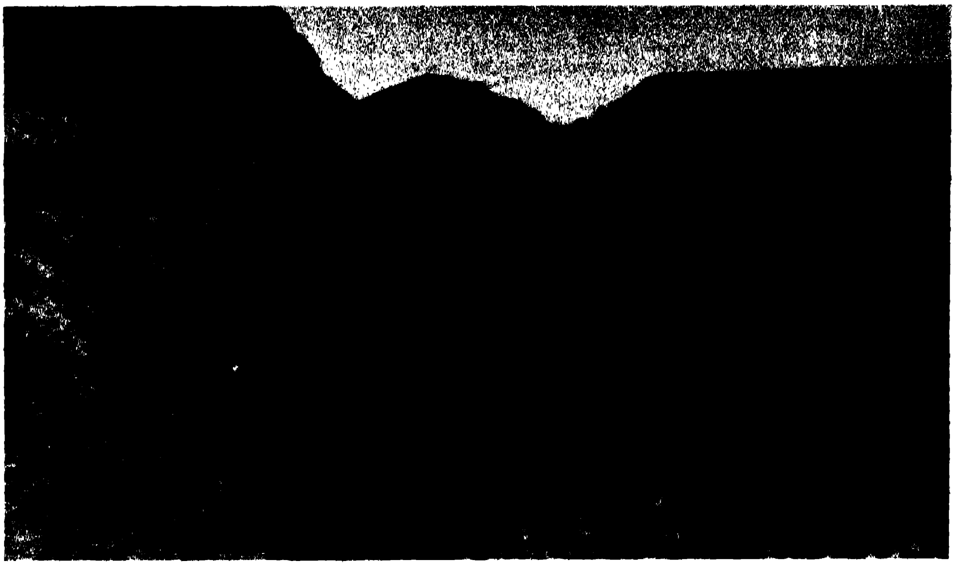
THE CONE IS LOW AND BROAD, A CHARACTERISTIC OF BASALT, WHICH IS A VERY FLUID LAVA.



BLACK BUTTE AND ITS LAVA FLOW

which have been silted up through many centuries of flow. The older basalt areas traversed by the streams are well dusted over with soil in most places, and extensive farming acreages are irrigated from the streams. In the lava areas where the soil is too thin for cultivation or the land is too high to be reached by irrigation water, the lava is well covered by hardy grass and the inevitable sagebrush (*Artemisia tridentata*).

The Black Butte crater is fifteen miles due north of the town of Shoshone, Idaho. The basalt that issued from the cone followed the course of the Big Wood River, crowding it from its former bed and forcing it to cut a trench along the flow's edge. In many places the lava is cindery, frothy, and filled with gas bubbles. Gas bubbles more than two feet in diameter are common. The Big Wood River has made use of these pockets in cutting down its bed. The stream pebbles have ground out the gas pockets, forming large potholes, and at places the stream channel is fifty feet deep, though only four feet wide at



A PRESSURE DOME IN BLACK BUTTE CRATER

the top. The newly formed channel has not silted up appreciably, and the water loss is tremendous, amounting to half the peak flood in one section of three miles. The local irrigation company has built a by-pass around the recent lava section to avoid the excessive loss of water. In the older basalts above the Black Butte flow, the Big Wood River has cut a wide and deep canyon, which is well silted up, and here the water loss is small.

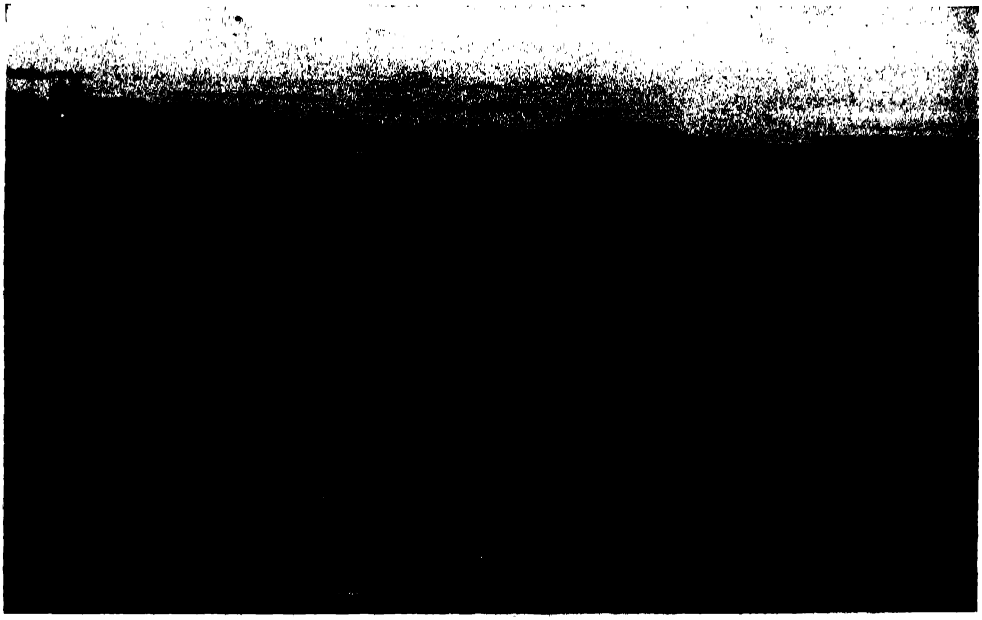
The Black Butte cone rises about four hundred feet above the old lava plain. Its broken top is surmounted by an irregular crater a mile long and half a mile wide. The crater is the perfect example of a recent volcanic subsidence. The final withdrawal of

CHANNEL OF THE BIG WOOD RIVER THROUGH THE BLACK BUTTE FLOW material left the inner crater "well" with nearly vertical sides and a flat, ropy floor broken only by a few small necks and domes.

The north rim of the crater is notched by a U-shaped ditch twenty feet wide and six feet deep. This ditch runs for more than half a mile down the north side of the cone. Evidently, for a time, the huge crater was filled with molten lava, and a part of it coursed down the north side, forming this well-defined ditch in which the flow lines are as fresh today as if the flow had taken place a few days ago. Two other less well-defined ditches also led small flows of lava down the north side of the crater. On the east side of the crater a small break a hundred feet below the rim flowed for a time and built up a small parasitic "spatter cone." The crater rim is made up partially of red, frothy scoria and a thin, platy lava mixed with volcanic ash, blobs, and bombs.

The largest part of the lava went to the south, passing through a channel below the surface of the advancing flow. This lava tube has now fallen in, forming a wide, ragged ditch that can be followed for several miles from the butte's base. At one point a mile from the crater, a portion of the channel roof has remained in place to form a natural bridge across the depression crack. It is apparent that this channel was the outlet that finally drained the Black Butte crater. As the lava subsided two hundred feet or more inside the crater, large rockfalls slid down the nearly vertical crater sides. As these rockfalls slid to the crater bottom, they cut grooves in the pasty lava walls or, in places, smoothed out the congealing lava into slick slides.

Today a view from the south rim of the Black Butte crater will show the larger part of the great flow curving to the south and west. To the east lie the irrigated fields of the Big Wood River Valley. To the west the older sage-covered basalt extends to the horizon. Between the valley and the old plain the recent lava lies like a great crescent-shaped splash of India ink.



LAVA DITCH ON THE NORTH SLOPE OF BLACK BUTTE

A number of older underground channels are found in the lava along the western side of the recent flow. One of these channels in the older lava is occupied by the Shoshone Ice Cave, now a National Monument.

The Black Butte flow is traversed by a road to the Ice Cave, but no road leads to the crater itself. A rocky road to the Magic

Reservoir does lead along the north rim of the flow, and the crater rim is best reached from the north side by walking up the lava ditch previously mentioned. The lava is cracked and broken, and its knifelike edges are very hard on shoes. Depression cracks and irregular lava blocks make detours necessary in places, but when the traveler reaches the crest he gets a real view.

From his vantage point the visitor need not be a "rock detective" to read the story. He can see the old lava plain as it was before the eruption of the Black Butte. He can visualize the formation of the crater from a small, active vent and watch it as it builds itself up above the old plain. He can imagine the crater as it was at the height of its glory when it was brimming full with a white-hot lake of lava a mile in diameter. In his mind he can see the white-hot lava splash across the north rim and pour down into the valley of the Big Wood River. He can all but hear the steam explosions as the hot lava poured into the stream channel to impound the water into a boiling lake. Finally, he can see the great lava tube open to the south and build up a huge red-and-black smoking crescent thirty miles long. On the north side of the crater the Big Wood River is already picking its hissing way across the smoldering rock dam. The crater is drained, the volcano dead.

When did the volcano die? Maybe it was a thousand years ago. Maybe it was only two or three hundred years ago. The visitor cannot determine the time, but the story is there for him to read. All he needs to do is to climb the north rim and look about him. Black Butte is without doubt one of the best examples of recent volcanic activity in the United States,

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THE AURORA

The sun spouts forth small particles
Sprayed like gigantic streams that trace
Vast, awesome, and electrified,
The realms of space.
Through millions on millions of miles
From this great, whirling soul of day,
The vast light sprinkler, arcing wide,
Rotates our way
Drawn by Earth's own magnetic fields
Where atoms, clashing, give off light
And spill their startled glory's tide
To rainbow night
Like some strange shoreless, burning sea
Swept from the sands of Eternity!

ERIS GOFF

THE RUPPIA BALLS OF LITTLE BORAX LAKE

E. O. ESSIG

After taking his M.S. from Pomona College in 1909, Professor Essig spent a year as horticultural commissioner of Ventura County, California, and three years as secretary of the California State Horticultural Commission. In 1914 he became assistant professor of entomology at the University of California (Berkeley) and he is now chairman of its Division of Entomology and Parasitology.

ON July 5 last, A. A. Trippel, of Nice, Lake County, California, brought to my office several strange-looking balls which had been collected on the shores of Little Borax Lake. These balls were regularly oval in shape, gray in color, and appeared to be composed of a hairlike substance. The collector furnished the following information concerning them:

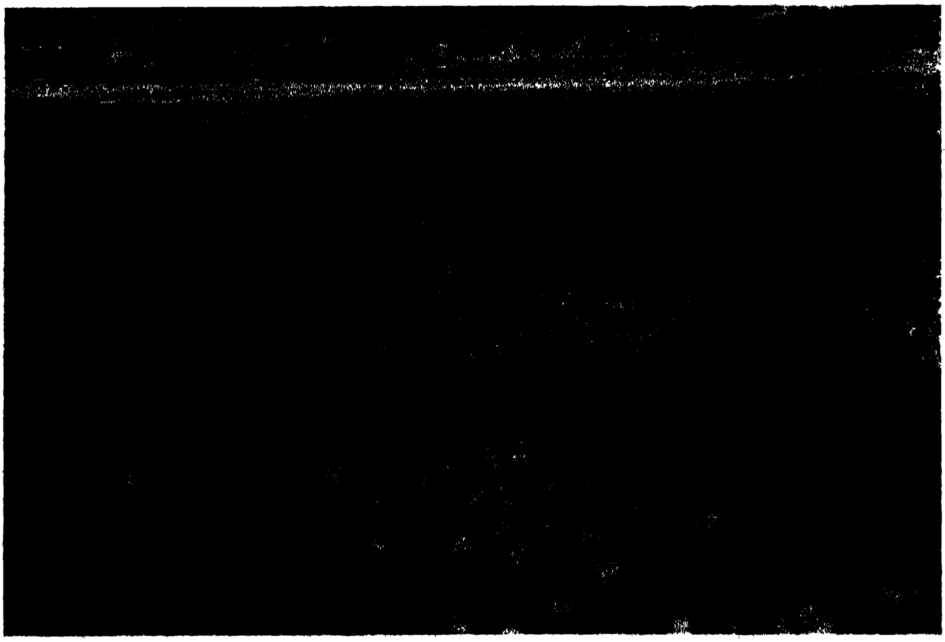
1. They had not previously been noted in the country even by the Indians or the oldest white inhabitants.
2. They occurred on the shores of this particular lake (when it had dried up) in great numbers—perhaps thousands.
3. They varied in size from a baseball to a small watermelon; in shape they were regularly oval, though a few of the smaller ones were spherical.
4. Their composition was unknown and could not be accounted for.
5. Some of them, certainly, were too large to be stomach hair balls, or aegagropilae.

A cross section of the balls showed an even, closely packed matrix of small, stiff, woody, needlelike fibers nearly uniform in length. These hairlike strands were so tightly and intricately interlaced as to constitute an almost indestructible whole. Yet there was no evidence of any adhesive binding the innumerable bits together, and no obvious core which might have furnished the

LITTLE BORAX LAKE

RUPPIA BALLS ALONG THE EASTERN SHORE LINE

THIS PICTURE WAS TAKEN FROM THE LAKE BOTTOM AND LOOKS SOUTHEASTERLY.



A CLOSER VIEW OF THE *RUPPIA* BALLS

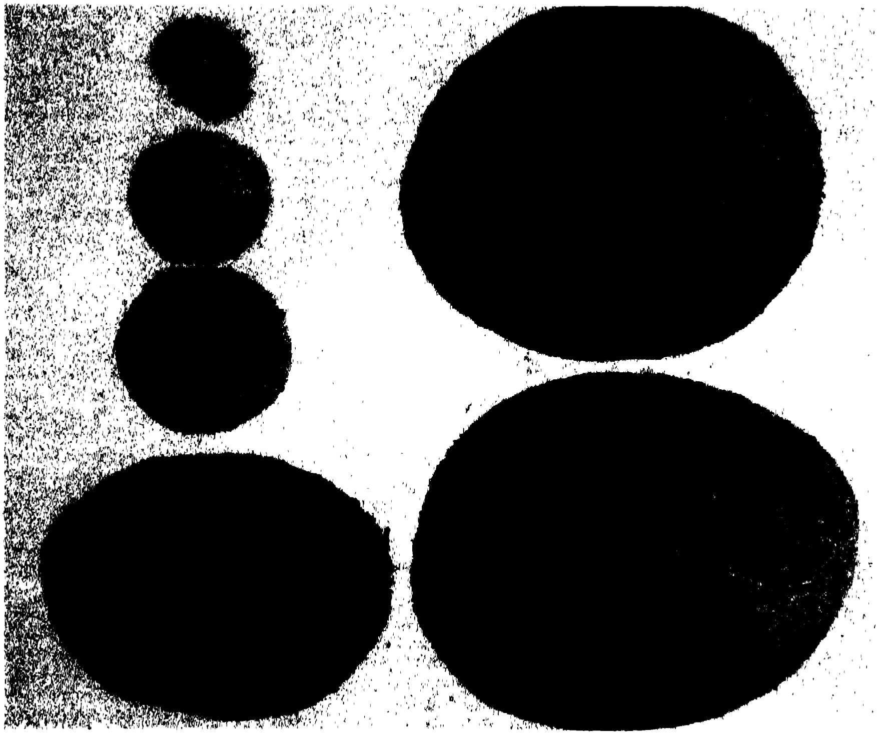
ABOUT HALF TOTAL NUMBER ARE SEEN, WITH THE LAKE BOTTOM AT THE LEFT.

beginning of the balls. It was evident, however, that the balls had been enlarged gradually, because formations as small as one inch in diameter were subsequently discovered. A few balls, opened later, revealed leafy tissues throughout the matrix, and even slight cavities.

A microscopic examination at once disclosed the true nature of these mysterious balls. The small, hairlike filaments were, in fact, plant material. Many of them terminated in an enlarged black, round seed pod, with a pointed end. Specimens were submitted to Dr. H. L. Mason, professor of botany at the University of California, who identified the plant as *Ruppia maritima* L., commonly called "ditch grass." The balls appeared to be composed almost entirely of the small seedstalks and, possibly, broken stems of this plant.

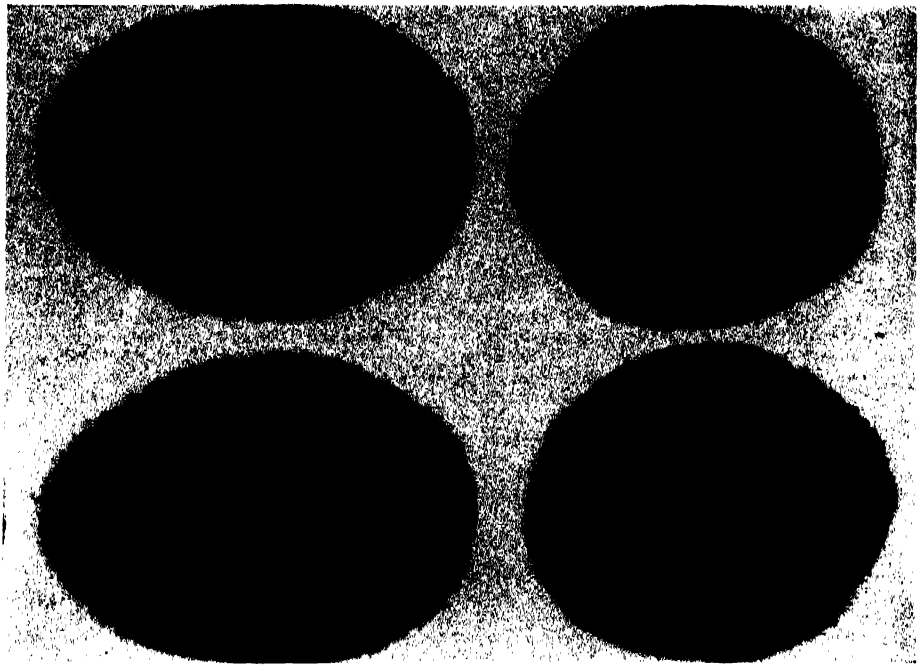
Ditch grass grows in brackish or salt water and is widely distributed throughout the Northern Hemisphere. Along the Pacific Coast it occurs from British Columbia to southern California. It is an odd plant, having greenish-white branching stems and threadlike leaves. It grows in large masses, which break apart and float about in the water.

The desire for more information concerning the origin of the balls, especially the manner in which they were formed, led me to visit Little Borax Lake, which lies at the foot of Mount Konokti, between the mountain and Clear Lake. The trip was made on October 12, just after an inch of rain had fallen in the area. The brackish water of "Borax Pond" (as it is sometimes called) ordinarily does not overflow; it therefore varies from slight alkalinity during periods of heavy rainfall (average annual rainfall for Lake County is 28.51 inches) to heavy salt



SPECIMENS OF *RUPPIA* BALLS

THEY VARY IN SIZE FROM A BASEBALL TO A SMALL WATERMELON AND ARE GENERALLY OVAL IN SHAPE. THERE IS EVIDENCE THAT SOME OF THESE BALLS MUST HAVE BEEN ON THE SHORE OF LITTLE BORAX LAKE FOR MANY YEARS, FOR GREAT NUMBERS—PERHAPS THOUSANDS—WERE FOUND IN VARIOUS STAGES OF DECAY.



CROSS SECTIONS OF TWO *RUPPIA* BALLS

SHOWN HERE IS THE EVEN, CLOSELY PACKED MATRIX OF STIFF, WOODY, NEEDLELIKE FIBERS. THESE HAIR-LIKE FILAMENTS, SO TIGHTLY AND INTRICATELY ENMESHED, FORM AN ALMOST INDESTRUCTIBLE WHOLE.

concentrations just prior to complete evaporation. The lake is small, almost circular, and approximately a quarter of a mile in diameter. Normally, there is not much more than four feet of water over the saucer-shaped bottom. At the time of my first visit, it was possible to walk across the exposed bottom, which in spots was covered with a thin layer of decomposed vegetable matter. This deposit, wetted by the rain, had about the same consistency as fresh cow manure and a rather offensive odor. This decaying plant residue indicated clearly that there must have been vegetation growing in the water at the time the lake dried up. I visited the lake again on January 18, 1948, at which time probably one-fourth of the bottom was covered by water. The mud did not permit an examination of any life that may have started. Decayed *Ruppia* appeared in a strip extending about one hundred feet from the upper edge of the shore line toward the middle of the lake, and was limited to the eastern shore.

On the cracked soil of the bottom, there were remnants of aquatic animal life, especially of the larvae and pupae of the so-called Kootsabe fly, *Ephydra hians* Say, which occurs elsewhere only in Mono Lake, Owens Lake, and East Lake, all of which are east of the Sierra. This fly has lived in Little Borax Lake for years. For this reason, it seems obvious that the adults must survive when the lake dries up, which has surely happened many times in past decades.

The *Ruppia* balls were mostly near the high-water mark on the eastern shore. In this same area, grasses were green and growing well, whereas everywhere else around the margin of the lake the banks were dry and devoid of vegetation. The prevailing winds may have come from the west and washed the balls onto the east shore.

It seems likely that the balls were formed by the loose *Ruppia* plants being rolled by wave action on the broken remnants of plants along the shallow shore line until the compact, almost indestructible masses were shaped, and that they were rolled up along the shore line only at high water. A close examination of the eastern shore of the lake revealed a great thick mass of small particles of the *Ruppia* plants imbedded in a matrix of decaying material. This would seem to indicate that only a very small amount of the available material was actually being shaped into these remarkable balls. The inexplicable thing was that practically all the balls were deposited in a relatively restricted strip not more than four to six feet wide. (This might possibly have been caused by a particularly violent windstorm when the lake was nearly dry.)

There was plenty of evidence that some of these balls must have been on the shore for many years, because countless numbers in different stages of disintegration were noted throughout the area.

WEATHER IS LIKE WILD HORSES

How can we hope ever to bring them in—
Black stallions from the mountains of the moon,
White mares that pasture down the Pleiades,
Or their fleet young of tapping, crystal hoof,
Dashing in silver herds across the roof—
How can we hope? Space, the unconquered range,
Rolls past surmise, and there are bars that hold
Against all capture. O they may even come,
Their quivering nostrils stilled one sudden hour,
Nuzzle a soft nose in a hand, their power
Seemingly broken, but surely as we cross
To claim their invisible tethers and voice for truth
Their courses one turn hence, then will they stiffen,
Rearing against all forecast, the erstwhile tame,
Foam-flecked and thundering through crackling whips of flame!

Isabelle Bryans Longfellow

PERSONALITY CLASSIFICATION AND COUNSELING TECHNIQUES

ROBERT H. SEASHORE and WARD JENSSEN

A specialist in the classification of experimental methods, Dr. Seashore took his Ph.D. in psychology at the University of Iowa in 1925. He has taught at Ohio State, Stanford, Southern California, Oregon, and at Northwestern, where he is now chairman of the Department. Mr. Jenssen is a graduate student at Northwestern specializing in personnel work.

PSYCHOLOGISTS have suggested a number of methods for classifying the so-called personality characteristics. Kretschmer, for example, has proposed a system of parallel physical and psychological types (the tall, slender man tending to be reclusive and self-contained, etc.), and Jung has offered the well-known extroversion-introversion scale as a basis for personality classification. Other systems may be even more familiar to the reader—handwriting analysis, interest inventories, and neurotic symptoms, to name but a few; there are many, some more scientifically sound than others.

A new method for the description of individual differences in personality is offered in this article; that is, the classification of personality factors in terms of what a person does in meeting common problems of everyday life. To the phrase "Life is a series of problems," might be added "And by one's temperament we mean the characteristic ways in which we meet those problems." Many of them we are able to cope with easily; others take more time and effort; some are never satisfactorily solved.

Since our theme is "problem-solving behavior in conflict situations," it might be well to define what we mean by this. By the word "conflict" psychologists mean the situation that arises when a person (or group) proceeding toward a goal meets an obstacle which hinders progress toward that goal and which does not yield to ordinary ways of overcoming it. A few other definitions may be helpful:

Goal: something satisfying a physiological drive (hunger, etc.) or a learned social motive (approval, etc.).

Obstacle: A competing physiological or social motivation (background stimulation) or an environmental factor (person or thing; even imaginary things) or the lack of something.

Frustration: A condition in which ordinary problem-solving methods have failed, but continuing or recurrent strong motivation forces the person to persevere in his search for a solution; often results in highly emotional (unorganized) behavior, which lasts until an efficient solution is hit upon.

WITH this background, then, let us consider the following examples, which portray the types of behavior common in frustrating situations. Imagine five men, all striving for the same goal, all faced with the same obstacle. The usual problem-solving methods having failed, each man turns to a different type of behavior with which to meet the conflict.

The first man may employ what is known as the "direct attack"—the most probable reaction. His is a habitual approach, with two possible variations: he may continue toward the goal, compensating for his difficulty by added effort, but still applying his original approach even though it has previously failed; or he may compensate by an improved method of approach, evolved by trial and error or some other means that has pointed to a better way.

Man No. 2, perhaps, utilizes the "flank attack"—a detour toward the original goal. Rather than attempting to break through or surmount the obstacle, as the first man is doing, this man *goes around* the obstacle by a circuitous route.

The third man, let us say, turns to "escape." Since no method of overcoming the difficulty is apparent to him, he simply gives up any active attempt to solve it and diverts his energy into other directions. He does this either by denying the very existence or importance of the problem, thus ridding himself of it, or he stalls for time with the hope that something will happen to relieve him of the necessity of doing anything further.

Our fourth man, finding himself blocked, applies the "diversion" method. He diverts his attack from the pursuit of the original goal to either aggression against the *difficulty*, as an end in itself—a largely emotional outburst as a relief from tension during frustration—or he distracts his own attention or that of others from the difficulty by drawing a red herring across the trail.

Our last man compromises—he applies the method termed "partial solutions." Unable to solve his problem, he resorts to an attempt to deal with one or more *parts* of it. There are a number of partial-solution mechanisms to which he may turn. The importance of understanding these mechanisms lies in the fact that they are the common *partial* solutions hit upon by people in difficulty. They are, therefore, descriptions of alternative courses of action for situations in which direct analytic approaches are either not known or too expensive in time and energy.

Common among these alternative courses are:

1. Rationalization—distorted reasoning about the problem.
2. Substitute activities—redirecting motivation into other channels.
3. Identification—joining the company of persons who do succeed and basking in reflected glory.
4. Projection—putting the blame for failure on some other person or thing.
5. Regression—reverting to the role of a younger, less experienced, or invalid person who could not be expected to solve the problem.
6. Fantasy—daydreaming imaginary solutions in a dream world instead of facing realities in the situation.
7. Overcompensation—developing motivation for original problem far more than is necessary.
8. Dissociation—formation of logic-tight compartments; splitting up the problem and dealing with one or several parts separately.
9. Egocentrism—getting some kind of personal attention, even if unfavorable.
10. Sympathism—getting someone else to assist with the problem, or at least to pay some attention to you if you fail yourself.
11. Negativism—refusing to act at all, or being contrary about the problem.
12. Repression—inhibiting overt activity by directing motivation to implicit activities, thinking and visceral (emotional) activity.

Our five men do not always retain their individual methods; finding one solution unsatisfactory, perhaps, they shift places, jumping about from one procedure to another until they find peace of mind. Further, no one method is appropriate for all situations. The objective of a well-adjusted person should be to use each method in the particular situation for which it is especially suitable, changing his type of approach as the problem itself changes.

The methods of solution that we devise to meet problems as they arise are a very direct manifestation of our individual personalities. The ways in which we attempt to meet our problems largely determine our success in solving them, and thus determine to a great extent how well we win social approval or vocational effectiveness. Two persons will rarely solve the same problem in the same way, and it is rare, too, that a single individual is able to solve all his problems by the same method.

Does it not seem logical, then, to base the identification of our personalities on our own pattern of problem-solving methods? The old concept of personality "types" really did much the same thing except that its protagonists failed to recognize the wide and practically continuous variations between the diversified patterns of problem-solving approaches that they called the symptoms of a syndrome; they failed also to realize that these patterns of mechanisms are not determined by the individual's temperament, but that they *are* his temperament.

What is the significance of this system of classification? Would the establishment of such a system be an aid to the counselor in his work? The authors have employed it in several ways, the simplest being discussions designed to assist students of elementary psychology in planning a career. These students were asked to describe what they would like to achieve in each of five areas of endeavor: education, vocation, avocation, leadership, and personal development. It was found that they could do this fairly easily, particularly when they were urged to set goals for which they would really be willing to work if they thought there was some reasonable possibility of success. After listing the advantages and handicaps they possessed for achieving each of the five goals, they were asked to describe what methods, if any, they had employed up to the present time to achieve each goal. If these did not appear productive of the desired results, they were then asked to examine the classification for alternative methods that might overcome the difficulties so far encountered.

This procedure is being elaborated by Dr. A. C. Van Dusen, of Northwestern University, in the form of a booklet designed to assist a student or his parents in formulating his plans for a career as far as possible before seeking expert assistance (where needed). This is based on the idea that the formulation of a problem is often more than half the process of solution, and that a systematic method of procedure may save both the student and his advisers a great deal of time.

Another collaborator, Dr. Eileen Sinclair, has employed the same general approach to problem solving for girls confined in an institution for juvenile delinquents. She, also, began by having the girl formulate the problem that had produced her difficulties, and then analyzing what she did to meet the situation. Obviously, this involves the building up of rapport with the psychologist, and the process of talking over the problem has the usual benefits of mental catharsis. It goes beyond this, however, in attempting to give the girl a clear understanding of the new situations she will have to face upon parole, and in the selection and development of problem-solving methods for dealing with each such situation.

Perhaps the most important factor of this system of classification for clinical psychology is the fact that it attempts to analyze personality characteristics in terms of *habitual methods of responding to the principal types of situations every person faces.* The end result should be an individual profile of the extent to which a person employs each main type of problem-solving method in dealing with his major problems.

An analogy might be drawn between this method of counseling and the work of the motion-study expert. Consider two drill-press operators; both men perform exactly the same job on identical machines, yet the output of one is many times greater than the output of the other. A motion-study expert is called in to analyze the job, and he finds that the slower man is using the wrong work method—the wrong approach to the problem. How easy it is for the expert to spot the wrong method and to correct the operator, thus making the job considerably easier and at the same time increasing the output.

The same thing applies to the counselor—with a knowledge of the classification of problem-solving behavior, he can steer his troubled client out of the frustrating situation in which he has placed himself.

The fact that such a classification is not a static one is perhaps one of its most hopeful features; it breaks down such large and vague concepts as "social maladjustment" into smaller categories and offers methods by which an individual can attack the particular problems in his social situation. It goes beyond diagnosis and places emphasis on the provision of technical abilities for assisting the individual to deal with his problems more adequately.

Both the consultant and the client can contribute actively to such counseling, and if the alternative methods of problem solving are made clear, there is greater probability of the client being able to develop skill in his methods of handling future problems himself, or at least becoming able to recognize when he requires expert assistance. In either case, this type of diagnosis tells him what he is doing that is inadequate in a given situation and suggests a complete classification of the various alternative methods he might employ. Changing to another method of approach is the principal means of getting out of a rut and onto the path toward solution of the problem.

CONFLICTING DOCTRINES ABOUT SOILS

CHARLES E. KELLOGG

Dr. Kellogg (Ph.D., Michigan State, 1929) served as research assistant in soils at Michigan State during 1925–26 and studied on a fellowship there until 1928, when he became a soil scientist for the Wisconsin Geological and Natural History Survey. He taught at the North Dakota Agricultural College from 1930 until 1935, when he was made chief of the Division of Soil Survey, U.S.D.A.

THE general public has recently discovered soil. For centuries country people have known soil—their own kind of soil. They have loved it, cursed it, sweated over it, and died for it. Their knowledge passed on from parents to children over the years. Also, a few early scientists studied soil, but only a few prior to about 1900. There was another thirty years before the public generally became deeply interested.

This is not to say that no scientific studies of soils began before 1930—not at all. The work of the famous Rothamsted Experimental Station in England, devoted mainly to soil study, was begun in 1843. The great Russian school of soil science originated about 1870. The American Soil Survey will be fifty years old in 1949. Most state experiment stations in the United States were making some soil studies by 1900, and several considerably before that date. But public support was meager. Much of the work consisted of simple tests directed toward immediate goals of increasing crop yields. But not all of it. A few investigators did raise fundamental questions: What *is* soil? Why are soils so unlike the rock beneath them? Why do soils vary so much from place to place? What accounts for their productivity for plants? Why are some good and remain good while others deteriorate?

The practical results of this work have more than paid for the cost—many, many times more. This was always expected. Yet it is a strange thing, perhaps, that people have recognized the cultural value of fundamental scientific research in what might be called the more "classical" sciences, such as astronomy, botany, or physics, and support such research generously without the promise of immediate practical returns, but rarely will they do this with fundamental soil science. Soil research must promise in advance to pay its way in the garden, field, or forest, and soon. But any scientist knows that applied science cannot get very far ahead of fundamental science.

Today people in the city as well as in the country are concerned about soil. They have been persuaded that the security of our country depends upon how our soils are used. Although we have had bad gullies for more than a hundred and fifty years, only a few people saw them before. Now what should be done? Questions are asked about world soil resources. That is, is the area of productive soils in the world large enough to feed all the people even if, by some miracle of statesmanship, the economic and political barriers to use could be removed? And what of the quality of our foods grown on different soils, and with different chemical fertilizers? Is it getting poorer? Gasoline motors are replacing horses. Can soil be kept productive without animals? Do we plow too much with the new machines?

In highly emotional terms, many popular books and magazine articles of the middle and late thirties stressed the catastrophic dangers of soil neglect. Some even asserted that soil deterioration was a primary cause in the decline of older civilizations. Most of these advanced some panacea, usually a simple cure for the "simple" problems. But since there were several panaceas, at least some of them must be partly wrong. Then, too, the soil is anything but simple, to say nothing of the people who live on it.

These trends continue—many panic makers and corrective panaceas. But the public is becoming a bit wary and demands more detail. For one thing, the experiences of victory gardeners helped. City people took the opportunity to try managing a soil. Panaceas were put to test. Many found that although soils may be stubborn or yielding, they are rarely simple. American farmers increased enormously their use of scientific principles during the past war. All this demand for specific facts and principles is good, even though many answers are still wanting; for the first step toward wisdom is an awareness of something to be known.

LET us look at a few of the current doctrines about soils, remembering when we do so that each has some basis of important fact and that many of the advocates of each are well intentioned and sincere. Correct appraisal is made difficult by the extremists. Any good idea, good in its relationship to other good ideas, can become bad, even vicious, when carried to absolutism. Then, all of us are likely to have at least some prejudices, possibly unconscious ones, hard as we try to avoid them.

The balance-sheet theory. We may start with the balance-sheet theory—the-soil-is-like-a-bank idea. This doctrine is not new. Quite the reverse. It was clearly stated by the great German chemist Justus von Liebig nearly a century ago.

In reaction against the more or less mystical notions that plants can change one element into another and so on, he explained the sources of plant nutrients in straightforward terms of the new chemistry. Plants require certain nutrients from the soil: calcium, nitrogen, potassium, and the like. If we analyze the soil and find what it contains, then, knowing the need for crops, we can calculate how many crops can be grown or what fertilizers to apply. With sarcasm and invective, Liebig swept away notions of plants feeding directly on humus or changing one element to another. Crops will grow, he said, in direct proportion to the increase or diminution of the nutrient elements in the soil. He re-emphasized the law of the minimum: where several factors are essential, plants will grow in direct proportion to the least favorable one—the limiting factor.

Just before his death, Liebig became unhappy over his theory. For one thing, his fertilizers were not successful. Then he went down the Danube Valley and saw soils which, according to his reckonings, should have long been sterile, but which were producing more than before, without fertilizer. Nor could he account for the nitrogen in certain plants. This last failure was nearly a deathblow, but with the discovery of nitrogen-fixing bacteria growing on the roots of legumes, the theory recovered. Thus, it is curious that a biological discovery saved the day for a purely chemical theory.

Many people believe this theory. And why not? It is so simple and so reasonable. That is, it would be if the implied concept of soil were true.

Neither Liebig nor his followers examined real soils out of doors. They didn't need to! Once the simple idea of the balance sheet was accepted, the soil *had* to be a static storage bin of plant nutrients. The early geologists accepted this notion, too, and filled the storage bin with ground rock—sandstone, granite, glacial drift, or the like, resulting in "granite soil," "glacial soil," and so on. A few other features were recognized. Some soils were rich in clay—"heavy" soils to plow—and others rich in sand—"light" soils to plow. And, with the rise of biology, organic matter and microorganisms were added to the mixture but without fundamentally altering the concept.

It was inevitable, of course, that it should occur to someone to look at the soil, dig into it, and see whether it was really like it had been assumed to be in the balance-sheet theory. Credit for much of the early work on the soil itself belongs to the Russians. They found that soils in nature are made of a series of layers, or soil horizons, reaching from the surface to a depth of a foot or many feet. Collectively, these horizons are called the "profile" of the soil. In Europe and the United States, soil profiles are usually 2–5 feet deep. They are shallower toward the Arctic and deeper toward the equator.

Not only that, from just one kind of rock all sorts of soils, varying widely in color, chemical composition, productivity, and so on, were found. In fact, kinds of soil were found to be more closely related to climate and vegetation than to the kind of rock. Whereas rocks are distributed more or less promiscuously over the earth, as far as the present environment is concerned, areas of individual kinds of soil occur in an orderly, discoverable, geographic pattern. Each distinct combination of climate, vegetation, parent rock, relief, and time—called the five genetic factors—gives a soil with a unique set of characteristics—a soil type.

This is a far cry from a simple storage bin. As the surface horizon of a sloping soil is gradually eroded away, the upper part of the horizon beneath is gradually changed and incorporated into its lower part. In turn, the upper part of the third horizon is changed and becomes the lower part of the second, until finally new, fresh minerals from the rock beneath are incorporated into the lower part of the lowest horizon. Other soils, instead of having fresh minerals added to the bottom, have them added to the top. Silty coverings are left by the overflow of rivers onto the surface of young alluvial soils like those along the Nile. (Probably a third of the people of the world get their food supply from alluvial soils.) On many soils dust and volcanic ejecta fall. As air enters and leaves the soil with wetting and drying, some gases are expelled and others absorbed. Rain brings soluble materials from the air, and percolating water leaches out materials. These actions change the soil materials because the various minerals and other compounds vary in solubility and rates of decomposition.

And so it goes. These and other physical and chemical processes are modified profoundly by plants, animals, and microorganisms. Nutrients are collected by the living matter, held against the forces of leaching, and returned gradually to the soil.

Thus, a soil is a solid, changing piece of landscape. Its upper surface is the surface of the land; its lower surface is defined by the lower limits of biological forces; and its sides are boundaries with other kinds of soil where there is a change in one or more of the five genetic factors. There are changes between day and night, from one rain to the next, from season to season, with plant successions, with shifting climate, and over periods of geological time—to say nothing of man's interference.

Thousands of distinct kinds of soil exist in the world. Each has its own set of characteristics, its own combination of environmental factors, its own dynamic, and its own potentialities for response to management. Some can furnish enough calcium, potassium, phosphorus, iron, or other mineral nutrients for crops indefinitely. Others respond greatly to one, or perhaps several, nutrients when first used. In some soils, nutrients become available to plants as fast as they are withdrawn; others are soon exhausted. Nor is the ability of a soil to furnish any nutrient measured by the total amount present.

This brief recital touches very lightly on just a few of the better-known processes. And yet some people, even scientifically trained people, can still be heard to say: "A soil is like a bank. One must put in what he takes out." What plants require for growth and what soils contain are relevant, but these values can help us significantly only where we know a great deal more about the soil. Because a soil in Maryland gives a good response to some fertilizer is no proof that one in Iowa ever will, regardless of how long it it cropped.

This balance-sheet theory does a lot of harm. Its superficial appearance of reasonableness delayed fundamental soil science for many years. It oversimplified the soil in terms of analytical chemistry to the neglect of the physical and biological characteristics and other qualities influencing soil productivity as much as fertility. It has delayed the recognition by farmers that different soils should have different fertilizers according to their nature. Even now, farmers generally are using mixed fertilizers, containing nitrogen, phosphorus, potassium, and perhaps other elements, as a sort of "shotgun" treatment on soils where only one or two of these nutrients are needed. Efficiency in the use of fertilizers demands that those nutrients be used that are needed to supplement the individual natural soil in order to make it a productive, arable soil for the plants to be grown. But a believer in the balance-sheet theory, whether fertilizer technician or farmer, avoids a lot of thought and trouble. He doesn't need to look at the soil or study it. It's like a bank.*

The erosion menace. The followers of the balance-sheet doctrine practically ignored soil as landscape. Many early agriculturists, like Jefferson, for example, emphasized the characteristics of soil. He invented a plow—the "plow of least resistance"—and practiced contour tillage to prevent excessive washing, or erosion, on the slopes. From time to time, later agriculturists and geologists studied this problem of soil washing and directed attention to the great gullies already eating away at good soils in parts of the United States. For years, scientists in the American Soil Survey emphasized the problem in reports on many counties where it existed, hoping some action would be taken, but few people were interested.

Then, about 1926, a few popular articles on soil erosion attracted public attention. The success of these stimulated others. Soon many articles and books were appearing on the dramatic story of catastrophic erosion. Pictures of great gullies were printed along with statements of the cost of erosion to the United States. Estimates were made of the plant nutrients—phosphorus, potassium, calcium, and the like—washed as soil material into streams, lakes, and oceans. Assuming that all these are available to plants, assuming a value for the chemical nutrients equal to that of commerical fertilizer, assuming that all sediment is useless, and assuming that the material exposed by erosion is all sterile—cost figures become truly astronomical.

On top of a bad business depression came the severe drought of the thirties and inevitable soil blowing in many parts of the Great Plains. By many this was regarded as soil erosion too—wind erosion. So to the pictures of deep gullies and washed slopes were added those of the skulls of starved cattle on windswept plains and of abandoned homes, half concealed by drifted soil.

For a time, it almost seemed that each popular writer was trying to outdo the others in dramatic statement. Some even went so far as to assert that erosion had previously swallowed whole civilizations; and others, that the League of Nations caused the great erosion catastrophe; or that we should tear up our democratic institutions and do what was necessary at once to save ourselves; or that country people are too ignorant anyway to learn how to handle the soil!

A few even went a little further, but not much—there wasn't any further to go. But today's writers are more sober. The great production record of American farmers during the past war has had some steadying effect. But, more important, a very large number of people are now acquainted with the problem at firsthand. It is sincerely to be hoped that the period of extreme statement on soil erosion is nearly over, else it might stimulate some counteraction just as extreme. Excessive erosion—accelerated soil erosion—*is* a serious problem in parts of our own country and in many other places in the world. Like other soil problems, it has been long neglected and it demands attention. Unless farmers are careful, there will be bad soil blowing again during drought years in the Plains. Another period of very low farm prices will amost certainly be accompanied by serious soil erosion.

Not all spectacular erosion is caused by man's use of the land. Much of it is a perfectly normal concomitant of mountain building and wearing down. Nor is it all harmful; in fact, an important part is essential to the formation of productive soils. One cannot, or should not, try to stop erosion, but rather to control it. On the other hand, some of the erosion least conspicuous to the casual observer is most harmful.

The effect of a removal of the surface soil horizons by erosion varies with the soil type. An otherwise productive soil with a massive clay pan or hardpan at 18 inches may be severely damaged by the removal of 6–12 inches of surface soil, whereas one with a deep friable profile may not be influenced at all, provided the process is stopped short of gullying. In fact, the more serious erosion injuries to soil are not chemical, not the loss of plant nutrients, but the change in structure, the loss of crumblike surface horizons and the exposure of massive clays, hardpans, or even solid rock.

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* The confusion is heightened by an old fertilizer trade slogan, "Feed the plant not the soil," along with substitution of the incorrect term "plant food" for both plant nutrients and for that portion of a commercial fertilizer that consists of plant nutrients. Of course, the food of plants consists of organic materials synthesized by green plants from the nutrients taken from the air, water, and soil.

Why do arable soils erode or blow? Generally, it may be said that a thick-growing vegetation has been replaced by a weaker vegetation or by widely spaced plants, thus exposing the soil to the forces of rain, running water, and wind. This can be brought about by drought, fire, cutting, close grazing, excessive cropping, and tillage. On any one soil type, the hazard of erosion depends upon the sharpness of the rains, the force of the winds, the permeability of the soil and its capacity to take in water, its slope, and its productivity for plants. Under clean cultivation, some soils with only very gentle slopes erode badly. At the other extreme, some steep soils are so pervious and productive that they may be cultivated safely. On highly fertile soils, a protective cover grows quickly. And so we might go on. Thus, specific answers to questions of erosion hazard or effects must be worked out for the alternative management practices on specific sets of soil characteristics, or soil types; and in detail, since these practices are parts of the management plan of very small units—fields and farms.

How can erosion be controlled? Although one should not push the analogy too far, this is like asking: How can a headache be stopped? That is, a headache is usually a symptom of some deeper maladjustment. So it is with harmful erosion: one must get at the cause and correct it. Concentration on mechanical devices alone to control the actual erosion itself may be fruitless—even injurious to the soil—if some fundamental causal factor like phosphate deficiency is neglected. Back of the phosphate deficiency we may find any one of a dozen reasons.

Generally, when a rural population becomes poverty-stricken, it fails to maintain its soil. An exploited people pass on their suffering to the land. Low prices, disease, and wars are all important causes. Things get on a hand-to-mouth or year-to-year basis. It is hard to look ahead of the immediate income. Where farmers can take a long view of production over a period of ten years or more, there are very few instances of conflict between those practices that give most return and those that maintain the soil from loss of fertility, erosion, destruction of soil structure, or other deterioration—and not only maintain it: a large part of the arable soils of the world are made better by good farming than they were naturally.

Then, too, when people move to a new area, onto a new kind of soil, the old practices and traditions may persist too long. The new soil may deteriorate before they have learned, through trial and error, how to manage it. Fortunately, modern science can shorten such periods or even eliminate them.

Thus, soil-erosion control is not an isolated problem. It is one important aspect of soil management for secure production in many places. Control must be sought within the whole management system of the land. It would be unfortunate for soil erosion to be neglected as it was in former years. It would be unfortunate for it to be exaggerated or overemphasized at the expense of other problems of soil use. Above all, concentration on the erosion itself to the neglect of its causes could be wasteful or even harmful. Many of these causes go back to economic and institutional maladjustments between soil and people. Fortunately, the trend is now strongly in the direction of a more balanced soil-management program in our own country, and in many others as well.

Although soil scientists may continue to regret the extreme statements about erosion, they should admit that the dramatic stories about it did much to make the public more conscious of soil problems than ever before.

The organic schools. For years most practitioners in the fertilizer field accepted the balance-sheet theory. Only recently have many of them sought an understanding of soil types. We have already seen how that doctrine viewed the relationships between plant roots and soils largely in terms of analytical chemistry. Even soil scientists themselves have given soil biology a relatively small place, except for the microorganisms concerned with nitrogen fixation.

Partly as a reaction against the simple

balance sheet, biological theories of soil productivity have arisen that lay all emphasis on living plants, soil organisms, and organic matter. Greater recognition of biological factors in soil management has been long overdue. But the advocates of the organic-matter doctrine go very far. They insist that organic matter is everything, or nearly so; that the usual chemical fertilizers are downright poisonous to soils; that the liberal use of compost gives special qualities to plants—they will be free of insects and diseases; and that animals, or even people, will be ever so much more healthy by eating plants grown "the organic way."

Within the organic group are separate schools. Some derived their notions from Goethe, by way of Rudolph Steiner; others from experience with compost in tropical countries. Important as these differences may be to the devotees themselves, they need not concern us now. Many of the recommended practices are excellent, although not always for the reasons given.

Organic matter in soil is a very general term. It includes the living forms—roots, fungi, bacteria, and small animals; fresh remains of living matter; a more or less stable decomposition product, brown or black, called "humus"; and a host of intermediate products. The final decomposition products are, of course, water, ash, carbon dioxide, and a small amount of other gases. Perhaps the great importance of organic matter may best be realized by listing its functions in soils. The relative significance of the several items varies a great deal among different soil types. In no soil are all important at once.

1. Organic matter promotes granular structure and pore space in some soils. Thus, it may—
   - *a*) aid root extension;
   - *b*) promote entry of water into the soil;
   - *c*) reduce soil washing;
   - *d*) reduce soil blowing;
   - *e*) promote aeration or exchange of gases;
   - *f*) increase the water-holding capacity; and
   - *g*) reduce baking and crust formation.
2. It reduces the extremes of temperatures, especially high summer temperatures, when used as mulch.
3. It reduces evaporation, especially when used in the surface or as a mulch.
4. Humus aids in the maintenance of reaction (pH) in the soil by acting as a buffer.
5. Organic matter aids in the retention of soluble substances, including many plant nutrients, by holding them in living or nearly fresh forms against the forces of leaching and by the base-exchange properties of the humus.
6. Part of the organic matter furnishes a food supply for microorganisms and small animals in the soil, including forms essential for the transformation of nitrogen compounds and for other processes important in plant nutrition.
7. Organic matter furnishes directly, and indirectly by promoting bacteria and fungi, complex organic compounds which may include both growth-promoting and antibiotic substances. Very little indeed is actually known about the role of these compounds in soil productivity.
8. Additions of organic matter, especially from normal plants of mixed types, maintain a slowly available, fairly well-balanced supply of plant nutrients, including the micronutrients. This is very important everywhere, but especially so in warm humid countries where leaching is severe and fertilizers expensive.

Where organic matter is used to control temperature and evaporation, relatively fresh material is used as a mulch. When used wholly or partly as a source of nutrients, the material is often partly decomposed in advance. Such partially rotted material, or leaf mold, may be found naturally just above the mineral soil and under the surface layer of dry leaves in the forest. Commonly, it is made as compost.

The devotees of the organic schools have made rituals out of compost making. Fine points are argued about as heatedly (and with about as much reason) as recipes for mint juleps or Virginia ham. Some add special "secret" concoctions, to encourage certain microorganisms, it is said. These "secrets" are passed on only to the most trusted of the faithful. Most insist that at least a part of the mixture must be animal products. Although soil scientists generally advocate adding chemicals to the mixture, nearly all true believers condemn their use. The unforgivable sin is to add any "chemical" nitrogen, such as ammonium sulphate or ammonium nitrate, to soil or compost. Rock phosphate may be used; but superphosphate is condemned, bcause it is made by treating a natural earth with sulphuric acid—a strong poison. (Even that should not be so bad as common table salt—a resultant from two strong poisons!)

At great risk, we may state that the essential features in making compost consist of piling things like leaves, grass clippings, straw, and other plant remains in heaps some 4–6 feet wide, 6–12 feet long, and 4–6 feet high, let us say. Very thin layers of soil separate layers some 6–8 inches thick of organic matter. The material is kept moist, but not soggy, and allowed to decompose. If turned occasionally, it will be improved in both chemical and physical properties. If nitrogen is added as animal manure or fertilizer, the yields of good compost will be higher. Some ground limestone and superphosphate or rock phosphate are added. Many use a little mixed fertilizer.

The principal point in the process is the reduction of the ratio of carbon to nitrogen. In normal soil, this ratio is in the neighborhood of 10 or 12. In the fresh organic matter, it is much higher—30 or 40, or even more. If the fresh material is added to the soil directly, microorganisms will increase enormously in the soil, using the energy-giving carbohydrates. They require nitrogen for their bodies and will compete with plants for the available supply. Of course, when the carbohydrates are used up, the numbers of microorganisms will again return to normal and the nitrogen in their bodies will be available to plants. But by composting in advance, this initial period takes place outside the soil, and the resulting compost contains nitrogen available to plants. Further, decomposition in the soil of fresh organic matter requires more moisture than is sometimes available. The compost is in much better physical condition to use than fresh organic matter.

The ratio of nitrogen and carbohydrate, on one hand, and phosphorus, on the other, must be watched also. If both nitrogen and carbohydrate—energy material—are high, microorganisms will greatly increase and use a lot of phosphorus in their bodies. If the available supplies in the soil are low, plants will suffer phosphate deficiency. Thus, partially composted organic matter, rich in nitrogen, may give bad results unless the compost is enriched with phosphatic fertilizers or generous amounts are supplied to the soil.

Of course, the nitrogen in a chemical fertilizer is not different than the nitrogen in compost. But the organic schools have an important point, often missed because of their extreme statements: If chemical nitrogen is used as a *substitute* for maintaining a reasonable level of organic matter and for growing legumes, the soil may deteriorate badly through loss of structure, and the erosion hazard may be increased. Then, too, it is entirely possible to use too much nitrogen fertilizer and have an excess of soluble nitrogen in relation to the other plant nutrients. People often do so in their gardens, getting as a result tall, weak, poor-fruiting plants. Many things good for people or for soil, in moderation, are harmful in excess. Yet, if on a well-managed productive soil, one may raise the yield of corn from 30 bushels per acre to 75 bushels, let us say, with a commercial nitrogen fertilizer, it would be unwise not to do it.

The organic partisans exaggerate nearly as much as the erosion enthusiasts. All sorts of claims are made for disease resistance among plants treated with compost (made according to the particular author's ritual). Plants need all the elements the scientists say they do, some admit, but go on to insist that they need also a mysterious life-giving substance, a vital principle produced only by other living matter. Much that is written is pure speculation. Although, like Berkeley's theories, one cannot prove them wrong, there is no reason to believe them.

Such exaggeration and speculations are unfortunate. A great deal that is advocated by the organic schools is sound practice, even if they do often give unbelievable reasons. Certainly their implied concept of the soil is as good as, if not better than, that of their arch-enemies, the balance-sheet advocates.

Since much work is involved, these organic theories appeal most to small, hobby gardeners, especially those working with poor soil. And under such conditions, results will very likely be much better with the organic methods than with those of the balance-sheet school. But such a choice is not necessary. Both involve some good practices and some unnecessary ones. And both fail to emphasize the selection of plants best able to grow on the soil at hand. In ordinary large-scale farming, animal manure or nitrogen fertilizers may be applied along with organic matter to offset the effects of high carbohydrate content and avoid the need for composting. In the United States composting would need to show a great benefit indeed to make the method economically practicable. Considerable success is claimed in the humid tropics with "organic farming" where labor is plentiful, where organic matter is especially important, and where fertilizers are very expensive or difficult to obtain.

The organic devotees are primarily interested in production. They have not teamed up much with the erosion enthusiasts except to use their astronomical figures of damage to "demonstrate" the evils of chemical fertilizers and of failures to farm the "organic way." They also use the statistics on human disease to show the same thing.

As with the erosion problem, perhaps it takes exaggeration to bring recognition of the biological side of soil productivity, and such recognition is important. Here, too, there seems to be somewhat more reasonableness coming into the current popular articles, especially in the United States.

Earthworms. Enthusiasm for earthworms, among the many soil organisms, can scarcely be accounted for on scientific grounds. Perhaps it is because they are slow in their movements and large enough to be seen easily. Somehow they do seem more friendly than their quicker, many-legged associates. Most people who like the soil at all like earthworms.

For plants to get the nutrients in organic matter, it must be decomposed. Humus is the result of partial decomposition. The role of decomposer is played by a whole host of soil organisms. The large ones also mix the organic matter with the upper mineral soil, dragging bits of it down into holes in the ground. The friendly earthworm is only one among the many.

Years ago Charles Darwin wrote a whole book about earthworms and their influence in forming "vegetable mold" in the surface of the soil. It is perhaps still the best book. He measured some worms and their casts and showed that over a long period their effects were considerable. Farm and village boys digging earthworms for fish bait, look for them in fertile garden soil. But not all productive soils have earthworms, and they are sometimes found in soils of rather low productivity; still one is more likely to find them in productive soil. Even now there is no definite answer to the question: Are the earthworms present because the soil is productive, or do the earthworms make the soil productive? Probably it is some of both.

Recently, great claims have been made for soil improvement by earthworms, usually as a special phase of "organic" gardening, since they need organic matter as food. Here again the soil scientist must say that he does not know. Possibly earthworms do have some special advantages over other organisms; possibly they may be harmful where soil organic matter decomposes too fast anyway. The scientist cannot say it is bad practice to destroy them or good practice to introduce them. Yet even the scientist probably likes them in his garden—most people do.

Plowless farming. More than two hundred years ago Jethro Tull invented a grain drill and a horse hoe. Grains were planted in rows and cultivated, and yields were increased greatly over the older broadcast sowing. Tull taught that this benefit came from the very pulverizing of the soil, making it easier for the fine earth to enter the roots. This view has been long rejected. The great benefit from Tull's system came largely from the better control of weeds, which had become very bad in English grainfields.

A tradition arose, however, among European and American farmers that it was beneficial to plow and to cultivate a great deal. The invention of the steel moldboard, or turning, plow has been regarded as a great milestone in soil management, coming as it did during the breaking of the American prairies. It made plowing easier and neater. As cultivation spread west on the dry soils of the Great Plains, it was emphasized as a technique for saving water. Under the "dry-farming" system advocated at the end of the nineteenth century, crops were alternated with cultivated fallow. During the fallow year, moisture accumulated and a crop could be grown the following year. By maintaining a very loose surface soil—a "dust mulch," it was called—capillary rise of water to the surface was thought to be broken and the upward movement of water halted. But more recently it has been shown that here again the principal benefits came from killing weeds which robbed the soil of moisture. The farmers who went into the plains took their tools and methods with them, including the turning plow. They had to learn other methods.

For a long time soil scientists have been arguing against too much tillage. Obviously, unnecessary tillage wastes power. But, even more, tillage tends to destroy soil granules, to injure soil structure. The effects are, however, more striking in some soils than in others.

A few years ago came a new theory, full-blown overnight: Stop plowing altogether! It was supported by all sorts of carefully selected evidence. For one thing, how about the great dust storms of the early thirties? Had not the popular books on erosion shown that the moldboard plow was the evil instrument that had destroyed the grassland? Was it not the moldboard plow that tore up the slopes in the humid region now suffering from sheet wash and gullying? Is not the forest productive without plowing? Assertions went almost to the point of implying that all soils in America were originally productive, but that they vary from one another now because some have been plowed more than others.

Seeds can be planted and plants may be grown without plowing, or even with very little tillage of any sort. If plant remains are left on the surface, not plowed under, the soil will be protected against the forces of rain and wind. According to the advocates of plowless farming, soil organisms thrive better, plants are healthier, yields are much higher, and, of course, costs are lower. In fact, as plowless farming becomes general, the really big problem will be what to do with all the great increase in farm products!

So the question arose: Why plow? Look at the bounty of nature! And does she plow? Let each farmer answer for himself: Exactly why plow?

Now, this was a good question. A lot of people, agricultural experts as well as farmers, did not have a ready answer. Most farmers had plowed because it was more or less traditional to do so. For centuries, many farmers had plowed. Most of the standard books on soil science explained how to plow and when to plow, but few explained why one should plow at all.

The truth is, however, that already many tillage machines were in use instead of the turning plow in the Great Plains—machines like the one-way disc and the duckfoot cultivator. Many bulletins had already been issued to farmers explaining the use and advantages of such machines over the turning plow. Curiously, these were ignored for a time by both advocates of plowless farming and most writers of books on soils.

In the face of such claims, and with few clear reasons against them, some farmers and gardeners in the eastern part of the United States—especially city gardeners—put the theory to test. On the whole, results have been disappointing. Some took the doctrine literally and planted corn in fields of Bermuda grass—corn that got a few inches high, turned yellow, and finally perished.

Of course, plowless farming worked successfully on a good many kinds of soil, as alert farmers had already discovered. In fact, machines were already available for carrying on the necessary tillage for controlling weeds and preparing seedbeds that would leave trash and clods on the surface to protect soils from washing and blowing. But these were different soils than those traditionally plowed in the humid regions.

The kind of tillage required depends upon the crops to be grown and especially on the kind of soil. Most soils in humid forested regions are not naturally productive for crop plants. Among the very poorest are nearly level, deep, stone-free soils with clay pans or hardpans. The term "worn-out soils of the East" often refers to these. But they aren't worn out; they never were productive. On the other hand, the black soils developed under grasses in the subhumid region are naturally productive for similar plants—crop plants.

So, in humid forested regions like Eastern United States and Western Europe, most soils highly productive for crop plants must be made so by the farmer from relatively unproductive natural soils. Soil fertility must be increased. Soil structure must be improved, made granular, to a considerable depth. This means microorganisms and roots need to be encouraged. They require organic matter, lime, and fertilizers, not only in the surface horizons but also in the deeper ones. Therefore, one plows as the best way to accomplish this purpose.

There are other reasons. On many soil types, systems of tillage involving the use of the turning plow are best to control weeds, to prepare a seedbed, and to make the soil receptive to rain water. On other soils such deep turning is not necessary, or even proper, and systems that avoid the turning plow are better.

Aside from the disappointments and a few financial losses from trying plowless farming on soils where plowing is essential, the net effect of the doctrine has probably been good, absurd as its claims and reasons turned out to be. It jarred lots of people who had just taken plowing for granted into considering tillage critically. Was plowing necessary? Tests were made, and the results of older tests and experience re-examined. The inevitable result has been that plowing is found to be necessary on many soils and not on others. Farmers generally—of course not all—*had* plowed and cultivated too much.

As a result of the controversies that the doctrine stimulated, at least a few more people have thought about soil and now realize a little better why they do what they do.

Apparently the extreme doctrine will disappear almost as quickly as it arose, but more quietly.

"Balanced" minerals. Scientists in plant nutrition have been fairly well agreed for some time that at least fourteen elements are essential for plant growth. These include carbon, hydrogen, and oxygen from the air and water; and phosphorus, potassium, nitrogen, calcium, magnesium, sulphur, iron, boron, manganese, copper, and zinc from the soil. Certain soil organisms can use nitrogen from the air. Somewhat more recently molybdenum has been added to the list, especially for clovers. Only small quantities of molybdenum, iron, boron, manganese, copper, and zinc—often called the micronutrients—are needed, although the actual amounts required vary widely among plants. Common salt (sodium chloride) also stimulates the growth of certain crops on some soils when used as a fertilizer. In addition, iodine and cobalt are known to be essential for all or certain animals, and they obtain these largely from the food they eat.

Besides these, a great many other elements—in fact, most of them—may be found in plants. Some kinds of plants are especially rich in aluminum, gold, silicon, or other elements where certain compounds of the element are present in the soil in considerable quantity. Since these and many other elements are often present, one wonders how they influence the plant, if at all.

Fertilizer practitioners have given greatest emphasis to the "big three" plant nutrients—phosphorus, nitrogen, and potassium. Calcium has been thought to be abundant in nonacid soils and usually taken care of by liming in acid soils. In recent years, however, increasing attention has been given to magnesium, sulphur, and the micronutrients. In many instances, applications of one or more of these have been very beneficial.

Neither plant nor animal nutritionists can explain entirely the roles of the micronutrients that are known to be essential, let alone any possible effects of the large number of other elements (except for some of the established poisons) taken in by plants and animals. Nor are the optimum environmental conditions, including nutritional balances, known for the synthesis of vitamins and other growth-promoting substances.

Could it be that some of these micronutrients, even those having no clearly established functions, are in some way responsible for the synthesis in plants of certain organic constituents of nutritional significance to animals and humans? Even though the scientist would hesitate to say yes, he cannot say no. He doesn't know.

What a field for speculation! Here may be the key to better health and nutrition, even to the long-sought elixir to give us perpetual youth. It is only a step from the scientist's statement that he doesn't know whether these elements have important undiscovered effects to someone's saying: "Perhaps they have"; and finally to the idea "They *do* have."

Some have taken this final step. The claims vary from emphasis upon the micronutrients known to have at least some definite influence on growth to an impressive list of these and many more. The evidence offered includes good production, but not significantly greater than under good nonmysterious management; the fact that plants growing naturally contain such elements; and the improved nutritional value of the plants for man and beast. This last is easier said than proved. In fact, with the very best scientific facilities, it is exceedingly difficult to make accurate comparisons of foods through feeding trials to test such plants.

Research is badly needed in this area. Not, of course, to prove or disprove the claims of extremists for balanced minerals, but to uncover the facts, to find some basic principles of prediction value. There is great danger that before the facts are found much money and effort will be wasted on special fertilizers of "complete minerals." It is known that alfalfa, sugar beets, and other crops are especially sensitive to boron deficiency; tung trees are especially sensitive to zinc, as is the pecan; and so we might go on through a very long and complicated list. But there is another list of plants according to toxicity hazards to many of the same elements; that is, on the same soil, the same amount of boron may increase the yield of one crop and lower that of another.

As important as these plant preferences are two other factors: the available amounts in the soil throughout the root zone; and the general soil-management system—green manures grown, other fertilizers applied, liming practices, manures and compost used, crop rotations, and irrigation practices.

Soil scientists are going ahead slowly. Already methods of diagnosis have been fairly well developed for some crops on some soils. But these are time-consuming and highly technical. Not with even one of the micronutrients are there methods that will work generally.

Additions of reasonably small amounts of these special mineral fertilizers will probably not do much harm, except when a soil already contains a large amount of one of the elements in available form; in some instances, the applications may do some good. But the chances of meeting the actual soil needs are remote. There is no such thing as a generally "balanced fertilizer" in the sense usually implied—balanced for plants growing in soil—for all soils or even for the soils of a considerable area. As soon as a chemical mixture is added to the soil, reactions take place within the soil. The final "balance" of nutrients is quite unlike that added in the mixture. The aim in scientific or balanced fertilization is to add what is essential to achieve a balanced nutrient supply for the plant within the soil. This often requires two entirely different fertilizers, or fertilizer mixtures, for the same kind of plants growing in adjacent fields or gardens on different soil types.

Soil and health. Careful comparisons between soil geography and human geography show many extremely interesting and important relationships. Describing such relationships is one thing, and proving causal mechanisms quite another. In the past twenty-five years or so, several important relationships between soil conditions and nutritional diseases of animals have been fairly well established. These researches have been expanding generally throughout the world.

Questions then naturally arose as to what extent human nutrition and health might be related directly to types of soil and soil-management practices. Several soil scientists and nutritionists alike realized the possibility of important causal relationships. The problem is almost unbelievably complicated by the great number of combinations of relevant factors and the technical difficulties of the research. For dependable results, the study of relationships must begin with the soil type, considering its total environment and cultural treatment, and the plants grown. Then account must be taken of how the plants are processed into food, food storage, combinations of foods used, and so on. Finally, the study must be carried into the medical clinic.

A little evidence is in—about enough to show that some highly important relationships exist but that it is very difficult to obtain principles of real prediction value.

Unfortunately, some practitioners have taken the "long jump" from these conclusions of the scientists to claims of knowledge now. Of course, assertions that certain soil types or certain cultural practices give food of special health-promoting value stimulate the emotional interest of readers. Everyone wants to avoid sickness and be strong.

Such claims are made by some extremists of the "organic-farming" schools. Some advocates of the balanced-minerals fertilizers also promise healthful, even disease-curing, food plants. It has been said definitely that erosion lowers the nutritional quality of food plants. Extravagant claims have also been made for ordinary liming and fertilizing.

Any of these *may, under certain conditions,* be true. Certainly, none is generally true under most conditions. Several significant local problems of great importance to the nutrition of livestock have been worked out. But there are very few proved relationships with humans developed to a point where definite predictions can be made.

But before that happy day comes when the relationships between soil and food quality are well enough known that food can be graded according to quality, based upon soil type and cultural practice, the public will doubtless be presented with many more claims. In other words, dogmatic assertions on soil and health seem to be increasing rather than diminishing. Soil scientists and their colleagues in plant and animal nutrition and medicine have already made a reasonable start on this very complicated research. The great danger to their research right now is public reaction against the claims of the extremists—claims that the scientists find hard to deal with because they do not yet know the facts.

THE COMMON DIFFICULTIES

None of the various doctrines about soils can be declared to be wholly true or wholly false. Very few, if any, soil scientists are clearly identified with any of them. Although the doctrines differ from one another—some pairs very widely indeed—they all have three difficulties in common.

In the first place, their advocates hold a firm conviction—originally from revelation or authority, it doesn't matter—based upon something besides the complete soil. In the balance-sheet theory, for example, it was a notion of plant nutrition. In others, it was some superficial appearance of the land, or even of the very upper part of the soil—the part plowed or turned in spading. After the theory was firmly believed, *then* a concept of soil was developed to accommodate such a theory. In soil science, on the other hand, one begins with the facts about the soil itself, as well as these can be learned with the scientific tools at hand.

Second, all the doctrines imply that soil is everywhere about the same, except as modified by man's use. Because a practice seems to work well in one garden or one field, the extremist generalizes widely. Some have talked about "average depth of fertile topsoil," for example, in the United States or in the world. Naturally, unproductive soils are sometimes called "worn-out" soils. Actually, there are thousands of unique soils in the world, varying from one another in every respect. Some have little or no "dark-colored fertile topsoil" and never did have; others are black to more than 3 feet. Some are highly erosive even with gentle slopes, whereas others are not, even with steep slopes. Many steep, thin, stony soils always were steep, thin, and stony. And so it is with all characteristics of significance to soil productivity and use. "Soil" is a very broad term, like "plant" or "animal."

Third, these doctrines, especially in their most extreme aspects, deal with areas of the subject about which science knows the least.

They nearly always seem to affirm most strongly what scientists cannot deny for lack of evidence.

A GLIMPSE AHEAD

All this should not be taken to mean that soil scientists have no disputes among themselves. Of course they have, as in any science where advances are being made. But such disputes are in different terms than those among the advocates of the doctrines. They deal with facts and their interpretation according to the scientific method.

Nor should it be inferred that soil science is a helpless baby—not at all. It is young and still cannot answer many important questions. But it can and does answer fairly well a great many questions about a great many kinds of soil. Until recently, farmers, gardeners, foresters, and engineers learned about the soil through trial and error, with all its costly failures and headaches. Now, every day, in many parts of the world, soil scientists are making reasonably good predictions about the behavior of soils with many kinds of practices. In the future, they will know how to make these more accurately, for a longer period, and on all important soils. They will develop many new practices and understand many more relationships between soil and use. This goal can only be approached the hard way—through painstaking fundamental research. Then comes the applied research and demonstration.

If these doctrines have helped to make the public conscious of the importance of soil and the need for fundamental research, they, too, will have helped to lay a basis for substituting scientific prediction for habit and trial and error, even if indirectly.

FOR WE ARE ONE

Home is the earth, and all that here abounds,
A princely gift from Time's unending past,

A universe to thrill our opening eyes,
A sun for strength, and with companion earth
To bear us up, and give to all full life,
To bird and beast and every growing thing,
To man, as kin to these and one another.

E'en though our thoughts do reach beyond the clouds
How deeply rooted are we in the soil
That feeds and gives us heritage of power:
Our liquid gold, black diamonds, metal slaves—
As with clay and mind we build a pleasant home.

Yes, all the earth is home, and there is much
To make us one, as one we are, indeed.

THOMAS P. MONAHAN

MOSES IN THE LABORATORY

STELLA B. CAMERON

Born and raised in Oxford, England, Mrs. Cameron studied geography at Edinburgh University. She went to Canada in 1932 when her husband joined the staff of the Institute of Parasitology at McGill. She has always been interested in breeding exotic animals and introduced the golden hamster into Canada in 1938.

IT SEEMS so long ago that rabbits, guinea pigs, mice, rats, and monkeys were first introduced into the laboratory as experimental animals that it would be difficult to say exactly who introduced them and for what scientific purpose they were first used. This is not the case, however, with the most recent addition to the laboratory—*Xenopus laevis,* or Clawed Toad Frog. This article deals with our attempts to have it established on a permanent basis.

Macdonald College, located at Ste Anne de Bellevue, is a residential college of McGill University, Montreal, in the Province of Quebec, and, like so many similar institutions in North America, it was requisitioned during the war by the military authorities to house troops. In this case, the troops were women, and not all the College facilities were utilized by them. Among other things, some of the research activities of the College were continued, although their aim was directed toward war rather than peace.

One day, my husband (who is in charge of the McGill Institute of Parasitology at Macdonald College) received a telephone call from Ottawa. The Deputy Director of Medical Services wanted to know if he had a basement with running water in his Institute and, if so, would he be willing to permit the Army to use it as a pregnancy-diagnosis station. Since many members of the Canadian Army Women's Corps were married, and since the station at Macdonald College at that time housed a great many of these women, it seemed quite a reasonable request; so my husband said "Yes," but he was a little puzzled as to the need for running water. That was necessary for the South African frogs, he was told. A staff would be provided (which was fortunate as many members of his own staff were in the services), and the frogs would be flown over from South Africa in a week or so.

In a few weeks a telephone message came from the Montreal Airport at Dorval (then the headquarters of Ferry Command, Royal Air Force) to say that several cans containing about 300 live frogs had just arrived from Durban, Union of South Africa, and would my husband please do something about them —and quick! He did, and in forty minutes the cans of frogs were in the basement of the Institute. They had had a long flight from Durban—through Khartoum, Cairo, Gibralter, and London to Montreal—and had stood the journey remarkably well, although in later shipments there was a high death rate.

Fortunately, in the interval between the two telephone calls, we had had time to acquire some information regarding these frogs. Several peculiar and unexpected coincidences came to light. Before coming to Canada sixteen years ago we had been living in Edinburgh, where my husband was working in the Department of Zoology at the University. This department was housed next door to the Animal Breeding Research Institute directed by Professor F. A. E. Crew, who had established a pregnancy-diagnosis station for civilian use. He was using small mammals for the test at that time, but, just as we were leaving, he introduced the South African frog for this purpose. He called the test the "Hogben Test," after Professor Hogben, who had shown its theoretical possibilities in this direction. Hogben's earlier work was carried out at McGill, and he had actually lived in Ste Anne de Bellevue. In spite of these coincidences, neither my husband nor I had ever seen a South African frog, and it was necessary to hunt through quite a lot of literature before we really got to know them.

Xenopus laevis is a peculiar creature.

Although it is an amphibian and looks like a frog, it is not a true frog and is only distantly related to the frogs as we know them in North America. It has a broad, triangular head, very weak forelegs, and powerful hind swimming ones, with clawed feet. Its color is a dark brown with blackish markings, but it has the chameleonlike ability to change color—from light mustard in clean, warm water or if it is in a tank with a clear glass bottom, to almost black if in a tank with a dark bottom. It has no tongue and no teeth, but only ridges in its upper jaw. It is aquatic, but must breathe air. It is carnivorous and feeds by pushing small aquatic animals, including its own tadpoles and spawn, into its mouth with its front legs; it uses very little discrimination in feeding and will sometimes choke to death because of the large pieces of meat it tries to swallow. It is a very quick and strong swimmer, but prefers to rest in shallow water, with its hind legs on the ground, and its nose just above water level. It will stay thus for hours if left undisturbed, but will disappear quickly under the water if there is a sudden movement near by. Its favorite habitat, accordingly, is water about three to four inches deep. In nature it lays eggs that are fertilized, after leaving the body of the female, by the male, who clasps the female around the waist while so doing. Normally, it will not lay eggs in captivity, although in its natural habitat—which is Africa south of the Sahara—it pairs once a year in the spring and is very prolific with its spawn.

Specimens of this animal had been sent to the London Zoological Gardens as long ago as 1890, and, ten years later, Bles, working in the Zoological Department at the University of Glasgow in Scotland, had succeeded in breeding them. He did this by lowering, for some months, the temperature of the aquaria in which he kept them, then raising it again to about 70° F., in an effort to imitate the seasonable change from winter to spring. To simulate spring rains, some water was drawn from the tank, cooled, and sprayed back through a fine nozzle. Using this technique, he was successful in getting some of his frogs to pair and to lay fertile eggs.

The development of these eggs is as remarkable as the frogs themselves. They hatch in about thirty-six hours, and a little fishlike tadpole emerges and attaches itself by a fine filament to stones and weeds; or it may even hang from the surface of the water. It is white and about a quarter of an inch long. It remains thus for a few days, feeding meantime on yolk carried over from the egg. In three or four days it gradually darkens, becomes a typical *Xenopus* tadpole, and begins to feed on plankton. It is now almost transparent except for its entrails and the dark tail, which moves continuously as it balances itself in the water, head down, at an angle of about 45°, feeding as it does so on unicellular plants.

I was soon to become acquainted personally with these odd creatures, owing to the fact that quite by accident two male frogs had been included in the shipment from South Africa. In the next two years or so, a number of other shipments were received here, but all the frogs were females. Only the female is of value for the pregnancy test since the test results in the deposition of eggs by the *unmated* female after the injection of the pregnancy hormone.

Accordingly, the two unwanted males were quite definitely surplus, and my husband handed them over to me with the suggestion that I might see what I could do about repeating Bles' work. There proved to be no need to follow his technique, however, because Shapiro, of South Africa, in 1939 also succeeded in breeding *Xenopus* in the laboratory by injecting the same hormone into the male as was used to induce the female to lay eggs. This stimulated the male to pair with the female and to fertilize the eggs. There was no question of the simplicity of this technique as compared with that of Bles, and it was the obvious one to try first.

At the beginning of 1943, I injected the first male and female. These were placed in a small aquarium with plankton, sand, and various water weeds; the day after the male and female had paired, hundreds of eggs were laid, and the two frogs were removed from the tank. Each day I watched for any sign of life, but it was not until the fifth day, when all hope had been given up, that one tadpole was seen swimming about. This tadpole was promptly named "Moses"—and Moses it is still named.

The very fact that one had survived was enough encouragement to continue, but, with only two males, breeding was, of necessity, slow. After oviposition, both males and females must be given approximately four weeks' rest. The second pairing with the other male was more successful, inasmuch as ten tadpoles survived.

In about six weeks, Moses' front legs appeared, the transparent fan-shaped head began to disappear, and the typical *Xenopus* head was forming. At this time, the first male was injected again, together with another female. From that pairing we had about a hundred live tadpoles.

Moses had now developed into a frog, except for a short tadpole tail. He was removed to a tank with about one inch of water and, as he was now carnivorous, was fed on small pieces of minced beef heart, held in front of him on the end of an applicator. The other tadpoles were also growing at a very encouraging rate.

With only the two males, it seemed an endless time before they could be injected again. I gave up the water weeds and put large stones in the aquarium. This worked very well, and we were rewarded with hundreds of tadpoles adhering to the stones and hanging from the surface of the water after our next pair was injected. Moses now had several companions in his tank, but he remained the largest, owing, no doubt, to the fact that he had received individual attention when young.

After dividing the enormous number of tadpoles we now had into batches of 100 to an aquarium, the two males were given a long rest because of lack of space for further tadpoles.

Moses by this time had developed into a female!

When Moses reached the adult stage, she was injected along with a male a few weeks younger than she. It was with some trepidation that we watched to see whether the second generation would react to the hormone. All doubt was dispelled when we found the stones at the bottom of the aquarium covered with eggs the next day. Two weeks later, from this tank 2,633 tadpoles were separated into various tanks.

One presumes that in nature the sexes of *Xenopus* are fairly evenly divided, but I have had many more females than males. Perhaps this is due to the fact that the female is always larger than the male when adult, and often at the metamorphosing stage, when the death rate is very high, the tiny frogs—about as large as a house fly, before the tail has entirely gone—will be found dead in the tank; these may be males.

It is a comparatively simple matter to induce the frogs to pair and have fertile ova, but it is quite a trick to get them past the critical metamorphosing stage. However, although even with the greatest care one loses thousands, hundreds live, and once they have developed into frogs, the death rate becomes very, very low.

Landgrebe, of Aberdeen, Scotland, reported having kept one of these frogs for fourteen years. As it takes them about two years to reach the full adult stage, it is encouraging to know that they may live so long. They can be used for the pregnancy test a little younger than two years; moreover, with suitable treatment and feeding they can be used repeatedly. The feeding and maintenance of the adults themselves is simple and inexpensive.

The only drawback to the utilization of this useful laboratory animal was the problem of having to import them from South Africa. The time has passed when this was necessary, and I have now reached the stage where space is the chief factor in continual breeding—even 2,000 frogs take up a fair amount of room!

CONTACT LENSES

IRA LUNAN FERGUSON

Professor Ferguson took his M.S. in preventive medicine and public health at Minnesota in 1941. During the war, he was in Washington with the Office of the Surgeon General as associate consultant in the Production Control Division, and later as statistician in the Medical Statistics Division. He is now associate professor of health education at Southern University.

FOR years, scientists have sought to produce eyeglasses that would be as inconspicuous as possible. Today, defective vision can be corrected with lenses that are practically invisible. These modern miracles have no frames, nose bridge, nor earpieces to annoy. It is estimated that there are now approximately 35,000 pairs of contact lenses being worn in the United States, and their popularity is steadily increasing.

Contact lenses are thin, oval-shaped lenses about the size of a 5-cent piece. Formerly made of glass, they are now made of smooth, nonirritating plastic which may be ground to any power for the correction of eye defects. They are worn under the lids, immediately over the eyeball. The principal purpose of the contact lens is to support, in apposition with the cornea, a fluid. The cornea, that clear, transparent, glasslike but hard protective surface in front of the lens of the eye, has a greater curvature than the rest of the eyeball. Within the spatial limits of the anterior surface of the cornea and the ocular surface of the contact lens, a fluid lens is formed. As this fluid is assumed to possess an index of refraction identical to that of the cornea, refraction by the anterior corneal surface is produced, its dioptric value being dependent upon its index of refraction and the radius of curvature given to the posterior optical surface of the contact lens. Inasmuch as the ocular surfaces of the contact lenses are spherical at all times, the newly created substitute cornea will introduce to the eye pencils of light rays which will find convergence at only one point.

There are many indications for the prescription and use of contact lenses—conditions in which experience has shown that contact lenses have a distinct advantage over spectacle lenses. Among these conditions are:

1. Lenticular astigmatism.
2. Conical cornea.
3. Ametropia (myopia, hypermetropia, etc.).
4. Anisometropia, or inequality in the refractive power of the two eyes.
5. Entropion—inward curling of lower eyelids with lashes.
6. Trichiasis, in which there is inversion of the eyelids in such a manner that they rub against the cornea, causing continual irritation of the eyeball.
7. Lagophthalmus—incomplete closure of palpebral fissure when the lids are shut, resulting in exposure of, and injury to, the bulbar conjunctiva and the cornea.
8. Irreparable weakness of sight, for which an enlarging system can be set up by combining a simple convex eyeglass with a strong concave shell acting like the mobile ocular of a telescope.
9. Replacement of forward spectacles on account of the wearer's occupation.
10. "Ophthalmic neurosis," sometimes called "spectacle complex."

Entertainers, professional models, and others whose livelihood depends a great deal upon their appearance prefer to wear contact lenses because they are invisible. Some people, notably many young women, experience feelings of inferiority when they find it necessary to wear glasses. The psychological "lift" that such people derive from wearing contacts is just as important, according to many leading eye specialists, as the actual improvement in vision. This is particularly true of extreme myopes, who can see themselves only dimly without contact lenses. When they look through their powerful concave spectacles, their eyes appear tiny, almost piggish, and they often become quite sensitive about this fancied peculiarity. Only with contact lenses can they see their eyes as they really are. Parents and teachers tell of the almost miraculous change contact lenses have brought to teen-age girls who had become shy and awkward because of their weak eyes and the necessity for wearing spectacles, which they considered detracted from their otherwise good looks.

THE history and development of contact lenses are as interesting as the techniques now being practiced in the making and fitting of them. Looking down the years, we can see and appreciate the many trials and discomforts, failures and errors, which followed one upon the other before today's techniques were perfected. Experimentation has finally produced a contact lens of finer optics, finer haptics (the science of adjustment to the sense of touch), and greater tolerance than the early investigators dreamed possible.

The contribution of Sir John F. Herschel, an English mathematician, astronomer, and physicist, introduces the story of the contact lens. In 1827, he first suggested the use of a "protective shell" to cover the cornea, protecting it from a diseased eyelid. Herschel devised a small saucer-shaped lens, filled it with a transparent gelatinous substance, and placed it in contact with the cornea to correct regular and irregular astigmatism, conditions which at that time were receiving considerable attention. He had experimented with the taking of gelatinous casts of the eye, but experienced great difficulty because of the fact that local anesthesia was unknown in his day. It was not until 1884 that cocaine was demonstrated to be of practical value in ophthalmic surgery.

The next investigator of the problem was F. A. Müller, of Weisbaden, Germany. He was a well-known eye glass blower and was commissioned in 1829 by a Dr. Saemich to make a thin glass shell similar to an artificial eye, to be used in the right eye of a patient. The physician stipulated that the shell of glass should have a transparent corneal portion in place of the usual iris and pupil, and he used it to prevent ulceration. Müller filled the physician's order, accurately following the specifications prescribed. So satisfactorily did the lens serve its purpose and so expertly fitted was it that the patient wore the shell until his death twenty years later. Müller made several other protective shells for patients. These shells were comparable to those he had been making for atrophied stumps, being for the most part thin, whitish glass shells with fine blood vessels sketched on the scleral portions. Instead of a painted iris and a black spot for the pupil in the center, however, he blew a hemisphere of glass, thin and transparent, through which the patient looked.

Next on the scene we find a noted physician of Zurich, Switzerland, Dr. Eugen Fick, who in 1887 published the results of experiments in which he used contact lenses as refractive devices instead of as mere protective shell coverings. He was the first to use the term "contact lens" (*Kontact Brille*). Dr. Fick experimented with the casting of the eyes of laboratory rabbits with plaster of Paris, had glass lenses blown over these molds, and applied them to the eyes of the rabbits, using various solutions for the liquid lens, finally deciding on a 2 percent dextrose solution as affording the best refractive medium. He further experimented with the eyes of cadavers. From these he had contact lenses blown for his own eyes and reports wearing them without too much discomfort for 2 hours at a time. Satisfied with this, he went ahead making contact lenses for his patients, later experimenting with contact lenses ground to correct visual difficulties, especially cases of keratoconus, a condition in which there is conical protrusion of the center of the cornea without inflammation. To Fick belongs the credit for formulating the idea that the fundamental principle underlying the use of contact lenses was the neutralization of all the irregular astigmatism produced by the conus with a liquid lens of approximately the same refractive index as the cornea, which lens is formed between the anterior surface of the cornea and the posterior, or ocular, surface of the contact lens. In this manner the refractive surface is moved from the highly irregular surface of the cornea to the inner surface of the contact lens with its spherical surface. Fick asked Professor Ernst Abbe, associate of Carl Zeiss, to grind for him a contact lens having a corneal curvature of 8-mm. radius and a small scleral band of 12-mm. radius. These pioneer Zeiss-made lenses proved unsatisfactory, but Fick, unperturbed, continued with his experiments, records of which reveal attempts to solve some of the clinical problems that even today confront the ophthalmologist in the satisfactory fitting of contact lenses—problems which include such factors as clouding, conjunctival irritations, and tolerance.

August Müller, a medical student at Kiel, Germany, submitted as his dissertation for his Doctor's degree a study of the effects of spectacles on vision, and in that paper he touched upon the subject of contact lenses, with which he had done some work. Independent of Fick and F. A. Müller (to whom he was not related), August Müller investigated the contact lens problem untiringly. He had a Berlin optician grind for him the first contact lens to specification, and was able to correct his own myopia of minus 14 diopters within 0.5 D., although he admitted that the lens was uncomfortable. He attributed the discomfort to poor scleral fit. In 1889, he experimented with the taking of casts from the living human eye, but was not very successful. He did, however, successfully take casts from artificial eyes as a basis for his lenses. These lenses were ground to specifications with varying radii, diameters, and thicknesses, but even with these he was unable to obtain a tolerance of more than 2 hours. In 1889, ophthalmologists had not yet accepted the idea of contact lenses, according to the scientist Joseph Dallos. Many specialists argued that an organ as sensitive as the eye could not possibly be expected to tolerate comfortably the presence of a large piece of glass, when even a tiny dust particle in the eye caused so much pain. They feared that use of contact lenses would cause trauma and subsequent infection of the eye, and were unwilling to take such risks. They were quite content to continue prescribing according to diopters the type of eyeglasses required, instead of experimenting with glass shells of imperfect dioptric effect, which could neither be numbered nor classified.

Another physician, D. E. Sulzer, of Geneva, Switzerland, introduced a new type of contact lens having a narrow scleral portion designed to rest at the outer edge of the cornea where pressure on the limbus often caused pain and sometimes tearing. The corneal portion of his lens was also small. During the period of Sulzer's investigations, in the 1880s and early 1890s, improper design and uncomfortable fit made all contact lenses unbearable after any length of time, and after 1892 the entire idea of contact lenses was abandoned for a number of years. However, the earlier "protective-shell" idea of Sir John Herschel had progressed to an actual contact lens that would correct visual error as long as it could be tolerated. The valuable work of the pioneers demonstrated that the optical principle involved was sound, even though faulty design and fit of the contact lenses at that time prevented their being worn longer than a few hours at a time.

The beginning of the second decade of the twentieth century saw a revival of interest. There were renewed efforts to make contact lenses practical, and many new names began to appear among the investigators of the problem. Until 1932 only the "blown" contact lens by F. A. Müller and the "ground" contact lens first made by Carl Zeiss were described in the literature. Up to that time the lenses used and experimented with were stock lenses, which did not fit—were not made to fit—the individual patient. There was still the problem of finding a material with which to make casts of the human eye. This problem was solved by Dr. Joseph Dallos, of Budapest, Hungary, now working at the Contact Lens Centre, London, England. He has been called the "father of the modern contact lens" because in 1932 he made perfect negative casts of the living human eye with a material known as Negocoll. From the negative mold a positive cast was prepared from a paraffinlike substance called Hominit. Later, as is now done, the positive cast was prepared by pouring dental stone into the negative mold, and these positive casts were then used as a base for the making of the lenses. The latest advance in the making of impressions of the living human eye has been the introduction of alginate compounds, such as are used by dentists for the making of negative molds. The material has the advantage over Negocoll in ease of preparation, quick setting, and easy removal. For some time wax-casting was tried; a shell of wax was placed between the lids of the eye, and body heat was employed to make it conform to the shape of the patient's eye. After application of ice water, it was carefully removed, and molds were made. This procedure was abandoned on account of its persistent inaccuracy.

In 1939, Dr. Arno E. Town, of New York, whose work is associated with the individual wax-molding method, presented a paper as a graduate lecturer at the 45th Annual Convention of the American Academy of Ophthalmology on "Impression Technic for Contact Lenses." In his report he observed that the newer method of fitting contact lenses by a mold had superseded the previous trial-and-error method, and that stock lenses would fit in about 25 percent of the cases. He brought out the fact that there were so many variations in the shape and size of the sclera and the cornea that it was impossible to attempt a classification of all eyes into a few size groupings.

> The more impressions made of eyes for contact lenses, the more convinced I am that in order to have a good molded glass, it is necessary to have as good an impression as it is possible to make. The better the impression, the better the glass will fit. Furthermore if a technic is used which will help changes to be made in the glasses, work with contact lenses will be more successful [he explained].

The introduction of the all-plastic lens was yet to come in answer to Dr. Town's expressed hope.

In 1934, Gauldi, of Italy, announced an original design for a contact lens with a 20-mm. diameter and with only one continuous posterior curve, eliminating all the various combinations of curvatures for sclera and cornea. His lenses were useful for refraction purposes. In this same year, the Müller-Welt firm, of Germany, which had previously made a lens similar to the Zeiss lens in principle, now made another type of lens, still a blown lens, but blown over toric castings of various curvatures, and fitted by the trial-and-error method.

Until the outbreak of World War II, the Zeiss Company still manufactured their molded lenses. These lenses were made from individual castings and were of glass. In fact, all contact lenses were made of glass up to 1937. In the United States, C. L. Stevens and Dr. W. Feinbloom, of New York, were both making outstanding contributions to the perfection of the contact lens technique. Stevens was successfully using and working with Negocoll, and Feinbloom worked with wax molds of the eye as well as with molds from Negocoll. At this time, Leopold Dreifuss, of San Francisco, was experimenting with molds of an alginate compound he called Modeloid for taking impressions of the anterior segment of the eye. This material set in 3–4 minutes, as against 5–10 minutes required by Negocoll.

The part-plastic and the all-plastic contact lenses made their debut in 1937. In that year, Ernest Mullen, of Boston, made the first all-plastic contacts, and Theodore Obrig, of New York, at first associated with Mullen, began his work with all-plastic lenses, laying the groundwork for his now famous Obrig Laboratories in New York, which specialize in the manufacture of contact lenses, instruments, and supplies for the trade. Feinbloom also became a prominent figure in contact lens work at this time, and during the years 1936–39 he published his method of making glass lenses from individual molds. In 1939, he announced a variation of the same general type of lens, in which the need for casting was eliminated. Both lenses were constructed of two separate parts, a ground-glass corneal portion and a molded plastic scleral band, fitted to each other in much the same manner that a watch crystal is fitted to its supporting bezel.

Albert L. Anderson, associate owner of Precision Contacts Company, Minneapolis, and head of the Scientific Department, N. P. Benson Optical Company; Dr. Edwin Adams, of Detroit; Dr. L. L. Beacher, of New York; and Dr. Eugene Freeman, of Chicago—all are familiar names in recent contact lens experimentation and development.

In fitting the patient with contact lenses, the eyes are first examined by the ophthalmologist to ascertain whether there are any pathological conditions that would contraindicate the use of contact lenses. The patient's vision is tested and refracted while a pair of trial lenses is being worn, in order to determine the extent of improvement over spectacles that the contacts will provide. If there is little or no improvement, contact lenses will hardly be worth the much greater expense involved, although cosmetic factors may warrant the change if such considerations enter the picture. If patient and ophthalmologist decide on contacts, the technical consultant from the optical company is called in for the taking of the impressions. The patient is placed in a reclining position on the usual operating table or adjustable chair. The ophthalmologist anesthetizes the eyes with 0.5 percent pontocaine or 2 percent butyn, 3 or 4 instillations of 2 or 3 drops each being placed in each eye. There is no pain or danger, and in 5 minutes anesthesia is complete. The molding material, Negocoll, Modeloid, or the more recently developed Moldite, is then mixed by the technician and is placed in the eye, directly over the eyeball. Two molds are usually taken by the more experienced consultants, so as to obviate the necessity of having the patient sit through the ordeal again should one mold turn out to be imperfect. When the semisolid material hardens in 3–5 minutes, depending on the type used, it is carefully removed. From these molds the lenses are made. Then begins the long and tedious process of fitting. The patient has to sit to a series of delicate fittings at the optician's, the lenses being inserted in the eyes, removed, inserted again, and removed time after time by the technician for alterations, trimming, beveling of edges, thinning, further polishing, and other adjustments, all of which is done to ensure perfect fit and comfort. When the technician considers the lenses a perfect fit, he returns them to the ophthalmologist, who, inserting them in the patient's eyes, tests again thoroughly to make sure that the patient is receiving maximum vision from the lenses, adding whatever correction may be necessary before sending them back to the optical technician for final grinding and finishing.

When the new contacts are at last handed to the patient, the optical technician instructs the patient in the care of his contact lenses, how to insert and remove them, the type of buffer solution to use, and so on. Because of the amount of work involved in prescribing and fitting, contact lenses are expensive, costing around $200 a pair, and the patient is strongly urged to insure his lenses against damage or loss.

REGULAR forward glasses limit the field of correctible vision. When the eyes are rotated, only a small area near the optical center is free of aberrations, and vision is distorted in the peripheral area. Contact lenses, on the contrary, move with the eye. That being the case, the optical center is always before the pupil, and as a consequence the number of aberrations is reduced to a minimum. Contact lenses also offer a practical method of obtaining binocular vision in monocular aphaxia, absence of the crystalline lens of the eye. In general, high myopes obtain much better vision with contact lenses than with regular glasses. The eyes do not appear larger or smaller when contacts are worn, nor are the lenses rendered useless by snow, sleet, hail, rain, or steam, as forward spectacles usually are. Seamen have only to blink their eyes to clear their lenses of spray. People who wear regular glasses are familiar with the fogging caused by steam or during cold weather and can appreciate this particular advantage. Swimmers like them because they protect the eyes when swimming or diving. Actors, actresses, clergymen, public officials, and others use them to hide their defective eyesight and at the same time secure improved vision. Hockey players, ski jumpers, high divers, fancy skaters, hunters, sportsmen, and tennis, baseball, basketball, and football players report that they find them more practical than spectacles because of the increased field of vision, lack of hindrance, and the fact that risk of breakage is practically nonexistent. Mining engineers, whose ordinary glasses become misty from moisture and dust underground, prefer contacts. Industrial workers report that contact lenses afford much more protection against flying bits of steel, glass, and other particles. Flyers who wear contact lenses report that they can now fly open-cockpit trainer planes, or even power-dive from 30,000 feet, without using goggles. Some aviators say they can see far better with contacts at high altitudes than with regular glasses. Along with those engaged in dangerous sports listed above will be found boxers, some of whom actually wear contact lenses in the ring with perfect safety, while seeing their opponents more clearly.

Contact lenses have been found particularly effective as light filters for use on beaches and snowfields, enabling many individuals whose eyes are hypersensitive to light and glare to tolerate such conditions much more comfortably than they could with glasses. Tinted contact lenses are used by lifeguards, skiers, and other persons disturbed by excessive light. Within the past year, Obrig Laboratories announced that a drop or two of a harmless green or yellow dye in the solution filters out painful rays of light without changing the normal appearance of the eyes, thus protecting the eyes against glare and making it unnecessary to wear sunglasses for that purpose.

There are at present three or four main types of solutions used with contact lenses. Some of these are made up to various pH's, and can be made up with different osmotic pressures. Normally, whatever the type of solution used, the osmotic pressure is invariably made equivalent to that of 1.5 percent sodium chloride solution. The solution of choice is the 1.5 percent sodium bicarbonate, which may be used in 1 percent or 2 percent strengths with practically the same good results as the 1.5 percent solution recommended by ophthalmologists and optical consultants. Then there is the sodium bicarbonate-sodium chloride solution made up as follows: sodium chloride, 1.68 gm.; sodium bicarbonate, 0.30 gm.; distilled water, 120 ml. The modified Gifford and Smith buffer solutions are made up according to different formulas, but are all adjusted to give an osmotic pressure equal to that of 1.5 percent NaCl. Table 1 gives the amounts of chemicals to be used with each 120 ml. of water.

TABLE 1

CHEMICALS TO BE USED WITH 120 ML. OF WATER FOR VARIOUS pH'S

pH	BORIC ACID (GMS.)	Na_2CO_3 (GMS.)	KCl (GMS.)
8.0	1.640	0.452	1.056
8.2	1.497	0.543	1.056
8.4	1.332	0.626	1.056
8.6	1.175	0.703	1.056
8.8	1.023	0.777	1.056
9.0	0.874	0.845	1.056

The Feldman buffer solution is another type used with very good results. It is modified to make it isotonic with 1.5 percent NaCl. Table 2 shows the ingredients used.

TABLE 2

FELDMAN BUFFER SOLUTION (120 ML. WATER)

pH	BORIC ACID (GMS.)	BORAX (GMS.)	NaCl (GMS.)
8.0	0.98	1.04	1.01
8.2	0.89	1.21	1.01
8.4	0.80	1.40	1.01
8.6	0.61	1.79	1.01
8.8	0.28	2.47	1.01

It is recommended that the solutions be prepared by pharmacists to ensure accuracy, and that distilled water be used always. Dr. R. R. Tracht, an outstanding ophthalmologist of St. Paul, who specializes in contact lens work, reports that many of his patients who make up their own sodium bicarbonate solution use tap water with no ill effects. However, it is wiser to use distilled water with such precious and delicate organs as one's eyes. Optical technicians further recommend that the solution be prepared fresh every 3 or 4 weeks, in small amounts, say 4 ounces at a time—possibly more often than every 3 or 4 weeks in warm weather and less often in cold weather.

The average length of time that contact lenses can be worn without removal is 4–8 hours. Obrig reports the extreme case of a soldier who wore his contacts for 2 weeks without removing them. This is an individual problem, however, and only experience can decide how long any individual can wear contact lenses without changing the solution. I find that after I have worn my contact lenses for 4 hours they begin to cloud, and the solution has to be changed. Halos are a phenomenon observed by wearers of contact lenses who do not remove their lenses for 10 or 15 minutes when they first begin to note clouding. Rings of light—wide bands of light—are seen around lights or light sources such as lamps, candles, or even stars and the moon. Some experts explain this halo as being due to the fact that the eye literally "exhales" carbon dioxide—that this CO_2 in the buffer solution will reduce the pH of the solution—and that fact may account for the halos. Immediate removal of the lenses when the halos are noted is of course indicated.

At the present time bifocals are not ground in contact lenses. If necessary, an auxiliary pair of reading glasses may be used along with the contacts. Optical consultants suggest that contact lenses never be wiped with cloth, but that soft tissue of the Kleenex type be used to dry them. Care should also be observed in inserting the lenses to see that the proper lens for the right eye and left eye, respectively, is inserted. Each lens is marked in a corner with one or two small indelible dots to indicate right or left.

Plexiglas, the material from which the nose of army bombers is made, is the type of plastic used in making contact lenses. Plastic lenses last indefinitely—they are unbreakable—and they are 60 percent lighter in weight than glass. The old glass contact lenses were much heavier and in use quite often caused a gravity drop great enough to produce contact with the cornea at the upper limbus. Plastic lenses will scratch more easily than glass, but optically these scratches have no disadvantageous effects, because scratches on the inside surface are filled in by the solution, and scratches on the outside are filled in by the tears. Durability, adjustability, transparency, chemical inertness, and lightness now characterize the contact lenses made of plastic. A well-defined and systematic method of making the adjustments necessary in fitting contact lenses has been gradually developed, and any ophthalmologist, optometrist, or optician with no more than average mechanical ability can be trained to do the work. The State Board of Education in New York, otherwise known as the University of the State of New York, now has under its jurisdiction such a school for giving training in the exacting techniques of contact lens work.

In the foregoing pages I have given the reader a description of contact lenses, and the many advantages claimed for them by wearers have been enumerated. It is not the intention of this article, however, to "sell" contact lenses to readers, and it is therefore in order that some of the discomforts, drawbacks, and inconveniences attending their use be pointed out here.

First of all, contact lenses are rather small, and because of that fact they are easily lost or mislaid. Again, there are some individuals who simply cannot tolerate contacts, and such people should not insist on them if a reputable ophthalmologist advises against their use. In fact, under no circumstances should anyone allow himself to be talked into using contact lenses by optometrists or opticians, who, although quite well trained in their specific skills of grinding and fitting glasses to prescription, are neither trained nor qualified to diagnose eye defects or prescribe glasses, regular or contacts. In this matter, the patient should be guided entirely by the advice of his ophthalmologist. The ophthalmologist is a physician—a doctor of medicine who has specialized in diseases of the eye—and is the specialist best qualified to prescribe glasses of any type.

More experienced technicians teach patients how to remove their contacts with the thumbnail as well as with the vacuum cup. Students and younger people generally find it too much trouble to carry the vacuum cup around, and use the fingernail method of removal almost exclusively. The procedure is simple enough. For removing the right lens, the right forearm is raised high, as in the salute position, at right angles to the side of the head, and in a straight line with the eyebrow. With the eyes looking far over to the left the patient places the back of the right thumbnail on the contact lens, draws it across laterally until the edge of the lens is felt, then puts the thumbnail under the edge near the top of the lens and plucks it forward and out. The left thumbnail is similarly used for removing the left lens. A few minutes of practice soon develops deftness in this method of removal.

The use of contact lenses is, and for some time to come will be, limited, owing to their high cost, the fact that they can be worn for only short periods at a time, and the bother of having to use solutions with them, without which they are utterly useless. Experimentation continues, however, and it is quite possible that a contact lens may yet be developed which will be much easier to insert and remove, will cost much less, and with which probably no solution will be necessary.

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TWISTED TREES

To soilless summits high—
Abandoned by all living things save symbiosts—
The orphaned seeds are borne,
To be rejected by the rocks,
Save some
Who find their succour in the crevices
And sprout.
The cycling seasons claw;
And few remain.
But those whose roots, first tenuous,
Develop varicosities, dig deep
And etch their twisted starkness on the sky.
Before the wind,
With heavy arms and calloused fists,
They stand to battle.

JOSEPH HIRSH

ADVICE TO NEWCOMERS

A REPORT ON RELEASING THE RESULTS OF RESEARCH TO THE PUBLIC

A. J. RIKER, M. R. IRWIN, R. J. MUCKENHIRN,
HELEN PARSONS, and M. A. SCHAARS

The authors were a committee (in the College of Agriculture of the University of Wisconsin) assigned to study the obligations involved in releasing the results of research to the public and to indicate the best way to do it. The faculty adopted their report unanimously. Although it was prepared for a single institution, the report appears here because it contains much that may interest others.

THROUGH emphasis on learning and service, a cooperative and highly beneficial understanding has developed over the years between the state of Wisconsin, the College of Agriculture (including the Department of Home Economics), and the people of the state. Because of the interweaving of its activity with man's economic and personal welfare, this College of the University of Wisconsin has established many happy relationships and has made every Wisconsin citizen, whether farmer, public official, or industrial magnate, feel welcome while seeking help with his specific problem. When a citizen wishes to improve his own situation or that of his community, he can secure advice from the College. If inadequate information is available, a problem of critical importance may be investigated by the faculty.

Thus the College is the chief of the state's agencies to encourage, to assist, and frequently to lead ambitious and forward-looking agricultural people to improve their general welfare. In a very real sense, the limits of the College campus are the boundaries of the state.

The freedom and opportunity provided for the faculty have given it large obligations and enormous responsibilities. The splendid traditions of service to the public and of cooperation with one another are among its most cherished possessions. In the highest sense of the word, the faculty members are servants of the state. Among them, highest honors go to those who have served the public well. To be worthy of their trust is no small concern of these men and women.

However cloistered some theoretical and technical phases of agriculture may be, they belong in the science of life and they affect the food, shelter, and general welfare of every Wisconsin citizen.

After the answers to critical problems have been painstakingly worked out, the results verified, and the implications fully understood, there is an imperative obligation to make the new information available to the public promptly and to indicate what good might be achieved as a result of the work. Such releases that are premature, however, or that are made inappropriately, can be detrimental to the investigator, to the institution, and to the best interests of the state.

Technical matters are usually announced through lectures before scientific societies, in research journals, or in Experiment Station publications. The more popular accounts are made available by means of the press and radio and in meetings and classrooms.

Primarily for the benefit of new members added to the college faculty, some of the traditions and obligations of the staff regarding both the conduct of research and the announcement of the results are indicated below.

Safeguarding academic freedom. In accord with the regent's report in 1894, it is essential to keep the College of Agriculture, as part of the University of Wisconsin, a place where the mind is free to serve the best interests of mankind, where it bows only to the commands of an exacting conscience, and where there is the "continual and fearless sifting and winnowing by which alone the truth can be found."

Among the basic principles of academic

freedom are the following (copied, in part, from the statement in *Amer. Ass. Univ. Prof. Bul.*, 1944, 30, 104–105):

> *a*) The investigator is entitled to full freedom in research and in the publication of the results, subject to the adequate performance of his assigned duties.
>
> *b*) The speaker or writer is entitled to freedom in the classroom or before the public in discussing his subject, but when serving in a technical capacity he should be careful not to introduce controversial matter which has no relation to his professional field.
>
> *c*) The university man is a citizen, a member of a learned profession, and an officer of an educational institution. It is not to be believed, or even assumed, that a scientist has any special capabilities in questions well removed from his special subjects. When he speaks or writes as a citizen, he should be free from institutional censorship or discipline, but his special position in the community imposes special obligations. As a man of learning and an educational officer, he should remember that the public may judge his profession and his institution by his utterances. Hence, he should at all times be accurate, should exercise appropriate restraint, should show respect for the opinions of others, and should make every effort to indicate that he is not an institutional spokesman.

Since being worthy of academic freedom is the best means of maintaining it, some of the obligations of the staff are indicated.

Responsibilities of investigators. Mature scholars working together for the development and utilization of knowledge form the foundations of our civilization. Few things can be more important than to enhance and maintain the dignity of the investigator's profession so as to attract into its ranks and to hold men of highest ability, of sound learning, and of strong independent character. Such men have special obligations to one another, to their institution, and to the public.

Important among the responsibilities of those doing research are the following:

a) The highest standards of investigation, interpretation, and release of information to the public are obviously necessary. The policies, procedures, and achievements in each program may be evaluated best by giving first importance to their effectiveness in promoting the research project in hand, its publication, and the public welfare. Purely personal aspects or the roles of individual workers are secondary.

b) Because of the many factors involved in any important program, every effort should be made to emphasize the subject itself rather than the activities of particular administrative units. An environment is to be encouraged in which there is the greatest possible freedom for cooperation between individuals irrespective of budgetary divisions involved.

c) Decisions regarding important technical matters are wisely made only after all those interested have had the opportunity to express their views. The decisions should be unanimous before action is taken, before recommendations for practical applications are given, and before the results are published. It is particularly unfortunate when two scientists give the public conflicting advice. It is to be expected that there will be differences of opinion. Controversy in science is healthful, but its place is on the campus and in scientific journals, rather than in the popular press or in the talks before general audiences. If differences of opinion are so strong that individuals are unable to agree, such differences can be settled (1) by ending the undertaking, (2) by the withdrawal of one faction, or (3) by calling on the services of an umpire selected by mutual consent.

d) Any publication of important new research sponsored by the Experiment Station must have the approval of the director.

e) The results of research should be released only after an exacting scrutiny has shown that the experiments are valid and that the conclusions drawn are the only ones justified. Ordinarily, the work should be criticized by two qualified colleagues who did not participate in the study.

f) The release of technical or institutional information without the knowledge and approval of the responsible investigators and administrators is a breach of confidence and contrary to the best interests of the staff and the public. The privacy of conferences and of the papers sent through the campus mail deserves respect.

g) If someone on the staff should gain important information in any manner and should release it prematurely with resulting embarrassment to the faculty, the institution, and public relations, the situation calls for investigation by a special fact-finding and duly constituted committee of the faculty.

h) When discussions are called for about controversial questions, a worthy man presents impartially the evidence on both sides.

i) Discussions regarding the results of one's own research and those of other scientists are in bad taste when personalized and in good form when objective.

j) Disparaging remarks, especially before students or the public, commonly boomerang to the disadvantage of the speaker.

k) When valuable results have been satisfactorily proved and are ready, they should be made available to the public as promptly as feasible. It is recognized that when the work is reported, either through the printed page or from the platform, it becomes public property.

Recommended procedure. For all ordinary situations with new material, the following course of action seems desirable:

a) The first public statement of a major research finding should be through an approved manuscript or through a communication before a scientific society. Almost simultaneous release can be arranged for press and radio.

b) When popular releases are provided, they can be made, for example, through the staff of the Department of Agricultural Journalism, so that all the news agencies of the state will receive the story at the same time. The prepared account should be carefully checked for accuracy by the investigator.

c) Newspaper reporters and correspondents for magazines often are anxious to secure prior announcement of important findings for the exclusive use of their own publications. Such "scoops" can be disastrous from the public-relations standpoint. Every editor who sees a rival thus obtain important information has a right to complain. The College can lose more good will through a scoop than it can gain in credit from one publication. It cannot afford to play favorites among the gentlemen of the press.

The same principle applies when new animals and plants or when new technical procedures are developed. They are made available in as impartial and equitable a manner as possible so as best to protect and serve the public interest.

d) Occasionally, an avid reporter does obtain more or less distorted information not yet authorized for release. If this should happen, it is wise to consider with the administration and the Agricultural Journalism Department the desirability of heading off the impending scoop by an accurate and prompt release to all papers.

e) However, when correspondents seek additional information about material already announced, it is desirable to accommodate them so far as seems feasible. In such cases, there is no reason to consult the administration or the Agricultural Journalism Department. Likewise, no approval is necessary when members of the research staff write clarifying articles for popular publications or speak over the radio. They wisely utilize such opportunities to round out the public's information, so long as it does not involve exclusive release of original research.

f) Scientists giving important speeches before popular or technical societies or publishing valuable technical papers should take the initiative in providing for and correcting popular releases with the press service available on the campus.

g) None of the cautions mentioned above should put off or impede the prompt and adequate release of significant information about progress in research. More investigators commit sins of omission than of commission. Those in publicly supported institutions have a special responsibility against unnecessary delay.

In conclusion, while releasing the results of research, the best way for research men to safeguard their academic freedom is to be worthy of it. From the nature of their positions they have a large and exacting responsibility to their colleagues, to their institutions, and to the public welfare.

TECHNOLOGY AND COMMUNITY: THE MANDATES OF SURVIVAL

MELVIN RADER

Dr. Rader (Ph.D., Washington) began his teaching career in the English Department of Western Reserve University. Within a year he had decided to transfer to philosophy; he has been teaching it since 1930, currently at the University of Washington. Dr. Rader is the author of A Modern Book of Esthetics.

AT THE dawn of modern science, men were immensely confident that its uses would be beneficent. The great medieval seer, Roger Bacon, was fired by a deep enthusiasm for the new world that science would create. It would reveal the past, present, and future, and secure the vast improvement and the indefinite prolongation of life! Similarly, such Renaissance thinkers as Giordano Bruno, Leonardo da Vinci, and Tomasso Campanella—harbingers of the modern scientific and technological revolution—were intoxicated with its infinite promise. Such optimism found ample expression in the work of Francis Bacon, who believed that science would enlarge "the bounds of the human empire to the effecting of all things possible," and, in writing the *New Atlantis,* jubilantly imagined the Utopia he believed scientific progress would achieve. Another great prophet of science, René Descartes, looked forward to the time when we human beings would understand "the force and the action of fire, water, air, the stars, heavens, and all other bodies that environ us, . . . and thus render ourselves the masters and possessors of nature." The same confident note is struck again and again in the writings of the great Renaissance scientists such as Kepler and Galileo.

It is sad to turn from such inspired optimism to the views of some more recent thinkers. Ironically, it has been within the past hundred years, when science has been achieving its greatest triumphs, that the most pessimistic views have been expressed. For example, Marx maintained that science and technology, in the absence of a sound economic structure, only intensify the insecurity and misery of life. "Even the pure light of science," he said, "seems unable to shine but on the dark background of ignorance. All our inventions and progress seem to result in endowing material forces with intellectual life, and in stultifying human life into a material force." The more pessimistic American thinker, Henry Adams, wrote at the beginning of this century:

> I apprehend for the next hundred years an ultimate, colossal, cosmic collapse; but not on any of our old lines. My belief is that science is to wreck us, and that we are like monkeys monkeying with a loaded shell; we don't in the least know or care where our practically infinite energies come from or will bring us.

Likewise, William James, although not given to dour predictions, warned that natural science might culminate in the total destruction of civilization, and that modern scientific man may be like the child drowning in a bathtub because he has turned on the water without knowing how to turn it off. Since the development of atomic fission, not only philosophers and social theorists but even the most eminent scientists have become alarmed. Professor Harold C. Urey, a Nobel prize winner in physics, has declared: "I'm a frightened man. . . . All the scientists I know are frightened—frightened for their lives—and frightened for *your* life."

What accounts for this difference between the early optimistic prophets of science and these latter-day "alarmists"? The explanation, in part, is that bitter experience has taught men a simple truth—that every science and technique can be used for either good or ill. Knowledge of drugs may be used to heal the sick or to poison one's enemies; military art may be used for legitimate defense or ruthless conquest; economics can be used to swindle or to aid; education may inculcate base ideals or true wisdom. Hence, it is very important to choose the proper goal.

As Francis Bacon remarked: "The lame man who keeps the right road outstrips the runner who takes the wrong one. Nay it is obvious that when a man runs the wrong way, the more active and swift he is the further he will go astray." A society, like a runner headed in the wrong direction, may be efficient and yet damned. Hitler provided the most vivid illustration of this contention. With the technical resources of a highly civilized ruler but with the moral impulses of a barbarian, he was able to wreak havoc beyond the wildest ambitions of an Attila. Hell may be paved with good intentions, but it is also paved with science and techniques. Was it not to *know* more that Faust answered the devil's call?

We must, therefore, have sound ideals to determine the uses to which science shall be put. During the dawn of scientific thought, when glorious new horizons were becoming visible, it was easy to overlook this fact; but we who live with the *Blitzkrieg* fresh in our memory are fully aware that science can be used either to construct or destroy.

In the present article, I shall maintain that nothing short of the ideal of community, as embodied in a cooperative social order, can guarantee the utilization of science for welfare. When I speak of "community," I am using the word in a broad sense. Some writers, such as Baker Brownell, insist that the term should be reserved for the small, intimate, face-to-face group, such as the old-fashioned "neighborhood." Other writers, such as Josiah Royce, use the term to denote any essentially cooperative group, even though world-wide in scope. I shall use the word to cover both these meanings; and, indeed, I shall argue that we need both the *intensity* of community as insisted upon by Brownell, and the *extensity* of community as envisaged by Royce.

The need to integrate science into the wider life of the community becomes obvious when we consider technology—the instruments and techniques that spring from applied science. Because of the ever-accelerating technological revolution, including the development of atomic fission, men's ability either to cooperate with or to injure one another has become exaggerated beyond all comparison with former times. As this process of change rushes toward a climax, the sole way to avoid dire catastrophe and to reap the immense potential benefits of the new technology is to organize a world community among the medley of separate sovereign states, of antagonistic races, of embattled economic classes. As the atomic scientists have told us, it must be *one world* or *none*. It must be a unified and peaceful world or it will eventually cease to be a decent world, a human world.

At the same time we build a world community, however, we must not neglect the small community, without which the great community will lack roots and reality. We cannot create a lasting world community unless we reconstitute and conserve the values of the intimate neighborhood—the love that comes only with intimacy, the appreciation of personality in its wholeness and integrity, the sense of mutuality and interdependence. Technology as we have known it in the past hundred years has been lethal to these values. It has substituted the standardized routines, impersonal relations, fragmentary contacts, and rapid shifts of an anonymous mass society. It has thus tended to undermine the person-centered culture which alone has nourished the ideal of the brotherhood of man.

In this article, I shall consider the relation of technology both to the small primary community and to the great community.

No PHASE of technology has been more disruptive of primary group life than the development of mobility and communication as a result of modern inventions. People dart about by means of trains, ships, busses, trolleys, automobiles, and airplanes in a manner that would have been incredible fifty years ago. Likewise, a startling revolution in communication has been produced by the rotary press, linotype, telegraph, telephone, phonograph, camera, radio, moving picture, and television and by the multiplication of museums, libraries, postal facilities, educational institutions, and publicity and propaganda agencies.

Mobility shifts the individual relative to his environment. Communication, in a sense, shifts the environment relative to the individual: it brings the distant in time and space into the immediate focus of attention. The effect of both mobility and communication is to weaken or dissolve the old neighborhood unity. Essential contacts are often not immediate: they are five miles away at the club, downtown at the office, a hundred miles distant in a different city. A person may be swayed far more by a writer in Paris than by the man across the street. In the metropolitan environment, where mobility and communication are most highly developed, the main direction of human attention has shifted to unstable contacts, remote influences, and dispersed currents of thought.

This effect has been reinforced by the economic fluctuations and world wars that have been characteristic of our technological age. The greater part of the world has rocked on from crisis to crisis: from war to boom and then to depression and to war again; and this fatal cycle threatens to begin anew. These inflations and depressions have hurled millions out of one stratum of society into another; they have left a great many people with a sense of undeserved frustration and a diminished faith in the old verities. As a result of war, too, the pattern of living has been rudely marred; habits, ambitions, ideologies, have been scrapped; new programs of life have been swiftly improvised. Almost everyone has experienced to a degree how violently the personality is wrenched by these social transitions. Technology cannot be held exclusively responsible for these developments, but they could occur on a huge scale only in a technological age.

Even the mere frequency of inventions has a disturbing effect upon the folkways of the old neighborhood life. Modern inventions are not only numerous but are increasing at a fast, cumulative pace. Most of these inventions are unpredictable, and a great many of their effects are unforseeable. Consequently, men cannot prepare for the unknown future, and they find that the old ideas and prophecies do not apply. They feel bewildered, as though they had awakened after a severe earthquake to find the old landmarks scarcely recognizable.

The cumulative effect of these factors—mobility, communication, economic insecurity, modern technological warfare, and frequency of invention—is a disorientation more radical than the world has known for many centuries. When the mind is subjected to rapid shifts in time, space, rank, and expectation, impressions are multiplied beyond the individual's power of synthesis. Faced by this disjunctive multiplicity of experience, many minds have floundered in their effort to achieve life organization. The technological revolution has transported them to

. . . . the waste beyond God's peace
To maddening freedom and bewildering light.

The quiet, intimate, stable pattern of the small community or family group has, in consequence, been severely shaken.

No doubt the restless and sophisticated man of today is posterior in point of evolution to the stable "Philistine" type of the old-fashioned neighborhood and primary group; he is possibly higher in scale of "intelligence" and certainly wider in range of experience. Others besides Marx have spoken of the "idiocy" and dullness of rural and village life. Most city dwellers, indeed, would be reluctant to leave their cramped quarters for the more spacious and beautiful environment of the small town or countryside. To some extent, their reluctance is the result of a kind of blindness, like that of the city dweller who takes a portable radio when he goes into the country for an "outing" lest he feel bored in contact with earth and sky without the commercial advertising and continual mechanical din of the radio programs. On the other hand, many people have good reason for their reluctance to leave the great city. It affords more opportunity for varied experience, critical choice, purposive association, and many-faceted cultural experience. The small, relatively static community, in contrast, has less stimulus and challenge, less dramatic appeal, more stodgy conservatism, more intolerance toward variations in personality.

Life in the metropolitan environment, however, is now becoming so congested and hectic, so routinized in its mechanical regimens, so casual in its human contacts, that relatively few of the inhabitants can find any deep happiness or intimate companionship. The happy life, like music, has its melody and its harmonic background, its essential motifs and subsidiary phrases, evolving in an orderly and continuous pattern. Such organic development of personality occurs only when there is a considerable degree of organicity in the stuff of experience, whereas the environment of the machine-age man is disorganic in its confusion, profuse novelty, fragmentation of experience, and mechanized routines. The high rates of crime, juvenile delinquency, insanity, and suicide in the great urban areas are one indication of the human toll that is being exacted.

Congestion in these overgrown cities is paralleled by depopulation and anemia in the countryside and the villages. Wherever modern technological civilization is highly advanced, talent, wealth, and population are being drawn off from the small towns and hinterlands to the metropolitan centers. Vast rural expanses have become, in effect, economic and cultural colonies exploited for urban advantage. The more educated and energetic of the youth float away to the city, never to return. Spengler finds in such devitalization of the countryside and smaller communities, with its attendant metropolitan concentration, one of the surest signs of the "decline" in civilization; and both Patrick Geddes and Lewis Mumford have vividly described the social decay that sets in when cities reach the stage of extreme congestion and giantism while the surrounding hills and valleys and towns are drained of their human wealth.

It is doubtful if the ideal of the brotherhood of man, which even now is honored more in the breach than in the observance, can be long sustained in this metropolitan environment. Modern technology and its giant spawn, industrialism, have so atomized human relations, so specialized human activities, so externalized human contacts, so entangled human nature in a net of technical functions, that the loving, intimate, comprehending relations of man to man have given way increasingly to the hostile, abstract, uncomprehending relations of "pressure groups" and mass organizations one to another. A face-to-face community, in contrast, is required for the appreciation of personality in its fullness and integrity.

As an illustration of the callousness of people in a great city, I might cite an Associated Press dispatch from Chicago dated August 13, 1947. It tells how a man was waylaid and robbed and then left late at night in an area of cheap bars and flophouses, to stumble along barefoot, shirtless, gagged, and with hands bound. He wandered into one saloon and then another to secure aid. In each case, the bartender took one look and ordered him out. He then approached a couple walking along the street and, with the gag in his mouth, tried by a pleading expression on his face to enlist help; but the two passers-by walked briskly away. Finally, he managed to work loose his hands, removed his gag, and went into another saloon to tell his story. The bartender, who consented to listen, tossed him a nickel to call the police. This misadventure, although extreme, illustrates the indifference—the lack of sympathy and understanding—that only a great city can breed.

It stands in contrast to the sense of the preciousness and intrinsic dignity of personality which our civilization has so largely lost. We remember, as if from another world, how a crisis that churned France to its depths resulted, in great part, from the mistreatment of a single Jew, Alfred Dreyfus. It is even harder to realize that, prior to the first world war, civilized people throughout the world became incensed when a German officer in Zabern brutally attacked a lame cobbler who refused to yield the sidewalk; and that the German Reichstag, because of this military arrogance, censured the government by a vote of 293 to 54. What a vast chasm there is between such intimate concern for human personality and the incredible mass sadism of Buchenwald and Maidenek! The fascists' degradation of human beings to the status of things, their cruelty and contempt for life, is an extreme expression of the mass anonymity of a depersonalized civilization in which the relation of men to machines outweighs the relation of man to man. There are many causes of fascism in addition to mechanization and urban overconcentration; but the tendency to be callous toward human beings, so pathological in the case of fascist behavior, is endemic to a highly urbanized and technological civilization in which human contacts are casual, fragmentary, psychologically remote, and sometimes deliberately cruel.

If we are to regain a keen sense of human dignity and worth, we must somehow reconstitute the small face-to-face community. There is no other environment that can nourish the intimate values of life. In such a community, whole human beings are related to other whole human beings; they know each other well. In a great city, on the other hand, people know one another, for the most part, only in fragmental, partitive relations—as abstract, anonymous, voiceless entities in massive impersonal aggregations where the sense of human fellowship is lost. All the great religions of human brotherhood, it is worth remarking, have sprung from a pretechnological, preindustrial, primary group life, and they have always expressed themselves in the language of that life, such as "the brotherhood of man under the fatherhood of God." Even democracy, as a system of values, is dependent upon sympathy, mutual aid, and the warm appreciation of personality—upon attitudes, in other words, that can flourish only in intimate and direct associations.

If democracy and human brotherhood are to survive, if the very future of mankind is not to be jeopardized, we must remaster the machines that have mastered us. By means of humane and democratic planning, we must guide technological trends, counteract some and encourage others, so as to reconstitute and conserve the small intimate community. No sensible person would wish to demolish the metropolis or the technology that has produced it; the city will always have an important function, and many people will find it congenial. What one should object to, however, is the lack of balance in modern life: the hypertrophy of urban congestion and cosmopolitanism and the atrophy of communal sympathy and local creativity. In the task of reconstruction, nevertheless, it is just as important to bring to the small community the real advantages of city life as to introduce into the city the quiet and more intimate ways of rural life. These two sets of values, metropolitan values and community values, must become more interpenetrative. Modern technology must be redirected into channels favorable both to the decentralization of the city and the cosmopolitan broadening, and thus the enrichment and strengthening, of rural and small-town life.

The present inhuman concentration of activities keeps on apace because the smaller communities are relatively incomplete; but there are already signs in the evolution of technology itself that this incompleteness can be removed. With the development of rapid transit, the labor supply can be more freely dispersed: the workers need not live in the squalor of overcrowded areas near the industrial plant; and people can have access to the city and its metropolitan services without living immediately in the city. Moreover, new methods of converting common materials to industrial uses are making every back yard a potential source of wealth: perhaps soon we shall be making most of our aluminum from the common clay of the soil, or be manufacturing plastics and many other synthetics from vegetable products grown on the farm or from the ordinary materials of the earth. Also, the technological aids to living—automatic heating plants, mechanical refrigerators and deep freezers, electric washing machines, radios and phonographs and television sets, small power tools, etc.—are making the home establishments and the neighborhood community relatively complete and independent. The development of local theaters, libraries, educational and cultural centers, and outlying shopping facilities can do a vast deal to enrich life in these new decentralized home areas. With the modern development of transportation and communication, moreover, greater choice can be exercised in setting up intimate communities:

like-minded and congenial people can flock together and can organize the community life in accordance with their mutual interests. Thus, the limitations imposed by the size and isolation of the small community can be, and to some extent *are* being, removed by technological and cultural advance.

A very important factor in this movement toward decentralization is the development of new sources of power—electric and atomic. When electric power becomes fully exploited, its inexpensive conductivity will greatly reduce the aggregation of industries near coal fields and transportation centers; it makes industrial production in the small town feasible. Military considerations, connected with the release of atomic power, may in time give an immense impetus to decentralization, since the concentration in huge cities enormously increases vulnerability to air attack, including the atomic bomb. One advantage of atomic power, when applied to constructive peacetime uses, is that the small bulk of the uranium or plutonium from which the energy is derived reduces the difficulties in the distribution of fuel and therefore permits the location of power stations in scattered areas near the consumers. This feature makes possible the industrialization of regions now unsupplied with coal or water power, where the absence of a power supply at the present time presents prohibitive difficulties. All these factors make for a decentralized economic and social structure.

Enough statistical evidence is already at hand to show that technology is, in fact, beginning to create a new type of social organization—less centralized, less congested, less impersonal, and less bureaucratic: that the centripetal city is giving way to the centrifugal city and the small community. Under the influence of automobiles and other forms of rapid transit, suburban villages and outlying trading centers are multiplying, and some of the smaller towns are beginning to revive.

In response to these trends, there has sprung up a number of writers and thinkers—a group of decentralists: men like Arthur Morgan, David Lilienthal, Baker Brownell, Frank Lloyd Wright, Lewis Mumford, Ralph Borsodi, and Paul Meadows. They believe that the new technological trends toward decentralization can be greatly stimulated by social planning and community organizing. They find a practical demonstration of some of their theories in the great T.V.A. experiment, with its decentralized type of administration and its use of an advanced technology in behalf of the smaller communities. They hope eventually to reestablish a person-centered culture and an intimate neighborhood life upon the basis of the new technological trends and the rich resources of modern life. Some of the "decentralists," such as Ralph Borsodi, have gone to reactionary extremes in deriding the machine and denouncing mass production and distribution; but others wish to turn ahead rather than turn back the hands of time.

THE reconstitution of the small community, however, is but half the problem. The organization of the "great community"—the international accord of all peoples—is no less imperative. Unless men in the small community learn the values of love and intimacy, and unless they extend the good will thus acquired to all nations, technology will prove lethal to civilization.

Everybody, in a sense, lives two lives—one life in a small, and the other in a great, circle. He is a member of various intimate groups—the family, the neighborhood, the occupational association, the cluster of friends. But he is also a member of a whole national society and, in the modern world, he is living within an interdependent system of nations. We cannot regard the national or international society with the same intimacy, the same loving concern and warm appreciation, that we naturally feel toward our friends and intimates. Yet certainly our life in each of these circles is modified by our life in the other. Our attitudes toward great national or international issues will strengthen or weaken our friendships or other intimate ties. Likewise, the cultivation of love and sympathy in the small community will necessarily affect our capacity to feel good will at greater remove from ourselves. It has always been the teaching of Christianity and other ethical religions that the feelings of the "good Samaritan," although nourished in the small community, can be extended to a much wider circle of human beings. No doubt there is a profound qualitative difference in our respective attitudes, but in a certain sense we can "love" not only our friends but all mankind.

It is in this wide sense of the word that Auden writes in one of his poems: "We must love one another or die." A decade ago this line would have sounded melodramatic, but it is now merely realistic. The facts are indeed stark, and although familiar, bear repeating. The scientists who made the bomb, the authorities who know it best, are agreed about certain essential facts. Most of the principles involved are no secret; apparently, any nation with a considerable body of competent scientists can in a few years learn to make it. No effective defense is possible: if only a plane or two gets through the antiaircraft defense, or if only a few bombs are planted by enemy agents, the destruction will be colossal; and, in the event of a surprise attack, the major damage can be done before the defenses can be mobilized. A single "improved" atomic bomb can devastate ten square miles of a city. Ansley J. Coale, in a comprehensive analysis of the social and economic consequences of the bomb, prepared under the auspices of the Social Science Research Council, has estimated that a concerted surprise attack upon the United States might result in fourteen to seventeen million deaths in a single day or even in a few hours. Einstein has estimated that in an all-out atomic world war, with both sides thoroughly equipped, two-thirds of all mankind might be slaughtered. I shall not discuss the even more dire prophecies, for example, that an atomic war would so poison the earth and the atmosphere that it would leave vast continental areas uninhabitable.

Other weapons may be no less deadly. Radioactive poison gases can be easily made as by-products in atomic-bomb plants and may rival the bomb in lethal efficiency. Another type of poison can be made in the biologist's laboratory; and it is much more controllable than any bacteriological weapons that could have been produced in the past. Until recently, biological warfare has been too dangerous for any nation to utilize, since an epidemic deliberately spread by the wholesale dispersion of germs might sweep the enemy like a scourge but probably would also sweep the world. Now, however, scientists have learned to separate inanimate poisons produced by germs from the germs themselves. The botulinum toxin, for example, is a by-product of butolinus spores; it can be used to spread death throughout enemy territory, by poisoning the water or food supply, without the risk of a world epidemic. These new poisons made by germs are much more powerful than anything made by man, and are lethal in almost infinitesimal quantities. Moreover, newly developed fire bombs, used as warheads in rockets or guided missiles, may be a weapon almost as sudden and decisive and deadly as the atomic bomb or the biochemical poison. With the use of advanced methods of saturation bombing, these incendiary weapons, containing a gelatin compound that defies normal methods of fire fighting, can be used to send up in flames great harvest areas and forests and cities. Such considerations led General H. H. Arnold, a hard-headed military man, in his last official statement as head of the Air Forces of the United States, to declare that "the only hope for preserving the values of our civilization" lies in the prevention of war. Similarly, Dr. Vannevar Bush, the director of the Office of Scientific Research and Development, has remarked that "non-atomic weapons which were under study, in preparation, or in existence when atomic bombs ended 'the recent war' could by themselves when fully developed obliterate civilization."

In the past, there was always the danger that some particular group or society would go astray; but now there is the danger that mankind itself will miss its way. Civilization, once so solid, appears fragile as an eggshell. After fighting a war of unimaginable horror, after inventing weapons of incredible deadliness, the nations are plotting, blundering, and drifting toward a war far more dreadful than the last. If war follows war, the finis may be universal genocide.

War cannot be prevented if, because of social injustice and the hatred that it breeds, creed is pitted against creed, race against race, class against class, *within* and not only *among* nations. For the peoples of the world are now interdependent; technology has so shortened distances, so interwoven lives, that a profound internal crisis, such as existed in Germany during Hitler's rise to power, endangers the peace of the world. In a very real and terrible sense, therefore, we are all our brothers' keepers.

THE potentialities of modern technology, however, are by no means simply destructive. This fact is emphasized by the chairman of our Atomic Energy Commission, David Lilienthal, who has made the following comment upon the destructive powers and the constructive opportunities provided by atomic fission:

> Look—this thing is tough. It is grim. It is not a happy experience to be living with a nightmare. But we must talk about it as mature people and not react like children to a horror tale at bedtime. Let us see what we can do toward insuring against atomic warfare, rather than waste our energies fearing the threat of it. Let us see to it, too, that the shadow of the bomb does not blind us to the scope of atomic development as a potential blessing to mankind.

This is sound advice. We need sober thought and quiet determination rather than hysteria; and we need to recognize the great constructive potentialities of modern science and technology. If men make proper use of the forces that are being unleashed, their destiny will be glorious rather than terrible.

Scientists are now penetrating the heart of the atom; they are engaged in the most fundamental discoveries; and just as basic discoveries led to profound technological changes in the past, so these new discoveries can be expected to produce like changes in the future. The study of molecules gave us chemistry, with its innumerable beneficent applications; the study of electricity and magnetism gave us the vast electrical industry, with its immense contribution to the power and convenience of living; the study of bacteria gave us germicidal medicine, with its great extension and improvement of life. Now scientists have made this new discovery of atomic fission, the most basic discovery, some experts have told us, since man learned to produce fire. Surely the results of this new fundamental research will also be very great. As a recent writer, David Dietz, has remarked: "Unbelievable as is the atomic bomb, it may bear the same relation to future manifestations of atomic energy as the 'electric machines' of Benjamin Franklin's day bear to the hydro-electric generators of Boulder Dam." If not perverted to evil ends, this future development of atomic energy will be very good. We are so gripped by fear that we all tend to think of atomic fission as a weapon of death, but actually its peaceful development promises a future of abundance for mankind.

In other respects, also, technology is striding forward with seven-league boots: in the development of electronics and synthetics; in food chemistry and medicine and sanitation; in the application of psychological insight to mental illness; in the invention of innumerable cunning machines. Relatively retarded are the social techniques—the methods and devices of the educator, the economist, the statesman, the community planner; but even in these fields there is a remarkable creative ferment, a vast deal of experimentation. It may be, as some authorities have told us, that science and technology have been merely rehearsing and that the real play is yet to be enacted. It may be that we are trembling on the verge of a new technological revolution that will dwarf any in the past.

The constructive potentialities of applied science, however, cannot be fully visualized unless we consider its relation to other cultural factors. Science operates within a vast complex of interdependent forces that cannot be neatly and simply described; but perhaps the most fundamental aspect of this many-sided process is the interplay of inventions and traditions. An invention is the contrivance of something new, and a tradition is the preservation of something old. In our scientific age, inventions are predominantly the machines and techniques that spring from applied science. Traditions are the institutions, ideologies, and forms of behavior inherited from the past. These two factors, the one relatively dynamic and the other relatively static, are variously related: the traditions fetter the inventions; or the inventions disrupt the traditions; or either one perverts the other; or they condition and transfigure one another; or the force released by an invention may put an unprecedentedly powerful drive into some traditional institution or mode of action.

As a result of inventions, the techniques of transit and communication, for example, have developed with extraordinary swiftness and have collided with the traditions of the parochial community and the absolute nation-state. The speed of transit has markedly accelerated only in the past hundred years. The previous rate of travel may be illustrated by Napoleon's headlong flight from Russia. Despite the fact that he deserted his army and sped forward as rapidly as he could, it took him three hundred and twelve hours to traverse the distance of fourteen hundred miles from Vilna to Paris. This meant an average of less than five miles an hour. One of Napoleon's contemporaries, Lord Byron, loved speed and wrote about it:

> Now there is nothing gives a man such spirit,
> Leaving his blood as cayenne doth a curry,
> As going at full speed.

Yet full speed for Byron was the gallop of a horse! A radical change was dimly in sight when the first practicable locomotive surprised the British population in the year 1829. By the end of the nineteenth century the velocity of transit had become "startling." Henry Adams felt that the automobile, in its development from 1893 to 1900, "had become a nightmare at a hundred kilometres an hour": yet the automobile of 1900 would, of course, seem very primitive and poky if we had to use it today. The main acceleration in speed, therefore, has occurred in the twentieth century, reaching its climax in air travel that now approaches the swiftness of sound.

It is possible to trace a similar revolution in communication likewise achieved for the most part in this century. Beginning with relatively slow methods of transmission such as the pony express, communication has accelerated until it has reached its upper limit, the speed of light, and this ultrafast method is now, by means of the radio, available to almost everyone.

What has been the impact of these inventions upon the parochial community and the absolute nation-state? Several things have happened. First, intervals and distances have been so greatly abbreviated, and the parts of the world have become so interdependent, that the old parochial divisions, ethnic and geographic, have suddenly become obsolete. Second, these new technological circumstances, although they have made the exclusive racial community and the "sovereign" nation-state anachronistic, have also immensely reinforced their drive and power. For example, if it were not for modern methods of communication such as the radio and the movies, the fascists could not have employed such terribly effective propaganda to intensify racial and nationalistic bigotry; and without modern methods of transport such as the tank and the airplane, they could not have waged a totalitarian war. Third, the parochial traditions, in turn, react upon the wonderful new inventions of communication and transport, warping their application. Peace-serving inventions are fettered and strife-serving inventions are stimulated. The development of military aviation, for example, has taken precedence over the development of civil aviation. Fourth, the new techniques of communication and transport, though they contribute to the senile flare-up of parochialism, also challenge, disrupt, and displace the obsolete traditions and strengthen the opposing forces of internationalism and interracialism. The ideal of the United Nations is rendered cogent and practical by the very technological developments such as the airplane that made possible Hitler's assault upon civilization. To the extent that communication and rapid transit undermine the intimate, face-to-face community, they are striking at a very essential ingredient of our culture; but, insofar as they build "one world" while conserving the values of the small community, they are ensuring the revitalization and survival of civilization.

A similar analysis can be made of the complex interaction between other inventions and traditions. Equally important, for example, is the interplay between the relatively new techniques of industrialism and the relatively old traditions of "rugged individualism" and "free enterprise." Karl Marx, Thorstein Veblen, Lewis Mumford, and others have analyzed the interaction of these factors; and they have revealed how complex and fecund is this interplay, involving the action and reaction, conflict and mutual transfiguration, of inventions and traditions. I have said enough, however, to emphasize that technology operates within a cultural context, that its potentialities are both destructive and constructive, and that it must be guided and controlled if its constructive development is greatly to outweigh and exceed its destructive tendency.

In view of the dynamic development of technology, it has been called "the blacksmith of civilization." We are now in a position to judge both the truth and falsity of this appellation. Technology implements each new civilization: by supplying the society with tools and energies, it establishes the limits of its potential achievement; and by challenging and disrupting traditions, it casts old forms of life, which are often a barrier to progress, upon the ash heap of obsolescence. However, at times its effect is to injure or retard. It may, for example, so swiftly and violently disrupt traditions that it fatally breaks the historical continuity of the civilization. Or, by supplying great power and efficiency, it may lend terrific drive to vicious traditions and aggressive intentions. Or it may so predominantly serve the rich and powerful that it tragically accentuates the chasm between the underprivileged and the overprivileged. Or it may become so refined and specialized that it ceases to minister to the fundamental needs of man.

Because it frequently does *not* promote the values of civilization, there is no necessary correlation between its growth and the growth of civilization. This generalization is confirmed by Arnold J. Toynbee in his erudite *A Study of History,* in which he examines the vicissitudes of twenty-one full-fledged civilizations and a number of "arrested" or "abortive" civilizations, including every great civilized society of which we have any sufficient historical record. Upon the basis of this survey, Toynbee comes to this conclusion:

> If we set ourselves to plot two sets of curves—one set representing the vicissitudes of civilizations and the other the vicissitudes of techniques—the two sets of curves not only fail to correspond but display wide discrepancies. We have found cases of techniques improving while civilizations remain static or decline and cases of techniques remaining static while civilizations are in movement, either forward or backward as the case may be.

It follows from this conclusion that technology *is* as technology *does.* It is a boon to mankind if it serves human interests; it is an abomination if it loses touch with these interests or serves the enemies of mankind. What, then, can guarantee its right use? Nothing but control by a democratic, inclusive, cooperative community. Technology cannot control itself: it cannot be controlled by the technicians: it cannot be successfully controlled by any limited or special group: it must, therefore, be controlled by the whole community through its representative agencies.

Some people, beguiled by the marvelous potentialities of applied science, have assumed that neotechniques will simply and automatically evolve into a vastly better world. This is a very plausible view because science *can* do wonders for man. There are great constructive potentialities, for example, in the development of electricity and electronics; in the use of the new alloys and light metals and synthetics; in the tapping of new sources of power, including atomic fission; in the invention of ever more automatic and fecund machinery; in the employment of radioactivity in medical therapy and radioactive "tracers" in medical research; and in the production of the sulfa drugs, penicillin, streptomycin, and other bactericides. When we consider such great scientific developments, we realize that science has given man the means to achieve a far healthier and happier existence. Since it is difficult to avoid wishful thinking, we are tempted to suppose that what *may* happen *will* happen. To be realistic, however, we must check ourselves: we must realize that a fascist government, or any gang of willful, selfish men, will misuse these neotechniques as readily as they will misuse any techniques. Moreover, the new instruments and methods are as destructive as they are constructive: witness the atomic bomb! What is decisive is *who* controls technology and *for what ends*. Technology cannot control itself.

Nor can the technicians alone do the controlling. Nothing would seem more natural than to suppose that the proper people to control technology are the scientists and engineers who produce it. The popular reform movement that calls itself "technocracy" is based upon this assumption. But technicians cannot and should not control technology. They cannot because they are not powerful enough; they are not, and cannot be expected to be, dominant as a class. Indeed, they lack enough money of their own to supply their expensive laboratories and equipment, or to maintain themselves and their families while working on long projects. Therefore, they are dependent on those who control the purse strings and are necessarily the servants of the dominant class and the dominant ideologies. In one country, for example, they will work primarily in the interests of big business; in another country, they will fit into the designs of a socialist planning board; in still another country, they will work for the fascist war lords. Even if we could imagine social changes that would make the scientists and technicians dominant as a class, we still would have no good reason to say that they should control the applications of technology. As Aldous Huxley has reminded us in his brilliant satire, *This Brave New World,* the objectives of the experts may not be the humane and legitimate objectives of the community. An economic expert, for example, may conceive and plan an economically efficient society of willing, or perhaps even unwilling, slaves; or a military expert may turn the society into an armed camp. In general, scientists and technicians are men of a high degree of specialization, and consequently they often have a very limited grasp of social objectives. Moreover, as individuals who have benefited from a long and expensive education, they are usually drawn from the more privileged groups—privileged, at least, in comparison with the masses—and they often retain something of the class bias of their relatively fortunate status. If we are sincere democrats, moreover, we will reject the ideal of an oligarchy of technicians. We will prefer the ideal stated by David Lilienthal in his *TVA: Democracy on the March* that the experts should educate the people, so as to enlarge their political choices by revealing what alternatives technology makes possible; but that the people, in turn, should educate the technicians so as to make clear the wants and needs of the community and thus to define the great ends that technology should serve. In order to secure this process of mutual instruction, what is required is not an oligarchy of technicians, but a democracy that will bring the people and the experts together in beneficent interaction.

If the technicians acting alone are not fit to control technology, neither is any other limited group. We know the immense suffering that was the price of the old industrial revolution, and we know how frequently it deepened the chasm between the overprivileged and the underprivileged. Now we are in the dawn of a new technological and industrial revolution probably far greater than any in the past. We should resolve that this second revolution will not be bought at the terrible price of the first and will not be used to deepen the gulf between the many who toil without enjoying the fruits and the few who enjoy the fruits without toil. No private interest working for profit, no restricted gang scheming for power, can guarantee that this will not happen. An impressive body of evidence has been collected by governmental agencies such as the Federal Communications Commission, the National Resources Committee, and the Temporary National Economic Committee, and by individual social scientists such as Bernhard J. Stern and Wendell Berge, to show that private business and other special interest groups have in many instances thwarted science, opposed technological progress, and maintained their private "rights" above the interests of the community. We cannot, therefore, permit applied science—which can either destroy civilization or give it a new birth of freedom

—to fall under the control of a little grasping plutocracy, or a scheming military cabal, or an irresponsible political crew.

The only alternative that remains is to put technology under the control of the whole community operating through democratic processes. This community may be local, regional, national, or international—small or great—depending upon the scale of technological factors brought under control. Only such a community, organized justly and operating democratically, can serve the common cause and avoid partiality, segmentation, and conflict of interests. Only the community, representing the composite interests of all, can weave together the human, natural, and technological factors required for a balanced and economical development of resources. Only T.V.A., as a comprehensive democratic planning agency, for example, can achieve an integrated use of technicians, technologies, and machines, and land, minerals, waters, and forests in their interaction and interdependence and in their relation to the *people* of the Tennessee Valley. Only the whole community, acting through its responsible planning agencies, is great enough to perform the greatest tasks of technology, such as the development of water power and land reclamation throughout the great Columbia and Missouri valleys, or the full, constructive exploitation of atomic power. Nothing short of the whole community, operating through a National Research Foundation and kindred agencies, has the resources and interests adequately to support science and technology. Only the community, guided by an intelligent awareness of its own needs and interests, can weigh various technological factors, and emphasize some as more consonant with social welfare than others. Only such comprehensive social control, aiding and abetting the lagging factors in the culture, can hope to rectify the profound cultural disequilibrium that results from the relative underdevelopment of the humanities and social sciences in comparison with the immense development of the physical technologies and the natural sciences.

Admittedly, there are great difficulties in achieving a democratic control by the community of the rampant forces released by applied science. The toughest problem is the readjustment of the old institutions of private property and national sovereignty to the new factors of technology and industrialism. So long as irresponsible private wealth and the absolute nation-state are more powerful than the democratic community, the problem of achieving a humane control over technology cannot be solved. By means of social regulation or social ownership, therefore, the community must assert its supremacy over irresponsible private economic power, especially over vast cartels, monopolies, and billionaire corporations. By strengthening world government, the international community must achieve dominance over the irresponsible nation-state. We, the citizens, operating through the state and other civic agencies, should bring about these changes democratically and experimentally, adapting our policies to a host of concrete circumstances that we cannot foresee. The means must be flexible, but the ends must be clear and the determination unfaltering. If we do not act with resolution and dispatch, the alternative of war or revolution will eventually overwhelm us.

Is catastrophe inevitable? Some men believe so. "Optimism is cowardice," argued Spengler. The only allies of technology, he maintained, are imperialism and war. "Not so!" we whisper in our hearts. "Power is good if he who wields it is good." Technology gives us stupendous power either for good or ill. If for good, we must preserve and revitalize the small, intimate community, in which men learn the ways of human fellowship, and we must create the great community of all peoples, which will find institutional expression in a powerful world government. Only devotion to the community, small and great, can ensure survival. If we decide, instead, to continue the old game of reckless profit-seeking, urban concentration, power politics, imperialism, and selfish nationalism, our fate will be sealed. In the words of Thomas Hardy:

> No auguring mind can doubt that deeds which root
> In steadiest purpose only, will effect
> Deliverance from a world-calamity
> As dark as any in the vaults of Time.

LIFE'S GREATEST EVIL

ALEXANDER F. SKUTCH

Dr. Skutch took his doctorate in botany at Johns Hopkins in 1928 and spent the next two years in Panama and Honduras studying the anatomy of the banana leaf. At that time he became interested in the birds of Central America and resolved to make a study of them. This he did, combining his bird studies with plant collecting. Having, in the course of his travels, come to look upon Costa Rica's Valley of El General as his home, in 1941 he bought a farm of 50 hectares, newly carved from the forest. Here he has lived ever since, doing subsistence farming and devoting his spare time to the study of wild life and to writing.

DURING my senior year in college, I used to indulge in long philosophic discussions with a lower classman—now a distinguished professor, but then still in the overwise stage of early learning—who held that there is neither good nor evil in the world, but only survival and failure to survive. After long pondering, I believe that I can define good and evil in terms to which even a biologist of the mechanistic school can hardly take exception. At least, I fancy that I can do so for evil.

The great evil of life is parasitism. In the vegetable kingdom it is easy to recognize a parasite, a plant that does not manufacture its food directly from inorganic substances present in the soil, water, and air, but removes it from the tissues of other living plants, or even from animals. But when we turn from the vegetable to the animal kingdom, the definition of a parasite is not so simple. In the sense in which we apply the term to plants, all animals are parasites, for they must take their nourishment from other living things. For them we need another definition of parasitism, and it will be well if we can apply this definition also to parasitism among plants. For the purposes of this discussion, we may define a parasite as an organism that lives without making use of those faculties which are the peculiar characteristics of the class of organisms of which it is a member. The outstanding characteristic of plants as a whole—what sets them apart from other groups of living things—is their ability to build up organic compounds from inorganic substances, using for this purpose the energy of sunlight or, in rare instances, forms of chemical energy. Any plant lacking this faculty is by our definition parasitic, although I would exclude from this category the saprophytic fungi, which possess the peculiar faculty of breaking down dead organic substances and are, by virtue of this, most useful organisms, playing an indispensable part in the economy of nature. Mistletoes and other plants of similar habit are generally called half-parasites—a well-chosen term. They possess the faculty of photosynthesis, but lack another ability almost equally widespread in the vegetable kingdom, that of absorbing water and salts directly from the environment; rather, they take them from the tissues of some other living plant. Vines are another kind of partial parasite because they lack still another prime characteristic of vascular plants, that of holding their stems erect.

The peculiar faculties of animals are the directive senses and the power of locomotion. The host of flukes, tapeworms, cestodes, and other animals that live entirely within the bodies of bigger animals—unpigmented, sightless, deaf, practically devoid of the power of independent locomotion—are parasites in the highest degree. A tick that lives on the outside of its host is scarcely less a parasite; it merely sits down and sucks blood, has poorly developed sense organs, and only the weakest powers of locomotion. But a mosquito, which also gorges itself with our blood and is almost equally repugnant to us, is not a parasite. Its senses are acute, and it possesses a high degree of agility, as we soon learn when we try to swat one of our tormentors of this class—at least before it becomes bloated and heavy with blood. In its manner of life it is almost in the same class with the bee, which sucks the juices of flowers rather than those of red-blooded animals. The shark, although a carnivore, is not a parasite; but the remora, which attaches itself to the shark and shares its food, is a parasite, because it has lost much of the faculty of swimming, a prime characteristic of fishes. Among birds, cowbirds, cuckoos, and other species that drop their eggs into the nests of other birds to be incubated by them are justly called parasites; although in their manner of flying, obtaining food, and the like they are normal birds, they have lost one of the primary avian faculties, that of building nests, with the subsequent hatching of eggs and attending the offspring.

Parasitism is an unmitigated evil—an evil that cuts two ways. It results in the degeneracy of the parasite; and in the host species it causes immense destruction while contributing little or nothing to the evolutionary development of that species—perhaps even causing retrogression if the parasitism is heavy. The degeneration of parasites, both structural and functional, is so obvious and has been so often discussed that here we need only to call attention to it. Compare any parasitic plant devoid of chlorophyll with any leafy green plant; a tick or other sessile bloodsucker with a spider or any flying insect; or, in the particular field in which it is parasitic, a cowbird or European cuckoo with any nest-building bird—the points of degeneration are obvious.

Turning now to the effects of the parasites on the species that serve as hosts, when we consider the countless numbers of individuals that through many generations have been attacked or killed by the parasites, I think it altogether marvelous that parasitism has had so little apparent evolutionary effect upon the hosts either in structure or in habits. Among plants, I am familiar with no important structural modifications that might with confidence be attributed to the selective influence of parasites. Perhaps some plants have developed heavier and denser bark, thicker and more impermeable cuticles, or increased lignification of internal tissues through the natural selection of individuals that, by these means, more successfully withstood the penetration of parasitic fungi. But these modifications of vegetable tissues appear to result from the influences of dryness, strong insolation, and other physical conditions of the environment rather than from the menace of parasites. At best, the modifications induced in plants by parasites are largely in the microscopic field rather than in gross structure.

Among animals, parasites appear to have had strangely little effect in modifying either structure or habits. When we reflect upon the countless millions of men who have succumbed to various widespread forms of disease caused by microscopic parasites—and some of larger size—it is unbelievable how little direct effect the incidence of these diseases has had on the dietary, sanitary, or other habits of mankind. I have heard it said that the widespread custom of tea-drinking in China arose out of the danger of drinking polluted water without boiling; and possibly religious injunctions against the consumption of pork were given as a preventive of tapeworm infection (although if this had been the real motive the danger could have been avoided by adequate cooking). But instances of this sort are uncommon, and at most they represent attempts to meet the menace of a pitifully small proportion of the parasites that from time immemorial have preyed upon mankind. It was only after scientific investigations—conducted largely within the past few generations—had shown us how by modifying our way of life we could avoid certain parasitic infections that the existence of these parasites caused any salutary change in our habits. In a restricted but fascinating field in which I am especially interested, I cannot discover that the parasitism of nests—extremely heavy in some species—by cuckoos, cowbirds, and their like has had any marked effect upon the nidification or other reproductive habits of their victims.

It is instructive to compare the evolutionary effects of the struggle between parasite and host with other forms of conflict in the living world: that between individuals of the same species or of species with similar habits, and that between predator and prey. Quite in contrast with parasitism, these forms of competition have had immense effect upon the development of both plants and animals. The competition of green plant with green plant for a place in the sun has led to the progressive upward elongation and strengthening of stems; it is apparently to this struggle that we owe the loftiest trees and the most impressive forests. Yet the competition between self-supported green plants and even half-parasites like vines and creepers—a struggle keen enough throughout the humid tropics—has led to very little modification in the structure of the victims, save perhaps here and there a quickening of upward growth, a reduction of branching in early life, and greater smoothness of bark, which make it more difficult for vines to attach themselves to trees. The competition between vine and vine can lead only to a chaotic tangle of vegetation.

Among animals with kindred dietary habits, competition has led to a sharpening of sight, scent, and wits; an increase in swiftness, endurance, and radius of foraging; and, to take a single classic example of modification in structure, to the lengthening of the neck of the giraffe, which enables it to reach foliage beyond the grasp of other browsing animals. Even more important, perhaps, this competition has led to endless diversification and specialization, with correlated changes in structure and function. Thus the flycatcher, the wood warbler, the creeper, and the thrush are all insectivorous birds, but competition for this type of food has favored specialization, so that the first catches it in the air; the second, among the foliage; the third, on the bark of trees; and the last, largely on the ground—and each has peculiar structures and habits to fit it for the part it plays in the economy of nature.

Predation has also been of the greatest importance in shaping the course of the evolutionary development of living creatures. It seems likely that the predation—if we may use the term in this instance—of grazing animals upon herbage has led to the development, in grasses and plants associated with them, of basal or intercalary modes of growth, which enable these herbs to resist the effects of grazing far better than plants with the apical type of growth that is more common in the vegetable kingdom. Since grasses cover so large a proportion of the land areas of our planet, the importance of this modification in mode of growth in response to predation can hardly be exaggerated. Among animals, predation has led to greater keenness of senses and fleetness of movement in both the predators and the prey, and to many curious modifications in form and color which help the latter to elude the sharp eyes of their persecutors. Among mankind, a few real or quasi predators, such as tigers and venomous snakes, have far greater influence upon the manner of living and thinking of primitive people than the parasites that cause them infinitely more harm. Only as science helps us to evaluate the real magnitude of the dangers to be feared from the various classes of our enemies is this situation reversed. My rather primitive neighbors are far more afraid of the *tigre* than I; I fear a host of invisible organisms they never think about.

We may lament the tremendous loss of life, even up to the extermination of whole species, brought about by the competition between individual and individual in a crowded world, and by the habit of one living thing preying upon another. But at least this competition and this predation have been fruitful in the progressive development of organisms in myriad diverse ways. Parasitism has taken its tremendous toll of life with scarcely any return that we can see; it has led to retrogression rather than to progress. Hence, we may call it the greatest evil of life.

The opposite effects on living things of parasitism on the one hand and predation and competition on the other are not far to seek. Modifications brought about in a species through the selective actions of the latter are of general value and usually result in the greater fitness of the organism to meet the varied stresses of its environment. The length of trunk that enables the jacaranda tree to hold its crown above that of the ira also serves to hold it above that of the campana and most others of its competitors in the tropical forest. The thorns that protect a bush from browsing by a cow usually also serve to guard it against the horse, the ass, and the deer. The fleetness that enables the antelope to outrun the lion will serve it in good stead when pursued by the tiger, the panther, and the wolf. The intelligence of the crow helps it not only in its quest of food, but also in outwitting its archenemy, man.

But the heritable modifications brought about in organisms through the selective action of parasites are as a rule neither in structure nor in habits, but only in the form of slight modifications in chemical composition, and these modifications are rarely of general value. A man may become immune to typhoid fever without acquiring immunity to lockjaw, although the bacteria causing these two diseases are so similar in appearance that they are classified in the same genus. Indeed, an individual may be resistant to one strain of a pathogene and susceptible to another; and it seems that some of the fungi and bacteria that cause disease may be constantly changing, either by selection or through mutation, so that they repeatedly overcome any resistance their victims might have acquired to them in their earlier forms. Such insidious, protean adversaries can have little effect in developing the species they parasitize; they serve only to destroy.

AMONG men the greatest evil is the same as that in organic nature as a whole—parasitism. In order to apply to mankind our definition of a parasite as "an organism that lives without making use of those faculties which are the peculiar characteristics of the class of organisms of which it is a member," we must first decide what are the peculiar, distinguishing characteristics of the human species. Considering chiefly the means by which each group of organisms maintains life, the peculiar faculty of men is their ability to produce what they need by mental or physical effort or, more usually, by a well-balanced combination of the two. This ability distinguishes all but the lowest savages among men from all other mammals except, to a certain extent, the beaver, and from all animals of other classes except a few of the social insects, notable among which are the agricultural atta ants.

The parasites among men are those who exist through the efforts of other men, producing nothing themselves. They include the idle rich who live in sloth on inherited fortunes, the shiftless poor who live on charity or such windfalls as they can find, the thief, the swindler, the forger, the smuggler, and the panderer to the vices and follies of men. The highwayman, were he a member of the feline or the canine tribe, would not be classed as a parasite but as a predatory animal—although predatory animals do not as a rule prey upon others of their own species. He has often developed to a high degree the keenness of sense, the speed, the endurance, and the cunning of a predator; but he has not developed the peculiarly human faculty of productive physical or intellectual activity, and as a man we must class him as a parasite.

The losses to mankind by human parasites, their continued drain on human effort through the ages, have been incalculably great. In aggregate they have probably been responsible for a wastage of human effort of the same order of magnitude as that caused by wars. The total becomes far more stupendous if we include in the category of parasitism all those wars which were essentially parasitic in nature. From the raids of wild clans to carry off cattle, grain, or other coveted goods from their neighbors, through the strife of medieval barons and princelings who coveted their neighbors' estates, down to World War II—which began as a gigantic attempt by the mad, unprincipled leaders of a fanatical political party to enrich themselves at the expense of all the world—a large proportion of all wars have been for the purpose of plundering, and hence parasitic in character.

Despite this vast and continued wastage, it is amazing how little effect parasitism has had upon the essential structure of human society. This statement may at first glimpse appear extravagant. To test the effects parasites might have had upon the character of human institutions, let us imagine what would be the results of their complete removal. Could we somehow get rid of all thieves, swindlers, counterfeiters, forgers, and idlers, poor and rich, in what way would our customs and manner of life change? Surprisingly little, I believe. We should have fewer locks on our doors; but only an infinitesimal portion of human ingenuity has been devoted to the devising of locks and safes. There would be fewer policemen in the streets and a slightly lower tax rate, because we would not be obliged to support a large force for law enforcement nor institutions for the confinement of thieves and their like. We would be less careful in examining the signatures on our checks and other documents. We should be less preoccupied over the safety of our property, but otherwise we should go on living much as we have lived. The constant drain on human wealth and effort through the activities of human parasites has influenced our way of life as amazingly little as the unending menace to human life and health by smaller parasites has altered our pattern of living.

Again, it is instructive to compare the effects of parasitism with those of competition within the species. Honest and legitimate competition in business or other fields of human endeavor has ruined the hopes and blighted the lives of the countless men who have lost out in it, and caused unmeasured misery and suffering to them and their dependents—possibly as much as the loss of possessions through dishonest activities. The latter has been an unmitigated evil, whereas the former has had many favorable consequences for society as a whole. A manufacturer who by fair means drives his competitors into bankruptcy or puts them out of business must either make a better product, or the same product by a more efficient process, so that he can sell it at a lower price. Driven by the pressure to improve their methods of manufacture or starve, other producers of the same article will perfect their operations, and eventually the whole community will benefit through having cheaper and better wares. Competition is cruel but productive; parasitism, cruel and nonproductive.

Among men, as among all other organisms, parasitism is the absolute and unmixed evil—the evil that cuts two ways. For the parasite it results in moral and often, too, in intellectual and physical degeneration; for the hosts—the rest of society—it causes tremendous losses with no compensating gains.

SEND THE SM TO VETERANS' HOSPITALS

Having reason to believe that patients in Veterans' hospitals want to read scientific as well as popular magazines, the Engineers' Society of Milwaukee has appealed for contributions of technical journals to the local hospital. As the response was gratifying, Samuel R. Snead, of that Society, suggests that the appeal be made on a national basis. Specifically, we suggest that unneeded copies of the SM be sent to the nearest Veterans' hospital. The SM has the desired meat in it, without being terrifyingly technical.—Ed.

SCIENCE ON THE MARCH

METEORITES: COMET COMPONENTS

IN THESE boom days of science, we are inclined to look with condescension upon the ignorance and superstition of our forebears. Among the most persistent of these superstitions have been those of astronomical origin, and no celestial phenomena have spread more general terror than the unheralded appearances of spectacular comets. The long and impressive list of evils attributed to these visitors includes droughts, plagues, pestilence, and even the loss of the Battle of Hastings. Evidently, disasters of one sort or another have been so prevalent that a current misfortune has always been conveniently available to blame on any hapless comet.

The appearance of a great comet is still an awesome but no longer a fearful sight. Thanks to such men as Newton and Halley, it is known that comets are not capricious in their movements but are obedient to the same laws of motion as are the planets. Progress toward an understanding of the nature and origin of comets, however, has been slow. Although comets may rival the sun in size and possess tails as long as the diameter of the earth's orbit, their masses are so small as to defy any direct measurement. When Halley's Comet paid its last visit in 1909–10, it passed directly between us and the sun. Projected against the sun's disk, it was completely invisible, although, had a solid object 50 kilometers in diameter existed in its head, this should have been detectable with powerful telescopes. In 1927, when Comet Pons-Winnecke passed unusually close to the earth, its nucleus was subjected to the most careful telescopic scrutiny under conditions which precluded the possibility of its containing solid pieces larger than one or two kilometers in diameter. In short, direct telescopic observation gives no evidence of any solid matter in comets.

Our ignorance concerning the origin and age of comets is even more complete. No one knows whether comets are coeval with the solar system or whether they are relatively recent additions of local or interstellar origin. We may well wonder whether such nebulous objects as comets can be expected to have maintained their identity during the several billion years that the solar system has existed. The study of meteors and meteorites has provided, and may continue to provide, clues to the solution of these cometary enigmas.

The revelation that a meteor shower may be produced by a collision of the earth with a swarm of solid particles moving along the orbit of a known or defunct comet was one of the greatest astronomical discoveries of the nineteenth century. It was in 1866 that the Italian astronomer Schiaparelli noted that the swarm of meteorites responsible for the annual Perseid shower was moving about the sun in an orbit almost identical with that of Comet 1862 III. In 1940, Whipple, of Harvard, brought the definite associations up to nine by relating the Taurid shower to Enke's Comet. But the discoveries still continue, and in 1946 a shower was observed in Europe that is probably related to Tuttle's Comet.

There is little doubt that the particles producing these meteor showers originate in the comets. Where a short-period comet is still sufficiently intact to be seen, the particles are usually strongly concentrated along the orbit only in the vicinity of the comet. The closer the earth is to the comet when it intersects the comet's orbit, the more intense the resulting meteor shower is likely to be. It should be emphasized that all meteor showers have not been associated with comets; neither are all comets known to produce meteor showers. Long-period comets return to the sun so infrequently that their gravitational bonds appear not yet to have been loosened by the tidal action of the sun and planets, and no debris has been found in the wake of such an object.

Careful studies of these meteor showers are revealing much valuable information concerning the nature and distribution of solid matter in comets. In the case of the Perseids, where material is rather uniformly distributed along the orbit, the mass of the comet, before disintegration began, can be estimated. The swarm consists of particles a few milligrams in mass separated, on the average, by distances of over 100 miles. A knowledge of the size of the orbit leads to a figure of 500 million tons for the lower limit of the mass of the comet. Since the particles in a swarm are all moving at about the same velocity in space, the meteors that they produce on plunging into the atmosphere are proportional in brightness to the masses of the particles. In the seemingly old Perseid, Leonid, and Geminid showers, faint meteors are rare, and fireballs are also very infrequent. Showers from the debris of relatively young comets, such as Comet Giacobini-Zinner, show, on the contrary, a wide range in particle size: 1,000 to 1 range in mass, with the small particles the more numerous. Observational data obtained from the shower of October 9, 1946, by La Paz at the Institute of Meteoritics of the University of New Mexico show not only that the particles trailing Comet Giacobini-Zinner are subject to considerable variation in size, but also that they exhibit a pronounced tendency toward clustering. The theory of relativity predicts a gradual segregation of the particles of a swarm into different orbits according to size (the Poynting-Robertson Effect). This circumstance is disappointing, as it indicates that the present population of a swarm may not reliably indicate the size of the largest particles originally in the comet. This segregation may be responsible in part for the fact that no meteorite from a shower has ever been known to reach the earth's surface. Those particles that were large enough to weather passage through the earth's atmosphere, if they ever existed in the swarms, are apparently no longer in them.

The complete lack of evidence that a meteorite from a shower has ever reached the earth's surface is of itself sufficient reason for questioning the similarity between the particles causing shower meteors and those causing meteors of the sporadic variety. There are excellent additional reasons. Fallen meteorites may be roughly classified as either irons or stones, and the spectra of sporadic meteors are found to be of two types quite compatible with the observed composition of the fallen meteorites. All spectra of shower meteors, on the other hand, indicate that the shower particles are stones. Sporadic meteors appear rather consistently at a height of 95 kilometers, whereas shower meteors that the earth strikes head-on appear at about 115 kilometers. Part of this difference can be attributed to the difference in velocity, but it seems necessary to postulate also a difference in constitution.

Precise data bearing on the friability of cometary meteorites were obtained by La Paz at the Institute of Meteoritics for the 1946 Giacobinid shower. Photographs of the shower were taken from the ground by the use of K-24 aerial reconnaissance cameras. Through the courtesy of the Army Air Forces, additional photographs were obtained from a B-29 flying above 20,000 feet. A bright fireball was photographed from the air. The photograph shows clearly that there were two flashes of light from the fireball before it disappeared. More precise pictures of the light variations along the paths of meteors were obtained by running microdensitometer tracings of the photographed trails. A photograph of the light curve of a bright Giacobinid meteor indicates that for this meteor there is evidence also of a double maximum. This was probably the result of a fracturing of the meteorite under the stress of the air through which it was rushing.

These evidences of differences between shower and sporadic meteors give rise to several fundamental questions: Why are they different? What is the ratio of shower to sporadic meteors? If the sporadic meteors are not of cometary origin, whence did they come? Clues to the solutions of these problems are to be found in a relatively new branch of meteor research that began with the application of photography to the study of meteor velocities. Harvard has pioneered in this work and, since 1936, has had a synchronized observing program in operation at

Cambridge and at Oak Ridge, Massachusetts. Cameras at both stations are directed toward common points about 80 kilometers above the earth. During exposures, rotating shutters occult the camera lenses twenty times a second. When a meteor is photographed simultaneously at both stations, its points of appearance and disappearance in the atmosphere and its velocity become calculable with considerable precision. The probable error of the velocity is of the order of 1 percent. Some 1,400 meteors have been recorded on Harvard plates, and at least 1,000 of these meteors have left measurable trails. At present, a meteor must rival a bright star in luminosity in order to record itself on a plate, but faster plates and cameras will make possible the extension of the work to fainter objects.

The British have carried out some successful meteor experiments with radar and have measured the velocities of some twenty invading meteorites by this method. In some cases, the reduction of velocity, owing to the retardation of the atmosphere, has been measurable.

The accurate determination of velocities that photography makes possible is of great importance in the study of the origin of meteorites. The rate of speed with which a meteorite enters our atmosphere identifies it as either a regular member of the solar system or a visitor from interstellar space. If the meteorite is exceeding the speed limit, so to speak, its path with respect to the sun is hyperbolic. If its velocity is below the critical value, its orbit is elliptical. Investigation points strongly to the fact that inexperienced observers tend, in estimating the point of appearance of a meteor, to extrapolate backward too far along the path. This procedure makes the corresponding velocity estimate too high and leads to too many hyperbolic velocities. In the fireball catalog of Von Niessel and Hoffmeister, 80 percent of the meteor velocities are listed as exceeding the parabolic limit. In 1931, Harvard and Cornell cooperated in a meteor expedition to Arizona, in an effort to accumulate accurate data based on the observations of experts. The results of the expedition, obtained visually, indicated that 66 percent of the sporadic meteors had hyperbolic velocities. Evidence of systematic errors, however, has been found in the data. The most significant information on meteor speeds is that, to date, no meteor that has been simultaneously photographed from Harvard's two stations has had a definitely hyperbolic orbit; in fact, none has had even a questionably hyperbolic orbit. In time, the photographic method may show that few if any meteors are produced by meteorites that are not regular members of the solar family.

At present, the ratio of shower to sporadic meteors appears to be at least one-half, and as additional data are obtained the fraction is increasing. It is currently estimated that three-fourths of the meteors observed from northern stations between August 2 and 22 each year are members of the Perseid shower. Hoffmeister reports evidence of meteor showers from meteorite streams moving in orbits of a planetary character with low inclination and small eccentricity. Some of these ecliptic currents, as Hoffmeister terms them, are very roughly defined and cause showers over periods approaching three months in length. He makes the interesting observation also that if the orbits of the Piscids, the Scorpio-Sagittarids, and the Virginids were each turned through 22° of longitude, they would closely coincide with the orbits of the minor planets Hermes, Adonis, and Apollo. Differences of the sort already mentioned between shower and sporadic meteors appear to exist between meteors from cometary currents and those from ecliptic currents. The meteorites have a different mass-frequency function, and the material in the ecliptic currents is more "consistent" than that in cometary currents. According to Hoffmeister, 68 percent of the cometary meteors brighter than magnitude 3.5 have trains, whereas only 18 percent of the meteors within the same brightness range from ecliptic currents exhibit trains.

Whipple, of Harvard, has independently discovered that all shower orbits are not typically cometary in character. From simultaneous photographs of five Geminid meteors he has calculated five reliable sets of orbital elements. On the average, the periods are only 1.65 years, and, when closest to the sun, the meteorites are within 13.9 percent of the earth's average distance from the sun. Their orbits are inclined 23.5° to that of the earth, and they have an eccentricity of 0.900. Such orbits are unique among those of both comets and minor planets.

If the fragments producing the Geminid shower were all moving in the same, or parallel, paths, all Geminid meteors would seem to emanate from a single radiant point in the sky, which would be the point of convergence of their apparent paths extended backward. Whipple's precise observations unquestionably indicate small variations of the radiant point, and hence of the elements of the orbits themselves. He is confident that the Geminid meteorites were once incorporated in the main body of a comet, and he favors the explanation that the present deviations from the mean orbit are the result of perturbations caused by the planets, particularly Jupiter, subsequent to some initial disruptive action. Whipple states, in addition, that a suitable theory of perturbations for the Geminid swarm should yield some measure of the age of the swarm—in other words, the length of time required to produce the present deviations from a mean orbit.

The application of precision methods to the study of the motions of meteors is still in its infancy, but the infant is a sturdy and promising one, and meteoriticists hopefully predict for it a brilliant future.

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ITINERARY*

FIRST ANNUAL INTERNATIONAL PHOTOGRAPHY-IN-SCIENCE SALON

Gamma Sigma Epsilon University of Florida Gainesville	May 13–June 1
The John Crerar Library Chicago, Illinois	June 8–June 21
Auburn Camera Club Bureau of Animal Industry U.S.D.A. Auburn, Alabama	June 28–July 12
American Osteopathic Association Annual Convention Boston, Massachusetts	July 19–23
Academy of Science Illinois Wesleyan University Bloomington	Otober 1-15

* Dates available for showing both 1947 and 1948 Salon pictures.

BOOK REVIEWS

WHEN DOCTORS DISAGREE

Private Enterprise or Government in Medicine.. Louis Hopewell Bauer. ix + 201 pp. $5.00. Charles C Thomas, Publisher. Springfield, Ill. 1947.

DR. BAUER, a member of the Board of Trustees of the American Medical Association and president of the Medical Society of the State of New York, is well qualified to present the official attitudes of organized medicine on the problem of more adequate distribution of medical services. I had hoped for something different after reading in Dr. Bauer's Preface that "the public, which, eventually, must decide the question, has not been given all the facts. The public wants to know both sides of the question." Unfortunately, the book presents only the well-known arguments promulgated by official spokesmen in support of the *status quo* in medicine.

The author devotes considerable space to the thesis that comprehensive Federal medical legislation such as the Wagner-Murray-Dingell Bill would "socialize" medicine. He overlooks the prime purpose of such legislation, which is to guarantee all our citizens preventive, diagnostic, and therapeutic medical services, independent of the vagaries of the economic cycle. Surely such a purpose is completely consistent with the highest ideals of our democracy.

Dr. Bauer gives the impression that only the low-income groups require financial assistance for medical services. Anyone who holds a contrary view he dismisses as a misguided left-winger. It is interesting that on March 8, 1948, the *New York Times* stated editorially:

> In the complex problem of providing medical care, one fact stands out clearly. The average person in the middle-income brackets simply cannot afford the mounting costs of first-rate medical care without some financial assistance or prepayment insurance.

The author is so preoccupied with fears about a future when the Public Health Service or other Federal agencies may supervise medical care that he completely fails to inform the reader how private enterprise can solve the following vital problems in medicine today:

1. The by-passing of the general practitioner by technical advances in medicine.
2. The place of group medical practice.
3. The salvage of rural medical practice, which has deteriorated alarmingly.
4. Adequate geographic planning of our national health facilities at a time when complex radioactive materials are required in diagnosis and therapy.
5. The inadequate education provided by 50 percent of our medical schools.

This list does not exhaust the many vital problems passed over by Dr. Bauer.

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STRANGE DEVICES

What Electronics Does. Vin Zeluff and John Markus. vii + 306 pp. Illus. $3.00. McGraw-Hill. New York. 1948.

THE associate editors of *Electronics* find in devices using electron tubes "a collection of engineering dreams come true and the Prince Charmings of electronics . . . happily married into the kingdoms of industry with every assurance of happiness ever after."

In concise accounts of some 400 applications and in 114 drawings and photographs, the authors emphasize the versatility of electronic devices and their uncanny ability to combine with ordinary instruments or machines to do jobs heretofore considered impossible. Time, weight, length, temperature, humidity, density, strain, viscosity, color, brightness, and many other variables can be measured by an electronic device of one sort or another, and the measurement can be translated into control.

There is little discussion of the limitations, economic or otherwise, of electronic devices. The physical principles on which they operate are discussed only incidentally, though accurately.

The numerous applications of electronics are presented, with some duplications, in 19 chapters bearing such titles as: In the Air, Afloat, Metals, Fighting, and Doctoring.

The general reader who wants to know what electronics can do for him will find in the lively descriptions of actual devices much up-to-date interesting information, and perhaps a stimulus for thinking of still newer techniques or better ways of doing old jobs. This book should be popular in school libraries.

ROBERT L. WEBER

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A SURGEON'S LIFE

George Crile, an Autobiography. Grace Crile, Ed. 2 vols. 624 pp. Illus. $10.00. Lippincott. Philadelphia and New York. 1947.

THIS "autobiography" is unique in that it was largely written in the form of notes and papers by Dr. Crile as his medical life unfolded and progressed. In the words of his wife,

> He knew that I was interested in his early beginnings and in all that came into his professional life, and since writing was his avocation, his satchel and pockets upon his return from out of town became for me a treasure store of autobiographical and wayside notes, amusing incidents and stories, dreams and plans for research and professional organization, and philosophical musing—all of which, for him had served their purpose since through writing he got them "out of his system" and, for me, they found their place in my files of his manuscripts and diaries.

These, religiously collected and carefully preserved by an adoring wife who felt that destiny had placed her husband in this world for outstanding achievements, reached the staggering total of more than 17,000 pages of manuscripts, a similar number of pages of diary, 38 additional volumes of war diaries, and 11 volumes of reprints.

From this mass of "autobiographical agenda" his wife, Grace Crile, who shared in a unique way in her husband's clinical, financial, and research activities and aspirations, has selected and edited the data which would in her opinion best depict the life and accomplishments of one of the great surgeons of a time when surgery was making some of its most outstanding advances.

Although the two volumes undoubtedly bear the imprint of the editor and reflect what she considers the more important parts of this mass of autobiographical material, additional interesting data from the point of view of the wife are given in the seven "side lights" written by her.

With this background and source of information, these two volumes of more than 600 pages lead one through Crile's early life and struggles, his successes, marriage, clinical and research activities, and down through the last years of failing health but years of intense activity and plans for the future.

Insight into the character of the man is given by the many references to strictly personal affairs, and his professional advancement is recorded in the development of a large practice with a group of lifetime associates, the rise to an important professorship in surgery, the experiences in two wars, many activities in the surgical world, and the establishment of the Cleveland Clinic Foundation.

A great deal of space is given to the development and progress of various research projects, particularly in regard to shock, control of blood pressure, hemorrhage and transfusions, effects of fear and pain, and the use of nitrous oxide anesthesia. As a result of these researches and observations, and certain deaths in toxic thyroid patients, he developed the idea of "stealing the thyroid gland." Later, in order "to prevent the discharge of nervous energy which would otherwise be caused by trauma," he blocked off the operative field with local anesthesia. In order to express this concept, the word "anoci-association" or "anociation," as later contracted, was coined.

Differences in electrical potential as an explanation of shock, and the development of the bipolar theory of life are described. There are many details of the hunting trips ranging from Florida to Africa, which he made to perform dissections on a great variety of animals differing widely in size and activity, in order to compare the relative sizes of the brains, hearts, thyroids, and adrenals, and the complexity of the sympathetic nervous systems, in an attempt to throw some light on certain human behaviors and diseases.

Even though his contemporaries placed different values and interpretations on these researches, and surgical advances and future developments may further alter their significance, their presentation here adds much interest to the life history of a great surgeon.

DERYL HART

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HIGH AND GHOSTLY MATTER

Telepathy and Medical Psychology. Jan Ehrenwald. 212 pp. $3.00. Norton. New York. 1948.

FREUD reported that he encountered telepathy in his psychoanalytic practice. Stekel wrote a book on telepathic dreams. Jung has long taken a reserved interest in parapsychological matters. Janet found what appears to have been telepathy early in his career, but did not follow it up. Among British psychiatrists, Mitchell, McDougall, and William Brown were frankly interested in the subject. Thus telepathy has been no stranger to medical psychology: nevertheless, Dr. Ehrenwald's book is the first full-length work on telepathy brought out in America by a medical man.

Dr. Ehrenwald is a New York neuropsychiatrist, formerly of the Universities of Prague and Vienna. He is a Fellow of the Royal Society of Medicine in England and is now associated with the Long Island College of Medicine.

As a psychiatrist Dr. Ehrenwald was less impressed by "the imposing array of statistical data furnished by laboratory experts" than by the "evidence of telepathy occurrences which can be observed in everyday life, in dreams, in the psychoanalytic situation, in neuroses and psychoses, if only we are prepared to meet them with an open mind." Our reluctance to do so is itself a "psychological symptom calling for inquiry."

The book deals first with telepathy in the relatively normal case, with telepathic dreams, and with the telepathic experiences often encountered in psychoanalysis and in trance states connected with mediumship. Next is a section on telepathy in psychotic patients, especially in cases of paranoia and schizophrenia. Dr. Ehrenwald concludes that telepathic research throws fresh light on the clinical picture and may have consequences for therapy itself. Finally, he offers detailed psychological analyses of two famous telepathic subjects and from the analyses draws implications for personality and personal relations. The book ends with a moderate and balanced comment on the significance of telepathy for better social organization.

J. B. RHINE

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Parapsychology Laboratory
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THE GRINDERS CEASE BECAUSE THEY ARE FEW

Your Teeth and How to Keep Them. Jerome J. Miller. ix + 232 pp. Illus. $3.00. Lantern Press. New York. 1947.

DENTISTRY is one of the most misunderstood of the sciences with which the general public comes in frequent contact. The average person has little or no idea of what is being done in his mouth, and the dentist seldom has the time to go into detail about his work. Miller's book does this task for the dentist in an admirable fashion. It begins with a description of the dental apparatus and its relationship to the general health, and proceeds to evaluations of the techniques of full and partial restorations of teeth. Between these extremes, Dr. Miller deals with children's dentistry, orthodontia, tooth-filling materials and their relative merits, mouth infections, wisdom teeth, and a factual discussion of the value of dentifrices and the ballyhoo that goes with them.

This book is a must for the person who is really interested in his teeth and who wants to know the how and why of his dentistry rather than remain in ignorance of facts so vital to his welfare.

RICHARD K. THOMPSON, D.D.S.

Washington, D. C.

WHAT TO DO ABOUT BACKACHE

The Back and Its Disorders. Philip Lewin. xii + 157 pp. Illus. $2.50. Whittlesey House. New York. 1948.

Dr. Philip Lewin is a well-known and well-qualified orthopedic surgeon practicing in Chicago. He has written many books and articles on orthopedic subjects, among them a textbook for nurses on orthopedics, which is most valuable; and an outstanding textbook for orthopedic surgeons: *Backache and Sciatic Neuritis.* Besides being able to write often, long, and well, he has a busy practice; and he spent four years, though not a young man, serving his country as a colonel in charge of a large orthopedic service in an Army general hospital during World War II.

In *The Back and Its Disorders,* Dr. Lewin explains concisely, for the nonprofessional person, the major causes of back pain. The word "orthopedics" is derived from the Greek meaning "straight child." The practice of orthopedic surgery involves the care of the bones and joints, their muscles, nerves, ligaments, and blood vessels. Backache is, therefore, primarily a problem for the orthopedic surgeon. Such a surgeon must have, as a sufficient background of knowledge, not only high-school and college education, but four years of medical school, followed by five years of hospital training under qualified surgeons and two years of practice under supervision. After that he must pass the American Orthopedic Board examination. Dr. Lewin has reached a stage in life where he can be an examiner for the American Boards. He knows whereof he writes as he tells you and me in clear and simple language, free from tough medical terms, about the back and its disorders.

There is no one living who does not suffer from a backache at some time. Dr. Lewin has pointed out many causes of this in his book and has alluded to many others. He has explained the more common causes in language anyone can understand. The difficulty in reaching an active diagnosis—its why and wherefore—is described, and the methods by which a physician must reach his conclusion are given. There was a time, not many years ago, when the catchword "sacroiliac strain" was the answer to most backaches from both a lay and a professional standpoint. Newer knowledge, acquired through years of research, has put that term in its rare and rightful place. Because of this same newer knowledge of the back and its diseases, Dr. Lewin's book, written for the consumption of the general public, is very necessary. To find such an authoritative, enjoyable, and readable short book as this, about our own medical problems, is an unusual treat in the literature of today. May I suggest it as a must to physicians, medical students, nurses, and to you: the reading public?

Milton C. Cobey, F.A.C.S., M.D.
Washington D. C.

COSMIC DISTURBANCES*

Causes of Catastrophe. L. Don Leet. vii + 232 pp. Illus. $3.50. Whittlesey House. New York. 1948.

Dr. Leet has written an urbane, instructive, entertaining, though somewhat uneven, discussion of some of nature's more violent manifestations. The forms of catastrophe examined are listed in a subtitle as Earthquakes, Volcanoes, Tidal Waves, and Hurricanes. Written for the general reader, the book presents effective descriptions of famous catastrophes; describes most of the important characteristics of earthquakes, volcanoes and their eruptions, tidal waves, and hurricanes; and discusses fundamental principles as well as proximate and ultimate causes.

The treatment of earthquakes is distinctly more comprehensive, more authoritative, and consequently more successful than that of the other topics. This is to be expected, as Dr. Leet's practicing field is seismology, in which he is a recognized leader. Since most catastrophic tidal waves (tsunami) are of seismic origin, it is surprising that the author gives them but a few pages. He fails even to mention an outstanding fact about them—namely, that major displacements of the sea bottom in shallow water can produce great tsunami on near-by coasts but create no destructive waves on distant ones (*p.e.*, the eruption of Krakatoa Volcano in 1883), whereas comparatively minor displacements in deep water can produce cataclysmic tsunami thousands of miles away (*p.e.*, the Aleutian submarine quake and accompanying tsunami of April 1, 1946, which did extensive damage in Hawaii).

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* Published by permission of the director, U. S. Geological Survey.

Dr. Leet does a valuable service in dispelling many myths connected with earthquakes. He effectively closes the "yawning chasms," disgorges the engulfed buildings and livestock, and modifies the "earthquake weather" of the imaginative but uninformed. At the same time, he warns that all the destructive earthquakes are not in the past and pleads for civic "catastrophe planning."

Popular misunderstandings regarding other forms of catastrophe are not as thoroughly resolved. In particular, Dr. Leet would have done well to extinguish the fire that is so persistently and incorrectly connected, in most minds, with volcanoes. The black clouds and fiery glow common to eruptions and the burnt appearance of some volcanic products make it easy to imagine that combustion is important in eruptions. Actually, the clouds are not smoke but rock debris and the glow is from molten, incandescent but not burning, lava. A little true burning doubtless results when certain uncommon volcanic gases, such as hydrogen, meet the air, but combustion definitely has only a bit part in the volcanic drama.

The book is abundantly illustrated, but suffers from poor integration of text and illustrations. All the illustrations lack numbers, and the sixteen pages of half tones even lack page numbers; many illustrations are far from their text references, with few clues to help the laboring reader find them.

In the otherwise well-organized text, the sections headed Modern Trails Across the Pacific (including a long discussion of time zones) and War-Name Islands seem irrelevant.

G. D. Robinson

U. S. Geological Survey
Washington, D. C.

GENIUS DOES WHAT IT MUST

The Gifted Child Grows Up. Lewis M. Terman, Melita H. Oden, and Associates. xiv + 448 pp. $6.00. Stanford Univ. Press. Stanford, Calif. 1947.

WHAT are the physical, mental, and personality traits of intellectually gifted children (i.e., those who score well within the upper 1 percent of American school children on intelligence tests), and what sort of adult does the typical gifted child become? More than a quarter of a century ago Professor Terman inaugurated a systematic study of this problem. The design of the investigation included the identification of some 1,500 children within the I.Q. range 135–200 in the California public-school population, intensive study of the characteristics of the subjects and their families, periodic follow-ups of the subjects through their adult years, and study of their progeny for several generations. This study has become a classic in the field of educational psychology.

The book under review here is the third published report on the gifted group. It is concerned primarily with a portrayal of the subjects twenty to twenty-five years after their original identification in 1921–22. Detailed descriptive data are presented concerning the mortality, physical and mental health, intellectual, educational, occupational, and economic status, avocational and other interests, political and social attitudes, marital status and adjustment, and war records of the subjects. Individual chapters are devoted to analyses of subjects of I.Q. 170 and above, subjects of Jewish descent, and subjects of high and low attainment. The discussion is enriched throughout by excerpts from individual case studies and by the authors' interpretation of the data.

What, specifically, does the study reveal about the gifted subjects who are now approaching middle adulthood? In the main, the findings are consistent with expectations aroused by previous study of the group. In terms of averages, the subjects continue to maintain their high intellectual status, are predominantly college-educated, enter the professional and higher business occupations, and are headed toward the higher income brackets. In physical and mental health, personality characteristics, and marital and sexual adjustments, the subjects tend to be slightly superior to the general population.

Not all the subjects, however, conform to the typical pattern. Some appear to have undergone considerable regression in mental ability; they range in vocational success "all of the way from international eminence to semi-skilled labor"; a number were dropped from college on account of poor scholarship; and a large proportion of the successful college students made mediocre academic records. Thus it is clear, as the authors point out, that "intellect and achievement are far from perfectly correlated." The gifted may fail; a high I.Q. by no means assures successful achievement.

The present reviewer agrees in general with the authors' conclusion that, considering the limited funds available and the condition of psychological methodology at various stages of the research, there is "little to regret" regarding the choice of techniques employed in the investigation. Some critical comment, however, is in order.

On the technical side, it is regrettable that the authors depended upon an exclusively verbal and linguistic test (the Concept Mastery test) to estimate the psychometric intelligence of the subjects. The administration of a more comprehensive test measuring additional mental factors would have allowed a greater measure of confidence in some of the conclusions reached.

On the interpretational side, remarkably little attention is given to the hypothesis that adult attainment may be related to the socioeconomic and educational level of the parents. A suggestion lies in several of the relationships presented that these factors may be crucial determinants of adult achievement.

The study provides an excellent illustration of some of the difficulties inherent in psychological research of this nature. We have here a group for which the primary selective factor was a high I.Q., but which is also characterized by high socioeconomic status, uniformity in state of residence, and predominantly urban residence. Consequently, in the absence of adequate controls, what is attributed to high I.Q. may be due, in part at least, to one or more of the other factors. Further, the very fact of being included in a study produces changes in the behavior of the subjects. In the present instance, membership in Terman's group must surely have affected the level of aspiration of many subjects as well as have provided unusual opportunities for scholarship aid and employment. In the light of these considerations, generalizations based on the present study must be made with considerable caution.

Despite these fundamental limitations, the California study of the gifted is an extremely significant one. Its contributions to psychology have been many, including stimulation of the study of deviates, demonstration of the value of the genetic method, and the development of a number of psychometric devices. Educationally, the study has been of inestimable value, for it has been primarily responsible for bringing American schools to an understanding of the needs of exceptional children.

MARTIN D. JENKINS

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Howard University

PRECIS FOR LAYMEN

Energy Unlimited: The Electron and Atom in Everyday Life. Harry M. Davis. xiv + 273 pp. Illus. $4.00. Murray Hill Books. New York. 1947.

TAKING advantage of his many on-the-scene contacts with scientists and their laboratories, the author describes and interprets many recent discoveries and inventions in nuclear physics and electronics with numerous applications of today and some possible applications of tomorrow. The book may be roughly subdivided as follows: (1) several introductory chapters containing some sketches of scientists and some of the basic ideas of atomic particles and radiation; (2) recent developments in electronics (such as electronic calculator, dielectric heating, electron microscope, radar, and television); (3) spectacular gadgets and discoveries in nuclear physics (such as synchrocyclotron, betatron, linear accelerator, nuclear fission, and isotopes); (4) concluding chapters on present and future sources of energy.

Although necessary technical terms are used, the book is on the whole clearly written, in nontechnical language, and is intended for the better-than-average reader. The facts presented are unusually accurate, and even the theoretical explanations—although frequently anthropomorphic—are only occasionally misleading. However, even to the "alert reader" (the author's words) to whom the book is addressed, the content is by no means light reading. Here and there he must interpret or dismiss statements of the following caliber: "The tube itself acts as a valve that allows energy to be pumped into a resonant 'tank' circuit at the phase of the cycle it is wanted." Fortunately, the chapters are subdivided into relatively short sections so that a busy man may take it in convenient doses. About a hundred photographs, which are collectively placed in three groups, and the subject index are helpful.

The author has made a long stride forward in meeting our urgent need that the public be induced to appreciate some of the recent scientific developments that are already rocking the foundations of our social structure.

KENICHI WATANABE

Department of Physics
Wabash College

SCIENCE AND ART

Music and Medicine. Dorothy M. Schullian and Max Schoen, Eds. x + 499 pp. Illus. $6.50. Schuman. New York. 1948.

THIS volume is a symposium of contributions collected by Dorothy M. Schullian, Curator of Rare Books at the Army Medical Library. It consists of sixteen contributions by recognized authorities in the respective fields covered. Among the titles are Music and Medicine among Primitive Peoples (Radin); Music and Medicine in Classical Antiquity (Meinecke); The Story of Tarantism (Sigerist); A Psychiatrist's Experience with Music as a Therapeutic Agent (Altshuler); Music in Hospitals (van de Wall); Music in Industry (Cardinell); and The Development of an Experimental Psychology of Music (Diserens).

In the chronological order of the development there is a striking decline in the intimacy of medicine and music. In primitive magic, they were practically one, but with the coming of historical and scientific approaches there has been a gradual process of isolation of the two disciplines up to the beginning of this century. Now there is a revival. The book is timely because we are now facing a scientific approach to the relationship.

The topics and authors are well selected, and the volume is well edited. Both specialists and the general reader will find the book scholarly and interesting.

CARL E. SEASHORE

The Graduate College
State University of Iowa

EYEWITNESS ACCOUNT

The American Iliad. The Epic Story of the Civil War as Narrated by Eyewitnesses and Contemporaries. Otto Eisenschiml and Ralph Newman. 720 pp. $5.00. Bobbs-Merrill. Indianapolis and New York. 1947.

DON'T think that all the towns in which they are still fighting the Civil War lie south of the Mason-Dixon line. Chicago plunges anew into that conflict with every meeting of its famed "Civil War Round Table." Dr. Eisenschiml and Mr. Newman are two of the most intrepid, and hence celebrated, members of that group. The former gained the public eye when his first book, *Why Was Lincoln Murdered?* suggested that perhaps Boston Corbett was aiming at the wrong man when he shot J. Wilkes Booth; he might have come closer to the real culprit if he had drawn a bead on Edwin M. Stanton. Mr. Newman is the proprietor of a Chicago bookstore named after Abraham Lincoln and has made a lifework of purveying Civil War books to a public which seems constantly to increase.

When they put this bulky volume together, Dr. Eisenschiml and Mr. Newman were following a simple but sound idea. Why not try to show the war as it looked to those who fought it, or lived through it? They accordingly laid down a pattern which treated each major battle and campaign (*a*) as it seemed to a high-ranking officer on the Federal side; (*b*) as it seemed to a soldier in the ranks on the Federal side; (*c*) as it seemed to a contemporary civilian commentator on the Federal side; (*d*) (*e*) and (*f*) the same process repeated for the Confederate side. The result is a fairly comprehensive, pretty well-balanced, and usually lively outline of the war. It is not the war as Douglas S. Freeman or Lloyd Lewis sees it, and much less as our economic determinist historians of the present see it. Nevertheless, it is the war as its participants knew it. And that is something worth remembering.

The human reactions have not changed essentially in eighty years. The men on the battlefield still show the same symptoms of fear. The men in command still seem to their subordinates to make the same bullheaded and boneheaded blunders. There may have been more confusion among the fighting men during the second world war as to what they were fighting for than there was during the Civil War, but there was plenty of confusion in the earlier instance, as this volume shows. And did the South plunge into the conflict as recklessly, as heedless of the possible cost, as we have been told? Read this from the famous diary of the wife of Colonel James Chesnut, former senator from South Carolina, aide to General Beauregard during the attack on Fort Sumter:

> *April 12, 1861.* I do not pretend to go to sleep. How can I? If Anderson does not accept terms at four, the orders are, he shall be fired upon. I count four. St. Michael's bells chime out and I begin to hope. At half-past four the heavy booming of a cannon. I sprang out of bed, and on my knees prostrate I prayed as I never prayed before.
>
> There was a sound of stir all over the house, pattering of feet in the confusion. All seemed hurrying one way. I put on my double-gown and a shawl and went, too. It was to the housetop. The shells were bursting. In the dark I heard a man say, "Waste of ammunition." I knew my husband was rowing about in a boat somewhere in that dark bay, and that the shells were roofing it over, bursting toward the fort. . . .
>
> The women were wild there on the housetop. Prayers came from them, and imprecations from the men. . . . Pryor, of Virginia, spoke from the piazza of the Charleston Hotel. I asked what he said. An irreverent woman replied: "Oh, they all say the same thing, but he made great play with that long hair of his, which he is always tossing aside!"

The book is full of just such fascinating bits as this, but the total result is not as effective as it might have been. This is principally due, I think, to the fact that most of the commanding generals are quoted, not in the day of battle or immediately after, but in terms of the alibis they had worked out for their own generalship fifteen and twenty years after the war was over. If Eisenschiml and Newman had stuck to the "contemporary" pattern more consistently than they did, they would have produced a better book. Insofar as their pages present actual "eyewitness" testimony, set down before counsel for the defense had a chance to coach the witness, the outcome is fascinating.

PAUL HUTCHINSON
The Christian Century
Chicago, Illinois

COMMENTS AND CRITICISMS

BUNYAN'S BARROWS

It is rather surprising that a biologist would support the idea that a peculiar microrelief, locally called hog wallows, or Mima Mounds, has been created by the pocket gophers. To the best of my knowledge, gophers are not mound builders, and it is hardly conceivable, to say the least, that any animals that do not have a certain instinct would acquire it as soon as they move into an environment unfavorable to their normal behavior.

It is no less surprising that a soil scientist would state that "closer observation reveals currently active ground squirrels that have mounds as large as 40 feet in diameter and 10 feet in height" (SM, January 1948, 175). Ground squirrels occupy many of these mounds, so one may say that they "have" them, but the fact of occupancy does not by any means prove that the squirrels built the mounds. A little closer observation, probably, would suggest that the ground squirrels' work tends to flatten down the mounds rather than to build them up.

There are, perhaps, no less than a dozen different hypotheses about the origin of these mounds, and I cannot resist the temptation to add one more, so far not mentioned in any list. It is not mine. It was related to me some years ago by an old farmer in the San Joaquin Valley, who was much concerned with hog wallows because he had a hard time leveling his land for an orchard. I shall try to present his story as near as it was told to me while he allowed his team of mules to rest. Here is the story:

"Once upon a time a man by the name of Bunyan—Paul Bunyan—lived in this country. Whether it was Paul himself or one of his Western relations—I am not suite sure on this score. Anyhow, I gather that this man was quite a character and had some ideas not to be expected of an ordinary fellow.

"Whether Paul heard something about the Chinese Wall or not, he got his own idea about it and concluded that the United States needed a proper protection so as not to be taken too easily by our enemies. So, when he had thought this over to his satisfaction, Paul hired a bunch of Irishmen and directed them to build a great wall of our own.

"The Irishmen got wheelbarrows and set to work. For awhile, no doubt, they worked pretty hard, so that eventually quite a hole resulted on one side, from which they moved the dirt to the other side. In due time, this hole apparently became flooded almost to overflowing, and this was how the Pacific Ocean was created, notwithstanding other, scientific explanations. And the wall they were building, it's —look back of you—the Sierras. . . . Yes, brother, it's how things like these have developed, as the saying goes.

"Well, the work progressed quite satisfactorily for awhile, and Paul was very much pleased with the creation. Somewhat before its completion, however, the Irishmen began to complain about the victuals that Paul provided for them. It turned out that Paul disagreed with the boys and just paid no attention to their dissatisfaction. So the Irishmen talked it over and decided to go on strike.

"Well, when Irishmen get an idea, they take it seriously. That's where Paul, probably, miscalculated. . . . They turned over their wheelbarrows, dumped the dirt all over the country wherever they happened to be, and went back to Ireland. Now, apparently such a calamity happened years ago, so that nothing is left of the wheelbarrows to the extent that even the nails corroded beyond recognition; but heaps of dirt from each wheelbarrow are still with us. . . . And this is how the hog wallows came to be, brother."

Although quite a few of Paul Bunyan's magnificent deeds are generally known, to the best of my knowledge his sad experience with the Irishmen has not been recorded. So, perhaps, lovers of our folklore will be glad to add this story to their collection. And with regard to science—well, the Paul Bunyan story cannot hurt any that can stand on its own merit.

A skeptical reader will say that Paul Bunyan is a fiction, but, perhaps, so is the mound-building gopher—what is the difference?

C. C. NIKIFOROFF

University Park, Maryland

A GLACIAL REACTION

Because Professor Emeritus W. H. Hobbs so long ago originated the "glacial-anticyclone" theory (about 1911) and since then has had his own facts for the most part interpreted against him, it is uncommonly interesting to find him today charmingly proceeding as if the "glacial anticyclone" were a well-established feature of large ice sheets (SM, February 1948). Most informed opinion and many additional facts have steadily been marshaled in favor of alternative explanations for the nourishment of continental ice sheets. Today's fuller picture is that of storm (cyclonic circulation) nourishment by air masses traveling common tracks and by common orographic processes operating when air masses meet lofty obstructions.

The experience of meteorologists, geologists, and persons with wartime knowledge of Greenland, among others, simply does not bear out the "glacial anticyclone." Anyone interested in a full, cohesive, and lucid account of glacial phenomena might consult the very recent book by Professor Richard Foster Flint, of Yale, (*Glacial Geology and the Pleistocene Epoch*. N. Y.: Wiley. 1947), which cites the literature exhaustively. However, one need not make a mere appeal to authority, for the article itself offers many items for critical review.

For instance, the continental ice sheet is pictured as nourished solely by ice needles from cirri of the central downdraft. Since cirrus clouds exist at altitudes up to 20,000 feet and more and, in the downdraft sketched by Hobbs, must come in from a region of great altitude and great cold where they simply cannot contain much moisture relatively, the mechanism is a dubious one. Singularly, continental ice sheets seem unable to accumulate much beyond 10,000 feet, even though cirrus clouds go much higher; on the other hand, 10,000 feet is approximately the altitude to which air masses on the average can reach and bring nourishment. The mystic 10,000 feet holds for Greenland as an order of magnitude, for Antarctica, and, probably, for the continental ice sheets of the past.

Around Greenland two main storm tracks are found: those coming upon the island from the SE and those from the SW. In either case they bring much moisture. Frequently, they are unable to surmount the ice barrier, but lap onto its flanks, and so are canalized into proceeding along the ice margin. As a result, not only is the greatest nourishment of the Greenland ice sheet in its southerly portion, but there also the ice stands at the highest altitude. Air masses have been so effectively robbed of their moisture by the time they reach northern Greenland that they have little to contribute; probably, even, after the manner that ice disappears from our winter roads though the temperature stays well below freezing, they pick up moisture by sublimation. The end result is that the far northern part of Greenland neither is ice-covered today nor shows any evidence of having ever been glaciated!

Undoubtedly, foehn-type winds may and do exist in Greenland. Dense, cold air equally may and does slide down a Sierra Nevada valley. Such winds are not invariable; they require appropriate conditions. It does not do to attribute to them transcending powers, particularly in the face of a well-established system of world air circulation and in the face of other evidence which the "glacial-anticyclone" concept by no means explains.

SANBORN PARTRIDGE

New Haven, Connecticut

P.S. Since writing the above letter, I have read a pertinent article by François E. Matthes, "The Glacial Anticyclone Theory Examined in the Light of Recent Meteorological Data from Greenland" (Pt. I: *Trans. Amer. Geophys. Union*, 1946, **27**(3), 324–41). Dr. Matthes never raises his voice, quietly cites the evidence, and applies sound, logical reasoning. I think that I have never seen an idea so thoroughly dissected, exposed, and found wanting as the "glacial anticyclone" at his hands.

THE SUBSTITOOTER

You have lately published some first-class advice on writing and public speaking. You may wish to pass on to your readers the following rules on how to give a guest lecture in a colleague's course. In our university we do a good deal of lecturing for each other. It may be that the same is true in other institutions of learning and in laboratories and industries where special courses are given on some new subject or problem that requires the collaboration of people in several specialties. If so, they may wish to profit from our experience.

HOW TO GIVE A GUEST LECTURE IN A COLLEAGUE'S COURSE

1. Assure your listeners that you haven't any idea what the course is about. This will convince them either that the people running the course didn't show you a syllabus, or that you didn't consider it important enough to look at. You will thus impart a proper idea of the respect in which colleagues hold one another, as well as of the nice, offhand lack of planning of courses.

2. Tell them that you really don't know what to say in "these few remarks." Then they will understand your doubts (duly expressed) about why you were asked to give the lecture.

3. When referring to any real or supposed differences of judgment between yourself and your colleagues, repeat frequently the phrase "in my humble opinion." For it is—unlike its mate, "I must confess my ignorance"—a phrase which is, of course, taken more seriously with each repetition. You will thus put any ensuing argument squarely on a personal basis, and you will also set yourself off clearly from your more arrogant colleagues.

4. Never fail to say that you can do nothing with so vast a subject as yours in so short a time. To demonstrate the truth of this, take ten minutes to get it said. Thus demonstrated, the statement will serve several of your dearest aims. It will make the students fully aware of your opinion of their intelligence. It will imply your notion of good pedagogy. It will prove your devotion to the scholar's leisurely disregard of time, especially of students' time.

5. To make sure that all the above sinks in, continue to talk for five minutes after the bell rings.

6. To make assurance doubly sure, bolt from the room before anyone can ask you a question.

I know that these rules are right, for they are followed by most of us on the faculty, and we are famous men. A few persistently disregard them. The results are astounding. The students talk of it for days.

EVERETT C. HUGHES

Division of the Social Sciences
The University of Chicago

Correction. Kurt W. Kohn and Walter J. Schneider, of The Bronx High School of Science, were among the forty prize winners in the Westinghouse Science Talent Search (The Brownstone Tower, May 1948).—Ed.

TECHNOLOGICAL NOTES

Better bone char. Many of the processes of industry are amazingly varied, and unsuspected by most of us. For instance, our 9 billion pounds of cane sugar are refined by a process that includes passing the syrup through immense filters of bone char, equal in weight to half the sugar produced. Of course 4.5 billion pounds of bone char do not exist at the same time; when the char no longer absorbs efficiently the spent material is revived by washing, drying, and heat. New char weighs about 40 pounds per cubic foot and is so porous that 100 pounds will have 2.25 square miles of surface area. In repeated use the pores clog and the char gains in weight. Bureau of Standards studies are pointed toward discovering ways to renew char with the smallest amount of waste—and, more important, to be able to predict the qualities of char as an adsorptive medium.

For enjoyment of salt. People who have dropsy may have to subsist on insipid diets, devoid of salt. One possible form of relief is to give the food savor with a mixture that tastes like salt but is free of sodium. Now the Permutit Company has a new method. During the war emergency kits contained resins that would make sea water potable by the ion-exchange process. The person who cannot stand salt may be able to enjoy his food in the usual way if he will take a generous dose of resin before and after meals.

Hunting iron in Minnesota. As the holes around Hibbing and other parts of Minnesota grow larger the iron ore reserves become smaller. Already there is talk of using leaner ores and of shifting to supplies from other parts of the world. But there may be rich deposits in Minnesota, buried under a thick blanket of glacial drift. The airborne magnetometer is being used in an effort to locate such bonanzas, if they exist. Minnesota and the U. S. Geological Survey are cooperating in this aerial prospecting. The device is flown over a grid course, making a continuous record of variations in the earth's magnetic field. Places where the magnetism is high will receive more detailed exploration on the ground.

No need to crowd. The number of persons who can have ringside seats to witness an operation is extremely limited. Television can increase the range. Northwestern University School of Medicine will make use of television to demonstrate surgical techniques to as many as 1,500 people at a time at the Chicago meeting of the American Medical Association June 21–25.

Heating and eating. Very cold weather may affect a limited food budget adversely. The tendency to eat heartily in cold snaps and to toy with food in warm weather has been confirmed and explained by Yale University researchers. The hypothalamus, heat regulator of the body, also has the say-so in deciding to eat and not to eat and when to quit. Nothing is said in the report about the tendency to take aboard too much food as a substitute for emotional satisfaction.

Appeal for invention. Quartz crystals, mostly imported from Brazil, are exceedingly important as radio frequency controls. A substitute for this purpose or a source of material inside the United States is called for in the *Journal of Economic Geology.*

Let there be light. From Hollywood or near it comes the triumph in application of scientific discoveries to modern gadgetry. Charles Tucker, of Los Angeles, has announced a table lamp that lights up when the word "Bright" is spoken with resonance.

Food from sawdust. When food is scarce the balance between grains used for food and for feed becomes delicate. Human beings have first claim. When that is the case, according to *Industrial and Engineering Chemistry*, sawdust may be made an important part of cattle feed. Chemical modification to remove the lignin and combination of the modified sawdust with sources of vitamins and minerals make a feed that is efficient and that, so far, is not in demand for human consumption. Paper pulp is another possible addition to cattle feed, but the present shortage of paper would seem to make that source of roughage unpromising.

M. W.

THE BROWNSTONE TOWER

DEAR MR. EDITOR-TO-BE:

THIS is my last message from The Brownstone Tower. The time has come for me to leave the SM and turn over its editorial direction to you. Not knowing your name as yet, I do not know how I can best help you, but I do want to encourage you at least.

You will take over a magazine that has been, since 1872, the chief medium of communication between American scientists and the educated public. With the increase of specialization in science, the importance of the SM has grown as a means for broadening the scientific outlook of scientists. The busy specialist who takes the SM can emerge from his diggings once a month for a view through its pages of the scientific world outside his own mine. You may feel that the view he has been getting is imperfect, and you will probably want to make it more comprehensive, understandable, interesting, or authoritative. You would not be a good editor if you did not see the need and opportunity for improvement.

You should not worry too much about format; it is the subject matter of the magazine that will make or break it. You will be dependent on manuscripts contributed by scientists. Without solicitation you will get more than enough manuscripts to fill the SM, but for various reasons related to your policy you will not publish a large proportion of those you receive. You will quickly find that solicitation of manuscripts is essential for the improvement of the contents of the magazine, and you will be successful to the extent that you can persuade busy scientists of reputation to write for the SM in an interesting, nontechnical fashion. You must become a salesman with the objective of convincing scientists who have something to say to a general audience that they should take the time and trouble to write it for the SM. You will have arrived when any scientist will give top priority to your invitation.

If you have not been a professional scientist, you may wonder with what manner of men you must deal, but you will soon discover that they are both human and humane. Upon request they will draw freely upon their time to give you advice, and your letters of solicitation will almost always bring prompt, courteous, and sympathetic replies. You will find that most scientists will want to write for the SM, and you will get many promises, some unconditional, and others carrying-various reservations. But remember that a manuscript in hand is worth a flock of promises. Unfortunately, scientists are often overoptimistic about their capacity to perform, and deadlines seem not to mean as much to them as they do to you. When you fail to get a manuscript at the predicted time—and that is the rule, not the exception—you should write repeatedly at reasonable intervals and make personal contacts if you can. Courteous patience will win, though it may take years to get a manuscript you want, for, as one potential contributor expressed it, "I'm sure that next time it will be easier to do the article than explain why I can't."

You will have little trouble with the writers of accepted manuscripts. Most will be grateful for your editorial suggestions and for careful copy editing and checking of facts. It is not easy to maintain a nice balance between your responsibilities to your contributors and those to your readers, but I think the readers must always be given the benefit of a doubt. Samples of readers might be polled now and then to find out how you are doing and you might modify your policies accordingly. Caustic comments on the articles you publish will be sent to you, whereas most bouquets will be sent directly to the authors. Your critics will not hurt you, but will merely spur you on to greater efforts. Your greatest satisfaction may come from the friendly contacts you will establish with some contributors and readers.

Whatever the future brings, hold to the principle that the A.A.A.S. must provide, through the SM or otherwise, a means of broadening communication among scientists in different fields. Go ahead, then, with confidence and enthusiasm!

Sincerely yours,
F. L. CAMPBELL

INDEX

CONTRIBUTORS

Principal Articles

Bakke, E. Wight: Teamwork in Industry, 213.
Black, Max: Principles of Really Sound Thinking, 232.
Blakeslee, Albert F.: Edmund Ware Sinnott, 5; Impressions of India, 141.
Brand, Charles J.: The National Fertilizer Association, 33.

Cameron, Stella B.: Moses in the Laboratory, 488.

Davis, H. M.: Liberal Education and the Physical Sciences, 421.
Davis, W. A., and Havighurst, R. J.: The Measurement of Mental Systems, 301.
Dragstedt, Carl A.: Intravenous Injections, 25.

Ellis, Albert: The Application of Scientific Principles to Scientific Publications, 427.
Ensign, George G.: Time, 206.
Eriksson, Herman: The Nobel Prizes, 363.
Essig, E. O.: The *Ruppia* Balls of Little Borax Lake, 467.
Evans, L. H.: The Library of Congress as the National Library of Science, 405.

Ferguson, Ira L.: Contact Lenses, 491.
Fish, F. F.: The Return of Blueback Salmon to the Columbia River, 283.

Harding, Harold F.: The Principles of Poor Speaking, 54.
Harrington, E. R.: Black Butte, a Recent Subsidence Crater, 461.
Havighurst, R. J.: *See* Davis, W. A.
Henry, Jules: Cultural Discontinuity and the Shadow of the Past, 248.
Hirsh, Joseph: *De Spiriti*, or History Is Right, 402.
Hobbs, William Herbert: American and Eurasian Glaciers of the Past: A Picture Based on Existing Ones, 99.
Hodge, W. H.: Agricultural Education in Colombia, 91.
Hutchens, John O.: The Tox Lab, 107.

Jenkins, M. D.: The Upper Limit of Ability among American Negroes, 399.
Jenssen, Ward: *See* Seashore, Robert H.

Keller, W. D.: Native Rocks and Minerals as Fertilizers, 122.
Kellogg, Charles E.: Conflicting Doctrines About Soils, 475.
Koons, Frederick C.: The Sand Mounds of Louisiana and Texas, 297.
Krogman, Wilton Marion: Physical Anthropology and Race Relations: A Biosocial Evaluation, 317.

Lacy, Dan Mabry: International Cultural Cooperation, 418.
Lapham, Macy H.: The Desert Storehouse, 451.
Lewis, Oscar: Rural Cross Section, 327.
Lindsey, Arthur Ward: The Faith of Science, 395.

McDonald, Thomas H.: Improved Methods of Transport and Their Significance, 57.
McIntire, Ross T.: For Now and the Future, 135.
Malamud, Daniel: The Counselor Says "M-hm," 145.
Manley, J. H.: One Atom and Many, 47.
Mayall, N. U.: Nature's Greatest Explosions: Supernovae, 17.
Miller, Benjamin F.: Group Medical Practice, 389.
Milne, Lorus J., and Milne, Margery J.: The Life of the Water Film, 113.

Oehser, Paul H.: George Brown Goode (1851–1896), 195.

Palmer, Archie M.: Patents and University Research, 149.
Pearson, Oliver P.: Metabolism and Bioenergetics, 131.
Penrose, Edith Tilton: A Great African Project, 322.
Perry, J. W.: The Utilization of Scientific Knowledge, 413.
Péwé, Troy L.: Origin of the Mima Mounds, 293.

Rader, Melvin: Technology and Community: The Mandates of Survival, 502.
Riker, A. J., *et al.*: Advice to Newcomers: A Report on Releasing the Results of Research to the Public, 499.

Seashore, Robert H., and Jenssen, W.: Personality Classification and Counseling Techniques, 472.
Skutch, Alexander F.: Life's Greatest Evil, 514.
Stuart, Neil W.: Growing Plants Without Soil, 273.
Subarsky, Zachariah: What is Science Talent? 377.
Suter, Rufus: Salomon's House: A Study of Francis Bacon, 62.

Visher, Stephen S.: Thunderstorms, 335.

Wedderburn, Alexander J.: Photography in Science, 9.
White, Leslie A.: Man's Control over Civilization: An Anthropocentric Illusion, 235.
Wilkinson, Ford L., Jr.: The U. S. Naval Postgraduate School, 183.
Wolf, Ralph F.: Eighty-eight Years of Synthetic Rubber, 221.

Science on the March

Beams, J. W.: Centrifugal Fields, 255.

Chester, K. Starr: Plant Protection Mutates, 157.

Huffman, C. F.: The Role of the Rumen in the Nutrition of the Ruminant, 342.

Luria, S. E.: Sex in Bacteria and Viruses, 159.

Ochse, J. J.: Experience Is Their Laboratory, 70.
Olive, Lindsay S.: The Nuclear Cycle in the Fungi, 444.

Richter, Charles F.: International Recovery in Seismology, 67.
Russell, John A.: Meteorites: Comet Components, 519.

Book Reviews

(Indexed By Reviewers)

Aginsky, Ethel G.: *The Indians of the Americas,* by John Collier, 351.
Atwood, Wallace W.: *Outlines of the Geography, Life and Customs of Newfoundland-Labrador,* by V. Tanner, 259.

Ball, Carleton R.: *Two Blades of Grass,* by T. Swann Harding, 75.
Bell, E. T.: *Whom the Gods Love. The Story of Evariste Galois,* by Leopold Infeld, 431.
Berlin, Richard B.: *Duncan of Liverpool,* by W. M. Frazer, 171.
Berry, Charles T.: *A Field Guide to the Shells of our Atlantic Coast,* by P. A. Morris, 262.
Buss, Frances: *Television Techniques,* by Hoyland Bettinger, 163.
Byerly, Perry: *When the Earth Quakes,* by James B. Macelwane, 165.

Carlson, A. J.: *What Is Life?,* by J. B. S. Haldane, 442; *Education for What is Real,* by Earl C. Kelley, 442.
Cobey, Milton C.: *The Back and its Disorders,* by Philip Lewin, 526.
Cofer, Charles N.: *Hypnotism Today,* by Leslie M. LeCron and Jean Bordeaux, 168; *Current Trends in Psychology,* by Wayne Dennis *et al.,* 265; *Conceptions of Modern Psychiatry,* by Harry Stack Sullivan, 352.
Collins, Henry B., Jr.: *Bridge to Russia,* by Murray C. Morgan, 352.
Crawford, W. Rex: *The Theory of Social and Economic Organization,* by Max Weber, 163.

Dale, Edgar: *From Caligari to Hitler,* by Siegfried Kracauer, 80.
Deichmann, Elisabeth: *Naturforskeren Johannes Schmidt.* Øjvind Winge and A. Vedel Tåning, Eds., 73.
DuBois, W. E. B.: *Ideas Have Consequences,* by Richard M. Weaver, 432.

Farnsworth, Alice H.: *The Naming of the Telescope,* by Edward Rosen, 264.
Fenton, William N.: *Chippewa Village,* by W. Vernon Kinietz, 263.
Fischer, Eric: *The Geographical Names of Antarctica.* U. S. Board on Geographical Names, Dept. of the Interior, 170.
Freeman, Ira M.: *The Scientists Speak.* Warren Weaver, Ed., 350.

Gardner, Thomas S.: *Rockets and Space Travel,* by Willy Ley, 78.
Grant, Chapman: *Amphibians and Reptiles of the Pacific States,* by Gayle Pickwell, 166.
Grauer, Ben: *Maya Explorer,* by Victor Wolfgang von Hagen, 435.

Hannaford, Earle S.: *Surveying Instruments,* by Edmond R. Kiely, 347.
Hart, Deryl: *George Crile, An Autobiography.* Grace Crile, Ed., 524.
Hawley, J. H.: *Alexander Dallas Bache,* by Merle M. Odgers, 438.
Hayes, H. K.: *The Hybrid-Corn Makers,* by A. Richard Crabb, 438.
Hedgpeth, J. W.: *English Naturalists from Neckam to Ray,* by Charles E. Raven, 353.
Hendricks, B. Clifford: *Alsos,* by Samuel A. Goudsmit, 346.
Hutchinson, Paul: *The American Iliad,* by Otto Eisenschiml and Ralph G. Newman, 529.

Infeld, Leopold: *One Two Three . . . Infinity,* by George Gamow, 440.

Jenkins, Martin D.: *The Gifted Child Grows Up,* by Lewis M. Terman and Melita H. Oden, 527.

Kennedy, John L.: *The Reach of the Mind,* by J. B. Rhine, 167.
Krogman, W. M.: *Men Out of Asia,* by Harold Sterling Gladwin, 437.

Lasky, Jesse L.: *Magic Shadows,* by Martin Quigley, Jr., 431.
Lehner, George F. J.: *Personality,* by Gardner Murphy, 168; *400 Years of a Doctor's Life,* by George Rosen and Beate Caspari-Rosen, 442.
Lindsay, J. R.: *Hearing and Deafness.* Hallowell Davis, Ed., 355.
Lindstrom, Ernest W.: *Genetics, Medicine, and Man,* by H. J. Muller, C. C. Little, and Laurence H. Snyder, 78.
Linton, Ralph: *Family and Civilization,* by Carle C. Zimmerman, 439.

McHenry, Donald Edward: *East of the Andes and West of Nowhere,* by Nancy Bell Bates, 266.
McMillen, Robert D.: *Cache Lake Country,* by John J. Rowlands, 440.
Mann, William M.: *A Multitude of Living Things,* by Lorus J. Milne and Margery J. Milne, 77; *One Day at Teton Marsh,* by Sally Carrighar, 263.
Mason, Howard S.: *Science in Progress.* George A. Baitsell, Ed., 169.
Merrill, Paul W.: *John Couch Adams and the discovery of Neptune,* by Sir Harold Spencer Jones, 77; *The Life and Times of Tycho Brahe,* by John Allyne Gade, 436.

Metzner, Charles A.: *Say It with Figures,* by Hans Zeisel, 76.
Miller, Benjamin F.: *Private Enterprise or Government in Medicine,* by Louis Hopewell Bauer, 523.
Milne, Lorus J., and Milne, Margery J.: *Insects and Human Welfare,* by Charles T. Brues, 441.
Myers, W. M.: *A Scientist with Perry in Japan.* Allan B. Cole, Ed., 433.

Nicholson, Seth B.: *Sunspots in Action,* by Harlan True Stetson, 265.

Oehser, Paul H.: *Louis Agassiz, Scientist and Teacher,* by James David Teller, 350; *Breaking New Ground,* by Gifford Pinchot, 443.
Old, Bruce S.: *Combat Scientists,* by Lincoln R. Thiesmeyer and John E. Burchard, 348.

Page, Robert M.: *New Weapons for Air Warfare.* Joseph C. Boyce, Ed., 345.

Rhine, J. B.: *Telepathy and Medical Psychology,* by Jan Ehrenwald, 525.
Robinson, G. D.: *Causes of Catastrophe,* by L. Don Leet, 526.

Seashore, Carl E.: *Music and Medicine.* Dorothy M. Schullian and Max Schoen, Eds., 529.
Stieglitz, Edward J.: *The Years After Fifty,* by Wingate Johnson, 264; *Medical Aspects of Growing Old,* by A. T. Todd, 264.
Sumner, F. C.: *Doctor Freud,* by Emil Ludwig, 436.

Thompson, Richard K., Jr.: *Your Teeth and How to Keep Them,* by Jerome J. Miller, 525.

Vargas, Oscar: *English-Spanish Chemical and Medical Dictionary,* by Morris Goldberg, 167.
Vold, Robert D.: *Small Wonder: The Story of Colloids,* by Gessner G. Hawley, 348.

Watanabe, Kinichi: *Energy Unlimited,* by Harry M. Davis, 528.
Weber, Robert L.: *What Electronics Does,* by Vin Zeluff and John Markus, 523.
Wekstein, Louis: *In Search of Beauty in Music,* by Carl E. Seashore, 74.
Whaley, W. Gordon: *Our Flowering World,* by Rutherford Platt, 353.
Wilcox, E. V.: *The Story of Water Supply,* by F. W. Robins, 79; *Science in Farming.* Alfred Stefferud, Ed., 262; *Fish Ponds for the Farm,* by Frank C. Edminster, 354; *Liberia,* by Charles Morrow Wilson, 434.
Woolfolk, E. J.: *Birds of Malaysia,* by Jean Delacour, 171.

Verse

Bolles, Laurens C.: Lines to a Skyscraper, 460.

Ericsson, Emily: Two Squares of Glass, 417.

Goff, Eris: Where Silence Sings, 321; The Aurora, 466.
Goodman, Mae Winkler: Texture of Snow, 56; Arctic Tern, 234; Telescope on Mount Palomar, 376.

Hirsh, Joseph: The Weatherman, 282; Twisted Trees, 498.

Johnson, Paul B.: The Roads, 140.

Longfellow, Isabelle Bryans: The Gray Edge, 398; Weather Is Like Wild Horses, 471.

Miller, J. S.: On the Death of a Willow in the Hurricane, 341.
Monahan, Thomas P.: For We Are One, 487.

O'Brien, Katharine: Well of Desolation, 247.

Sinclair, John G.: Ameba, 334.

Zahniser, Howard: America Grows Corn, 383.

Comments and Criticisms

Abbot, Charles G.: Re Rhine, 358.
Arkley, Rodney J.: The Mima Mounds, 175.

Cahn, Herbert: Last Word On Garrett, 447.
Campbell, F. L.: The Brownstone Tower, 90, 179, 272, 361, 450, 534.

Davidson, Harold B.: Vital Statistics, 358.
Dorchester, Frank E.: Shakespeare's Physiology, 270.
Dudley, J. Whitney: San Joaquin Valley Hog Wallows, 356.

Grant, Chapman: Crumbling Rocks, 88.

Harwood, Paul D.: Honeybees and Red Clover, 177.
Hughes, Everett C.: The Substitooter, 532.
Hughes, Helen MacGill: National Dog Week, 173.

Kalichevsky, V. A.: More on Man, the Myth-Maker, 357.
Kelly, Allan O.: Crumbling Rocks, 88; The Mima Mounds, 174.

Levine, Norman D.: Viviparity, 358.
Lord, Lindsay: Modern Housing, 176.
Lyford, C. Allan: To Ajax, 357; One Man's Meat, 447.
Lyman, John: The Sea's Phantom Bottom, 87.

Miller, E. C. L.: Man, the Myth-Maker, 83.
Montagu, M. F. Ashley: Racial Intelligence, 81.

Newberry, William: Natural Resources, 176.
Nikiforoff, C. C.: Bunyan's Barrows, 531.

Okun, Daniel A.: Racial Intelligence, 173.

Partridge, Sanborn: A Glacial Reaction, 531.
Pasamanick, Benj.: Racial Intelligence, 82.
Preston, F. W.: Bridgman and Cohen, 83.

Reichard, J. D.: The Mudfog Association, 174.

Samachson, Joseph: Racial Intelligence, 173.
Spillman, Ramsay: On Comments and Criticisms, 267.
Stallings, J. H.: The Mima Mounds, 269.
Stearn, John T.: Einstein's Error? 85.
Steinmetz, Harry C.: Obfuscating Personality, 87.
Steinour, Harold H.: Clotho, Lachesis, and Atropos, 447.

Taylor, Wm. Palmer: Scientists and Social Problems, 267.
Todd, Arthur J.: Man, the Myth-Maker, 82.

Weed, Merrill: Technological Notes, 89, 271, 359, 499, 533.

SUBJECT INDEX

Principal Articles

Agriculture (W. H. Hodge), 91; (N. W. Stuart), 273; (E. T. Penrose), 322.
Alcoholism (J. Hirsh), 402.
Anthropology (W. M. Krogman), 317.
Atom bomb (J. H. Manley), 47.

Bacon, Francis (R. Suter), 62.
Bell County, Texas (O. Lewis), 327.
Bibliographic citations, form of (A. Ellis), 427.
Bioenergetics (O. P. Pearson), 131.
Blood transfusions (R. T. McIntire), 135.

Chemistry (J. O. Hutchens), 107.

Desert flora (M. H. Lapham), 451.
Drugs (C. A. Dragstedt), 25.

Fertilizers (C. J. Brand), 33; (W. D. Keller), 122; (C. E. Kellogg), 475.
Fish and Wildlife Service (F. F. Fish), 283.

Geology (E. R. Harrington), 461.
Glaciers (W. H. Hobbs), 99.
Group medicine (B. F. Miller), 389.

History of science (C. A. Dragstedt), 25; (J. H. Manley), 47; (P. H. Oehser), 195; (R. F. Wolf), 221; (N. W. Stuart), 273.
Hydroponics (N. W. Stuart), 273.

India, science in (A. F. Blakeslee), 141.

Labor relations (E. W. Bakke), 213.

Mental tests (W. A. Davis and R. J. Havighurst), 301; (M. D. Jenkins), 399.
Mima Mounds (T. L. Péwé), 293; (F. C. Koons), 297.

National Library of Science (L. H. Evans), 405.
Negro intelligence (M. D. Jenkins), 399.
Nobel prizes (H. Eriksson), 363.
Nuclear physics (J. H. Manley), 47.

Ophthalmology (L. L. Ferguson), 491.

Patents (A. M. Palmer), 149.
Peanuts (E. T. Penrose), 322.
Philosophy (M. Rader), 502; (A. F. Skutch), 514.
Photography in Science (A. J. Wedderburn), 9; Second Salon, 180.
Postgraduate School, Navy (F. L. Wilkinson, Jr.), 183.
Problem solving (W. A. Davis and R. J. Havinghurst), 301; (R. H. Seashore and W. Jensen), 472.
Psychiatry (D. Malamud), 145.
Public speaking (H. F. Harding), 54.
Punched-Card Committee, A.C.S. (J. W. Perry), 413.

Race relations (W. M. Krogman), 317.
Reading list, scientific (H. M. Davis), 421.
Red Cross (R. T. McIntire), 135.
Rubber, synthetic (R. F. Wolf), 221.
Ruppia Maritima L. (E. O. Essig), 467.

Salmon fisheries (F. F. Fish), 283.
Science and religion (A. W. Lindsey), 395.
Science talent (Z. Subarsky), 377.
Sinnott, Edmund Ware (A. F. Blakeslee), 5.
Smith-Mundt Act (D. M. Lacy), 418.
Smithsonian Institution (P. H. Oehser), 195.
Sociology (L. A. White), 235; (J. Henry), 248; (O. Lewis), 327.
Soilless plant culture (N. W. Stuart), 273.
Soil science (C. E. Kellogg), 475.
Sound thinking (M. Black), 232.
Supernovae (N. U. Mayall), 17.

Toxicity Laboratory, University of Chicago (J. O. Hutchens), 107.
Transportation (T. H. Macdonald), 57.

U. S. Book Exchange (D. M. Lacy), 418.

Weather (S. S. Visher), 335.

Xenopus laevis (S. B. Cameron), 488.

Zoology (L. J. and M. J. Milne), 113.

Science on the March

Bacteria and viruses, genetics of (S. E. Luria), 159.

Centrifugation, high-speed (J. W. Beams), 255.
Cytology (L. S. Olive), 444.

Fungicides (K. S. Chester), 157.

Meteorites (J. A. Russell), 519.

Nutrition of ruminants (C. F. Huffman), 342.

Seismology (C. F. Richter), 67.

Tropical flora (J. J. Ochse), 70.

I. A. R. I. 75.

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